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Hydrology and Earth System Sciences Discussions

Interactive comment on "Subsurface lateral flow from hillslope and its contribution to nitrate loading in the streams during typical storm events in an agricultural catchment" *by* J. Tang et al.

Anonymous Referee #2

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General comment

This manuscript focuses on the assessment of subsurface lateral flow generation at the hillslope scale in agricultural environment and on its importance in transferring nitrate load to the stream. The topic is of great interest in catchment hydrology since the scientific debate over the influence of subsurface flow on the total runoff and transport of contaminants in many natural, agricultural and urban watersheds is still open. The paper benefits from a good dataset, spanning over more than one year of observations and including hydrometric and tracer measurements. The paper outline is logically

structured and, generally, results are graphically well presented. The findings are well projected against previous studies available in the literature. Regrettably, the English is quite poor and this makes the paper hard to read, severely hampering a correct understanding of the topics presented. Readability is also compromised by the use of many acronyms, symbols, chemical formulas, numbers, percentages, measurement units which may lead the reader to lose the thread and be diverted from the most important information. Therefore, I strongly suggest a correction by a native speaker and a revision of the manuscript aimed to present it in a more concise way. Secondly, I suggest to further describe the observed behaviour of transient subsurface flow generation and the contribution of different water sources in terms of old water/new water runoff. Particularly, the role of "new" water (page 4164) and the processes of displacement of nutrient enriched "old" water (page 4172), along with a hypothesis about the mixing of different water sources based on the availability of electrical conductivity data, might be analyzed and presented more organically in the manuscript.

Specific comments

-Abstract. The abstract is quite confused and does not clearly convey the main results of the investigation. Besides the bad English, it lacks a short mention to the study area (the typical environment), to methodology ("stream and hillslope hydrology" are somehow vague) and does not strictly focus on the essential results. Moreover, for the sake of clarity, in the abstract I would report the complete name of chemicals and avoid symbols and acronyms. Note that positive soil water potential was also recorded, even if for a limited period of time, at 0-40 cm depth (see Fig.2b, lower panel and Fig. 3b, lower panel).

-Page 4156, lines 10-22. I would use bullets to list in a clear way general and specific objectives, paying attention to distinguish between actual scientific aims and methods (simultaneous monitor of soil water potential might be seen as method and not as an objective).

-Page 4157, lines 13-14. How was the potential evapotranspiration computed? Please, explain.

-Page 4158, lines 13-14. How were the soil hydraulic properties determined? By laboratory analysis of samples taken in the field or by tabulated values reported in the cited literature? This is crucial since the soil properties are advocated later in the manuscript when describing the generation of hillslope and catchment runoff.

-Page 4159, lines 13-17. "The well water and spring water represented the subsurface soil water. Although the sites for soil water sampling were not located within the peanut hillslope, we assumed that soil water chemistry was similar under the same soil condition and cropping system." This assumption is quite important since it can affect the interpretation of the final results. Do the authors have some practical indications and data or reference that can support such an assumption?

-Page 4160, line 23. The Least Significant Difference test is used to compare means after an ANOVA F-test has been applied and the null hypothesis of equal means has been rejected. Since no results of ANOVA are presented in the manuscript, the reader assumes that all F-test have been performed, yielding indications to reject the null hypothesis. I think that a short description of the statistical analysis performed should be included in the M&M section.

-Page 4161, lines 1-14. Basically, does this allow for a three-component hydrograph separation?

-Page 4161, line 13. Is H+ equal to pH? If so, please be consistent throughout the manuscript, if not, please explain.

-Page 4163, line 5. "compared with the rainfall water, overland ïňĆow increased pH and decreased EC". If I understand well, this is quite surprising to me. I would expect that EC is higher in overland flow compared to rainfall, due to the interaction (even if limited both in space and in time) with the soil. Can the authors explain such a behavior?

-Page 4163, line 28. Please note that the soil water potential is also positive, even for a limited period of time, at 0-40 and 0-60 cm depth!

-Page 4164, lines 5-6 and further in the manuscript. "perched soil water". This sounds an odd concept to me. Do the authors mean perched water table?

-Page 4164, lines 18-19. It would improve readability to put in respective order the reported flood magnitudes at the different gauging stations.

-Page 4164, line 23. What do the author mean by "reversed" flow recession? Do they mean a trend opposite to the flow one?

-Page 4165, lines 16-19. When presenting linear correlations, a value of determination coefficient (R2) would be also useful.

-Page 4166, line 1. I expect that the EC increase is related to the subsurface flow contribution. Is that so?

-Page 4167, lines 20-23. I would report this lack of influence of land use on saturation dynamics also in the abstract and in the conclusions.

-Page 4170, lines 11-14. "The hydrographs and the chemographs of particulate N and P and suspended sediment were similar during the two storms (Figs. 4 and 5), showing increased concentrations with increased rainfall intensity, with the peak concentration before the peak ïňĆow and rapid decreases after the end of rainfall." This is not always true. For instance, look at TN and PN in Fig. 4 (station 4), which peak after the peak discharge. More generally, does this temporal delay lead to hysteresis loops?

-Page 4171, lines 11-14. "The dynamics of NO_3^--N concentration and ïňĆow were also similar during the two rainstorms at all stations (Figs. 4 and 5). The NO_3^--N concentration did not response to rainfall and increased on the recession limbs of hydrographs after the end of rainfall (Figs. 4 and 5)." I agree that no reaction occurs for the 14 May 2003 storm (Fig. 4) but I see some reaction during the rainfall for the 12 May 2004 storm (Fig. 5). The scale between the two figures is slightly different (it may

be a good idea to uniform the scales) but I wonder whether there is a reason for such a difference. Can the author explain it?

-In addition to the references suggested by the first referee, I indicate a supplementary reference that covers some topics discussed also in the manuscript and that might be worth to be considered in the introduction or discussion: McHale M.R., McDonnell J.J., Mitchell M.J., Cirmo C.P., 2002. A field-based study of soil water and groundwater nitrate release in an Adirondack forested watershed. Water Resources Research, 38 (4), 10.1029/2000WR000102.

Technical corrections

-Page 4158, line 20. “fast response time”: do the author mean “recording” time?

-Page 4162, line 2. I suspect that the word “respectively” should be deleted.

-Page 4163, line 16. Is P equal to PP?

-Page 4168, line10. Looking at Table 1 I would report 0.70 mm/day and not 0.64 mm/day.

-Page 4173, line 11: Change “Javie” into “Jarvie”.

-Page 4179, line 30. “Ocamp” should be “Ocampo”.

-Page 4181, line 28. “Weiler, M And McDonnell, J.J.” should be “Weiler, M and McDonnell, J.J.”

-Page 4184, Table 2. Different letters (a, b, c, d) do not clearly identify different statistical significances.

-Page 4186, Figure 1. There are two lines identifying “border”. Maybe “stream gauge” or “notch weir” is clearer than hydrological weir. This also in the text.

-Page 4187, Figure 2 (and also Figure 3, 4, 5, 7, 8). Change the label on y-axis from “rainfall (mm)” to rainfall rate (mm/hour). Adjust the scale so that the bars of

precipitation do not overlap with the curves. What happens at 7.20 am and 8.00 am? Is the storm onset? If so, I would change “Minutes after 7:20”into “Minutes after storm onset” or “Minutes after the storm start”.

-Page 4187, Figure 2. How do the authors explain the two small soil water potential peaks occurring approximately at 450 mins and 650 mins, since no rain were recorded?

-Page 4193, Figure 8. It might be better to specify in the Figure that “E” is the Nash-Sutcliff index.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4151, 2011.