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Discussions

Interactive comment on "Identifying hydrological responses of micro-catchments under contrasting land use in the Brazilian Cerrado" *by* R. L. B. Nobrega et al.

C. Stamm (Editor)

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HESSD-Manuscript "Identifying hydrological responses of micro-catchments under contrasting land use in the Brazilian Cerrado".

In addition to what has been already commented by referees, I would like to add some remarks as editor handling this manuscript. There is one main issue I would like to point out first. Subsequently, I list a few minor aspects.

Understanding baseflow generation: As clearly demonstrated in the manuscript, baseflow is the dominant flow component. W. Dawes has already pointed out that the manuscript puts too much emphasis on (local) soil physical properties for explaining the observed hydrological differences between the catchments. Information on properties and the (soil moisture) state in deeper parts of the underground is lacking. However, it is well-known that the lower boundary plays a crucial role in how a catchments responds hydrologically (see for example Boorman et al., 1995).
In this context, it is important to also consider the question about the actual catchment boundaries, which are relevant for the measured stream flow. Given the flat topography (quite pronounced for the cropped catchment) and the rather short stretch of open water course in each catchment (see Fig. 1), one has to ask where the water infiltrating into the soil will ultimately flow to. Surface topography is not necessarily a good proxy for delineating the subsurface catchment boundaries nor is it evident that all water should reach the stream network upstream of the measuring station. It might well be that the streams just intercept the most shallow parts of the groundwater flux and the remaining water that is not accounted for in the water balance leaves the catchments as deeper groundwater. How much discharge is measured may hence simply reflect the local conditions that determine how much groundwater is taped by the stream channel. Therefore, unless there are actual measurements in the saturated zone (like piezometric data on water table depths and gradients) that show where the water is flowing and what the dynamics are, the hydrology of these catchments cannot be reasonably understood. The authors have drawn similar conclusions (see e.g. p. 9938, L. 25 - 27), but I think it needs to be emphasized more throughout the manuscript.
In addition to an improved data basis on the compartment that governs the hydrology in these catchments (the deeper subsurface), a more detailed analysis of the available time-series could yield valuable insight. As mentioned already in the first review, the application of a simple water-balance model could be a useful step to learn from the observed discharge dynamics. The pasture catchments for example, reveals quite a strange behavior during the dry period: There a kind of a constant flow level while the

other catchments show an expected decrease over time. However, for analyzing the data in such a way one needs to explicitly deal with the pronounced data gaps in the data series. Unfortunately, this issue is not discussed at all in the current manuscript. Please show how the data have been treated to carry out the hydrograph analysis described in section 2.2.6 and any quantitative evaluation of the data (including water balances).

Detailed comments:

p. 9917, L. 12: What kind of models? Please specify.

p. 9918, L. 15: How is the water balance changed?

p. 9919, L.15: How do these outcomes differ? What is the relevance for your study? Be more precise and specific.

p. 9920, L. 21: What do you mean by deterioration of soil properties?

p. 9923, L. 14: Which algorithm has been used for calculating the wetness index?

p. 9926, L. 3: How representative was this transect? Please show the transects in Fig. 2 or Fig. 4.

p. 9926, L. 21: Are these transects related to the transect used for soil sampling? Please show the transects in Fig. 2 or Fig. 4, too.

p. 9932, sec. 3.4: These results are not linked to the transects described in the method section. Is there any relationship between TWI and soil moisture dynamics?

p. 9932, L. 24: You have no measurements on matric potential (which is a pity). How can you make a statement on reaching the wilting point?

p. 9937, L. 15: An excess of water in the water balance cannot be explained by water storage capacity. The apparent excess of 500 - 600 mm yr^{-1} would otherwise translate into a water table that rises at about 1 - 2 m yr^{-1}.

p. 9938, L. 2: Why should soil compaction contribute to an increase in flow in these catchments? Discharge is dominated by baseflow - why should that increase because of less e.g., macropores?

p. 9938, L. 15 - 21: This paragraph seems contradictory to me. The first sentence states that the cerrado area has the lowest recharge but the last phrase claims that this area has the highest infiltration rate. How do these two aspects go together?

p. 9940, L. 18 - 22: I do not agree with the argument on water storage (see also above).

Christian Stamm

References:

Boorman, D.B., J.M. Hollis, and A. Lilly. 1995. Hydrology of soil types: a hydrologically-based classification of the soils of the United Kingdom. Institute of Hydrology, Wallingford, Soil Survey & Land Research Centre, Silsoe, Macaulay Land Use Research Institute, Aberdeen.