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Interactive comment on "Model aided quantification of dissolved carbon and nitrogen release after windthrow disturbance in an Austrian karst system" *by* A. Hartmann et al.

Anonymous Referee #2

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General comments

The article analyzes the effect of windthrow disturbance on Dissolved Inorganic Nitrogen and Dissolved Organic Carbon exports in a forested karst system. The site is part of a Long Term Environmental Research Observatory, therefore such a study is a relevant and interesting use of the long-term data records that are collected on such sites. The analysis is based on a flow and solute transport model assuming that if the model is calibrated and assessed as able to predict the hydrochemical behavior on a non disturbed period, any failure of reproducing the hydrochemical behavior during and after the disturbance would emphasize actual changes in this hydrochemical behavior induced by the windthrow. The authors used the model to estimate transit

time distribution for quantifying the duration of the storm-induced impacts on water chemistry. The approach is highly relevant to demonstrate how models can be even more interesting when they fail when applied out of the boundary conditions of their calibration/development domain, providing information about what has changed in the system. Another primary assumptions of the study is that forest disturbance due to windthrow in this system is likely to (i) induce N leaching which is actually low in the initial state, and (ii) increase DOC leaching, according to an extensive review of the literature on the effect of forest disturbance on water chemistry well presented in introduction. However their results showed that only N was affected by the storms, and that neither DOC nor discharge depicts any response to the disturbance. The model was applied with parameter set calibrated on the pre-disturbance period and required a recalibration of hydrochemical parameters for simulating properly the DIN signature during and after the disturbance. Overall the paper is well written, while a few details and precisions would be worth to add especially about:

-the processes that are dominant in this systems (see below comments on LULC, fast/slow components, "N saturated systems" definition. . .)

-the procedure in model recalibration (so called "adaptation" by the authors) and underlying hypotheses the unmodified hydrological response to disturbance regarding the respective flow paths of DOC and DIN exports.

The presented work is original, presents a high scientific quality, and is fully relevant regarding the scope of BG. Therefore I recommend only these minor revisions before publication.

Detailed comments and technical corrections:

p. 11989 L.25: Please explain what is meant by "N saturated systems"?

p. 11990 L. 20-21: So the underlying hypothesis is that if the behavior changes, (which would be revealed if the model fails to reproduce behavior after the storms) it would

be due to changes in DOC and DIC inputs in the hydrological system only? As shown by the “adaptation” procedure (p. 11996, L. 15-16) no changes are assumed in the transfer processes: neither in flow paths (and while total flow could be unchanged, its relative contributors may be) nor in transit times along these flow paths because only hydrochemical parameters are readjusted? No transformation is assumed to occur along the flow paths (only before mobilization by water)? Additional discussion or argumentations about this point would be appreciated.

p. 19911 L.13: “Hydromorphic”

p. 11991 L.1 to 5: Is there any difference in the Land Use/land cover between the hillslopes and the plateau?

p. 11992 section 2.2.: So the DOC sources would be unimpacted? Could the impact be hidden by soil buffering effect or variations in the hydrological connectivity (e.g.: if less ET and less interception would induce more infiltration and deeper flowpaths through layers that would be poorer in DOC?)

p.11993 Table 1 does not describe all the variables: Rdiff,i ; Rconc,i ; Qgw,i and Z are missing.

p.11996 L.4 : What kind of threshold or rules are used to characterize the performance as significantly reduced or not? Is it a statistical significance test? If so please cite which one.

p.11996 L9.: At this stage it would be worth to know what are “adapted” and “non adapted “ simulations, it comes just after but these sentences could maybe be rearranged so that the reader immediately knows it?

p. 11996 L. 25: It is unclear for me if these times are mean transit times within the compartment or mean residence times in it as the compartment is part of the system. . .?

p. 11997 L. 1-2: Are slow and fast flows associated to the epikarst and the groundwater or do both contributions have a fast and a slow component?

p. 11997 L.5-7: How long is the pulse in the second virtual tracer simulation?

p. 11997 L.14: Could you explain what a “natural equilibrium concentration” is? The concept of production constant is different from a concentration which results from production/consumption rates but also from export rates and volumes in each component. What does it mean when this concentration is negative?

p. 11997 L. 22-23: Do you have any hypothesis to explain the higher stability of the second sample? Is there any difference in climatic conditions between both samples?

p. 11998 L.4: As DIN is diluted during peak flow and peak flows are underestimated, wouldn’t this contribute to an overestimation of DIN? However, is NH4+ sometimes monitored during peak flows? p. 11998 L. 24: “more than 2 times 2 mg/l that the predisturbance value” this sentence is not fully clear, is it? Please rephrase.

p. 11999 L. 2: How could this phase shift be related to hydrological changes (e.g. in relative contribution or mean transit times of the components)?

p. 11999 L. 17: “The soil” please remove comma. Aren’t these large storage capacity values related to the short storage constants? (There is probably some correlation between these parameters?)

p. 12000 L. 9-10: How was the “realism” of hydrochemical values appreciated? Were they compared to measurements? P_DIN is homogeneous to a concentration and not to a rate so I wonder how realistic is a negative value?

p. 12001 L. 7: Why total flow doesn’t vary? If the loss of trees is enough to change N uptake I am surprised that it is not enough to change transpiration. Moreover, there is at least some changes in the dynamic of flow: p. 12002 L.26.

p. 12003 L. 10: What were the dominant ranges of water ages in groundwater?

Figure 6: please correct in the legend “observed” and “comparison” p. 12024

Figure 7: please correct in the legend “scenario 1”, “scenario 2” and “variation” p.

12025

Figure 8: please correct in the legend “groundwater”, ”infinite virtual”, and “starting”.

Interactive comment on Biogeosciences Discuss., 12, 11987, 2015.