

## ***Interactive comment on “Large-scale lysimeter site St. Arnold, Germany: analysis of 40 years of precipitation, leachate and evapotranspiration” by N. Harsch et al.***

### **Anonymous Referee #2**

Received and published: 4 November 2008

#### General comments

The Authors provide a detailed analysis of the long lysimetrical-meteorological data series collected on the largescale lysimeter site “St. Arnold”, Germany, (1965–2007). They present “the water balance, the comparison of the differently planted basins, data quality assessment and the identification of trends over the 40 years of uninterrupted measurement”. The dataset is very interesting because it allows to analyse the long term water balance of three different vegetated soils (grassland in comparison to deciduous and coniferous forests). Many variables are collected and recorded: precipitation,

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humidity, radiation, air temperature, sunshine duration, wind speed and leachate.

The Authors describe each series assessing its quality and its evolution since 1965. Then other water balance components, such as potential and actual evapotranspiration, are estimated and the water balance for the three vegetated lysimeters is computed and compared. Results agree with the previous studies carried out over other German sites and this proves the reliability and the consistency of the dataset. Moreover the Authors analyse the trends in the series and they distinguish the trends caused by climatic forcing from the trends due to vegetation evolution and growth.

The title clearly reflects the contents of the manuscript and the abstract provides a good description of aim of study, the available dataset, the analyses made and the main results achieved.

The introduction well focuses on the investigated problems, well describes previous studies on the same dataset and the aim of the manuscript. Anyway in this part of the manuscript, the Authors should point out if other similar studies have been carried out on other sites and what are the new analyses, methodologies or new data introduced.

The “data sets and method” chapter well describes the long series available from St. Arnold site but it already contains results about trends, even before the evapotranspiration estimation method is presented. The Authors should better describe the analyses made and the applied methodologies: for example no description about trend assessment techniques is given and the water balance formula is put in the following chapter.

In the “results and discussion” chapter the water balance of the three vegetated soils, the trends, the weaknesses of data are explained in details. The Authors give a very complete description of the balance components, their evolution in time and their possible causes of distortion, such as the shelter provided by the tree populations, which affects the grassland leachate rates and the actual evapotranspiration.

The results are compliant with other previous studies, the Authors should emphasise

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what is the newer contribution from St. Arnold and its importance.

Conclusions do not contain what is stated in the abstract as the main results of the manuscript: the long term average annual water balance of the three vegetated soils. Anyway there is a good description of the seasonal water balances, the importance of the large scale lysimeter as a means to “transfer small scale results onto a regional basis” is stated (maybe this should be said in the introduction) and hypothesises on the possible future effects of the trends (vegetation degradation, soil dehydration and increase of intense rain events) are made.

I would recommend the editor to ask for a major revision of the manuscript before its acceptance for publication.

#### Specific comments

From a technical point of view no information is given about the significance of trends. What are the p-values? When do the trends start? The Authors should give more statistical information about trend analysis and make a sensitivity analysis of trends to clearly identify which mostly affects the water balance.

In the abstract the Authors declare that the main result of the study is that, “on a long-term average, the grassland basin turns more than half (53%) of its annually incoming precipitation into leachate and only 36% into water vapour, while the deciduous forest exhibits a rather balanced ratio with 37% for leachate and 44% for evapotranspiration, and the evergreen coniferous forest shows the highest evaporation rate (56%) and the lowest leachate rate (28%)”. Since the averages are made on the whole dataset and since the two tree forests are characterized by a growing period until 1990 (pag 2633 lines 1-9), the percentage values are not representative either for the growing period or for the equilibrium. The Authors should divide the whole dataset period into different intervals and make the water balance on homogeneous periods.

At pag 2632 line 3 the “version of Baumgartner and Liebscher (1990)” was adopted

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for actual evapotranspiration estimation. Why has it been preferred to Hackel (1993)? What are the consequences?

At Pag 2634 a weak correlation between precipitation and AET is stated. Authors should give proof of this weak correlation using some correlation coefficient and showing its values.

Technical corrections

At Pag 2628 line 14 it is said : “While the former average out at 3.0 m/s, the latter average out at 1.0 m/s”. In figure 3 the actual values of wind speed seem much lower. The wind speed at 9m never reaches 2.5m/s (average declared at 3m/s) and the speed at 3m starts from 1.5m/s and after 1976 it is always lower than 1/ms (indicated as the average value).

The sentence at line 13-14 pag 2633 “*by both the forests on and those around the lysimeter site*” is not clear.

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