We thank referee #2 for her/his positive opinion and valuable remarks. Please find below our response to the general and detailed/technical comments:

General comment 1: *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. . .)*

Response: More details to these points will be provided in the revised version of the manuscript. Please see our response to the detailed/technical comments below.

General comment 2: *a few details and precisions would be worth to add especially about (…) 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.*

Response: We will elaborate the adaption procedure in more detail in the revised version of the manuscript. Concerning the underlying hypotheses about the hydrological response on the disturbance please refer to our responses to detailed/technical comments 2 and 19.

Detailed/technical comment 1: *p. 11989 L.25: Please explain what is meant by "N saturated systems"?*

Response: Generally, pristine forest ecosystems are defined as N limited systems due to the marginal deposition of N and the lacking supply from weathering (i.e. growth is limited by the absence of available N). The substantial economic and population growth in Europe and North America since the 1950s has caused extensive emissions of nitrogen oxides (NO_x) into the atmosphere. In addition, the intensification of agriculture emitted large quantities of ammonia (NH_3). Subsequently, elevated deposition of airborne N increases the amount of N within the forest ecosystems readily available for prominent biogeochemical processes like tree growth, mineralization of organic carbon and nitrification. Sustained elevated N deposition raises the N status of these ecosystems until N saturation. Nitrogen saturation of forests is reached when the availability of inorganic nitrogen exceeds demand by plants and microbes and causes elevated NO_3 concentrations (in surface waters), elevated NO_3 leaching, soil acidification and nutrient imbalances of plants. We will extend the introduction of the revised manuscript to clarify on this.

Detailed/technical comment 2: *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.*

Response: Referee #2 is right – a disturbance on the forest cover can affect more than the DOC and DIN mobilisation and transport. There are possible impacts on hydrological processes such as a decrease of transpiration or an increase of groundwater recharge. But due to the karstic characteristics of our study site this increase may minor comparted to the typically high karstic recharge rates (see also our response on detailed/technical comment 19). In Figure 2e we show that there is no obvious change in the variability of discharge before and after the disturbance. Admittedly internal processes may change but if so, these changes are not identifiable by observed discharges alone. A better understanding about changes of system internal hydrological processed could only be derived by system internal observations, which were not available for this study. This information, as well as a more detailed discussion on possible changes of hydrological processes will be added to the discussion.

Detailed/technical comment 3: *p. 19911 L.13: "Hydromorphic"*

Response: Corrected

Detailed/technical comment 4: *p. 11991 L.1 to 5: Is there any difference in the Land Use/land cover between the hillslopes and the plateau?*

Response: Both plateau and slopes are mainly covered by forest. Norway spruce (Picea abies L. Karst.) interspersed with beech (Fagus sylvatica L.) was planted after a clear cut around the year 1910. The vegetation at the slopes is dominated by semi-natural mixed mountain forest with beech (Fagus sylvatica) as the dominant species, Norway spruce (P. abies), maple (Acer pseudoplatanus), and ash (Fraxinus excelsior). If necessary, bark beetle abatement measures (i.e. salvage of trees infested by bark beetle and/or affected by wind) were conducted at the plateau since the installation of the LTER site in 1993. At the slopes no forest management has been conducted since the implementation of the National Park. We will add this information to the study site description.

Detailed/technical comment 5: *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?)*

Response: In the sort-term forest disturbance has a substantial positive impact on DOC production via the large input of dead organic matter and altered soil climate. In the long run organic carbon input to the disturbed ecosystem – as important DOC source – decreases due to the decreasing litter input. However, as most of the produced DOC processed by microbials and respired back to atmosphere as CO2, the effect of forest disturbance is superior for NO3 than for DOC sources. Concerning DOC leaching, the disturbance effect seems to be the net outcome of increased DOC leaching due to increased and accelerated seepage fluxes and its highly efficient adsorption on mineral soil compartments within soil. Surprisingly, Figure 2b shows no substantial effect of forest disturbance on DOC leaching. Thus, more detailed analysis of existing data and high temporal-resolution sampling have to be undertaken to elucidate the effect of forest disturbance on DOC leaching within the studied ecosystem. We will expand the discussion of the revised manuscript by these interesting points.

Detailed/technical comment 6: *p.11993 Table 1 does not describe all the variables: Rdiff,i ; Rconc,i ; Qgw,i and Z are missing.*

Response: Table 1 was only meant to provide a complete list of model parameters, their description, units, ranges, and optimised values. Simulated fluxes as R_diff,I, R_conc,I, or Q_gw,I are variables that change over time; they do not have upper or lower ranges that are used for calibration. We therefore decided defining them within the methods description instead of another table.

Detailed/technical comment 7: *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.*

Response: We considered deviation of performance as significantly different when a component of KGE (correlation, bias, or variability) fell below or above its pre-disturbance variability as indicated by the whiskers of the calibration/validation periods in Figure 6. We will add this important information to subsection 3.3 of the revised version of the manuscript and to the caption of Figure 6.

Detailed/technical comment 8: *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 rear-ranged so that the reader immediately knows it?*

Response: The mentioning of adapted and non-adapted simulation will be rearranged accordingly. Thanks for this helpful advice.

Detailed/technical comment 9: *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. . .?*

Response: Thanks to referee #2 for this clarifying comment. As we assume complete and instantaneous mixing with each model storage (soil, epikarst groundwater) at each compartment, the time that we refer to as "mean transit time" of a model compartment is the time the virtual tracer needs to pass through the particular storage. If we would have only one storage for each compartment, our mean transit time would be similar to the mean residence time of the compartment but since we look at series of different storages that exchange virtual tracer within and between the model compartments we the term "transit time" more appropriate. A clarification will be added subsection 3.4 in the revised version of the manuscript.

Detailed/technical comment 10: *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?*

Response: Both epikarst and groundwater have slow and fast storage components as defined by their distribution of storage coefficients in Eqs (A6) and (A12). A clarification will be added to subsection 3.4.

Detailed/technical comment 11: *p. 11997 L.5-7: How long is the pulse in the second virtual tracer simulation?*

Response: The disturbance period lasted from May 1st, 2007, to September 30th, 2011. This is mentioned in the results section but it was not clearly stated that the same period was also used to define the length of the second virtual tracer injection. The missing information will be added to the revised version of the manuscript.

Detailed/technical comment 12: *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?*

Response: The term "natural equilibrium concentration" is not chosen well at least for DOC and DIN. As explained in subsection 3.1.2, we assume net production rates that result in typical DOC/DIN concentrations, which are variable over the model compartments and constant over time (DOC) or constant over the model compartments and variable over time (DIN). Negative values, as found for DIN, indicate that during some periods of the year all DIN is consumed by plants or soil organisms. But as also shown in Table 1, an amplitude A_DIN of the seasonal DIN production of 3.36 will mg/L also result in positive values of DIN production at another period of the year. In the revised manuscript, we will consistently use the term production rate over the entire manuscript. We will also clarify the meaning of negative DIN values in the discussion.

Detailed/technical comment 13: *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?*

Response: Thanks for this valuable comment. Since both samples' time span is only 4 years and the resolution of the hydrochemical variables (SO4, DOC and DIN) is rather low, differences between the two samples may mostly be due to their rough resolution. Since both samples are bootstrapped from the same period, climatic conditions are the same. A clarification will be added to subsection 4.1 of the revised manuscript.

Detailed/technical comment 14: *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?*

Response: This is a good point. Indeed, an under-estimation of peak flows would go along with a weaker dilution of DIN concentrations. However, since the model is calibrated by discharge and solute concentration, the resulting parameter sets may compensate for this, for instance by a reduced the DIN production parameter. Since the resolution of DIN observations is quite low compared to the resolution of the discharge observations we cannot evaluate the model's behaviour during events in more detail. High-resolution sampling of DIN (and NH4+) may provide some more insight, but such data was unfortunately not available for our study. We will add some discussion on calibration related compensatory effects on simulated solute concentrations in the revised version of the manuscript.

Detailed/technical comment 15: *p. 11998 L. 24: "more than 2 times 2 mg/l that the pre-disturbance value" this sentence is not fully clear, is it? Please rephrase.*

Response: The sentence will be rephrased in the revised version of the manuscript.

Detailed/technical comment 16: *p. 11999 L. 2: How could this phase shift be related to hydrological changes (e.g. inrelative contribution or mean transit times of the components)?*

Response: This small shift towards earlier DIN production may be due to a decreased shadowing effect due to the windthrow. Snow melt would initiate earlier going along with an earlier DIN production and leaching. Hence, an earlier snowmelt may also be visible in the discharge observations. However, due to the rather slow melting rates, most of the melting water will slowly/diffusively enter the groundwater system rather than flowing rapidly through the karst conduits. Therefore, a slightly earlier beginning of snowmelt may not be visible at the system outlet due to the slow reaction of the groundwater storage (also see our response to detailed/technical comment 19). We will add some more discussion on possible (non-visible) changes on the hydrological behaviour of the system in the revised manuscript.

Detailed/technical comment 17: *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?)*

Response: Thanks for this valuable comment. In the revised manuscript, we will specify our elaborations about the relatively high storage capacities of the soil and the epikarst by mentioning possible parameter interactions between their storage capacities and storage coefficients. The comma will be removed, too.

Detailed/technical comment 18: *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?*

Response: This was an unfortunate formulation. In the revised manuscript we will rephrase it to "A DOC production parameter P_DOC of ~1.6-1.8 mg/L resulted in realistic simulated concentrations at the weir." About an elaboration of the meaning of negative P_DIN values please refer to our response on detailed/technical comment 12.

Detailed/technical comment 19: *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.*

Response: This is a very good question. Our study site is composed of karstified dolostone resulting in strong subsurface heterogeneity. As a consequence there is an interplay of fast preferential flow and low diffuse flow through the subsurface resulting in a very dynamic hydrological behaviour at the outlet (see for instance Fig 4). When preferential flow paths activate during wet conditions large parts of the flow can bypass the soil resulting in generally lower evaporation rates in karst systems (Hartmann et al., 2014, 2015). Therefore, hydrological impacts of windthrow on karst systems may not be as pronounced as in non-karstic domains because a large fraction of the infiltration during high flow periods will not be available for transpiration anyway (see also our response on detailed/technical comment 2). However, during medium and low flow conditions, most of the water passes the soil and windthrow related changes of transpiration may alter the hydrological behaviour, as they also alter DIN production. Decreasing differences of pre-disturbance and wind disturbance DIN concentrations with increasing discharge (Fig. 2d) may support this argumentation. We will add these points to the discussion of the revised manuscript.

Detailed/technical comment 20: *p. 12003 L. 10: What were the dominant ranges of water ages in groundwater?*

Response: Previous studies (Kralik et al., 2009) indicated water ages of weeks to months at the weir (by Oxygen-18 analysis), while they found fast transit times of days (artificial tracer experiments) and old waters of several years (CFCs, SF6 dating) at small individual springs within the study area. Hence there is some indication that the mean transit times found by the virtual tracer experiment reflect at least the behaviour of the sub-catchment drained by the weir, which can be regarded as more dominant than the rather local observations at the springs. This information will be added to the revised version of the manuscript.

Detailed/technical comment 21 *Figure 6: please correct in the legend "observed" and "comparison" p. 12024*

Response: the legend will be corrected.

Detailed/technical comment 22: *Figure 7: please correct in the legend "scenario 1", "scenario 2" and "variation" p.*

Response: the legend will be corrected.

Detailed/technical comment 23: *Figure 8: please correct in the legend "groundwater", "infinite virtual", and "starting".*

Response: the legend will be corrected.