Dear reviewer,
please check our responses to your comments (marked in blue). The final changes will be made to the manuscript at the stage of submitting revision later. Thank you!

RC1:

This manuscript presents a series of hillslope-scale numerical experiments intended to explore how the topographic slope controls nitrate export. The authors applied a hillslope model with parameters from their previous catchment-scale modeling work and meteorology data from a small agriculture catchment in Central Germany as input. They mainly found that the response of in-stream nitrate concentrations to topographic slope is a three-class pattern rather than monotonous. The science questions and approach would appeal to the HESS journal audience and make a nice contribution to understanding these connections between topography, subsurface flow paths, and nitrate export.

However, I had some major concerns that need to be addressed prior to publication:

1) It is lack of field data to either constraint or validate the model. Although the authors provided lots of descriptions about their study site, it seems to me that they only applied the meteorology data and some parameters from the previous modelling work at this site as the modelling input. **Neither the simulated hydrological nor biogeochemical (nitrate) part are justified or calibrated according to the real field data** (e.g., water table depth, water content, and nitrate concentrations). Even though the authors stated that it is not important to accurately reproduce the flow discharge and solute concentration, I think it is still necessary to make sure that the simulated values are **comparable** to observations.

Response #1:

We agree that the comparison with data was missing in the study. Our 2D model was extracted from a 3D one of our precious modelling work, where the stream discharge, groundwater table were calibrated against measurements. Unfortunately, we do not have discharge and groundwater table data specially for the 2D slice.

We have nitrate concentration (C) data measured at the outlet of the 3D catchment. These C data are not for the discharge from 2D slice, however, we can still show that the simulated C is comparable with the measured C, but to exactly reproduce the measured C should not be expect. The comparison will be added to the manuscript in our revised version.

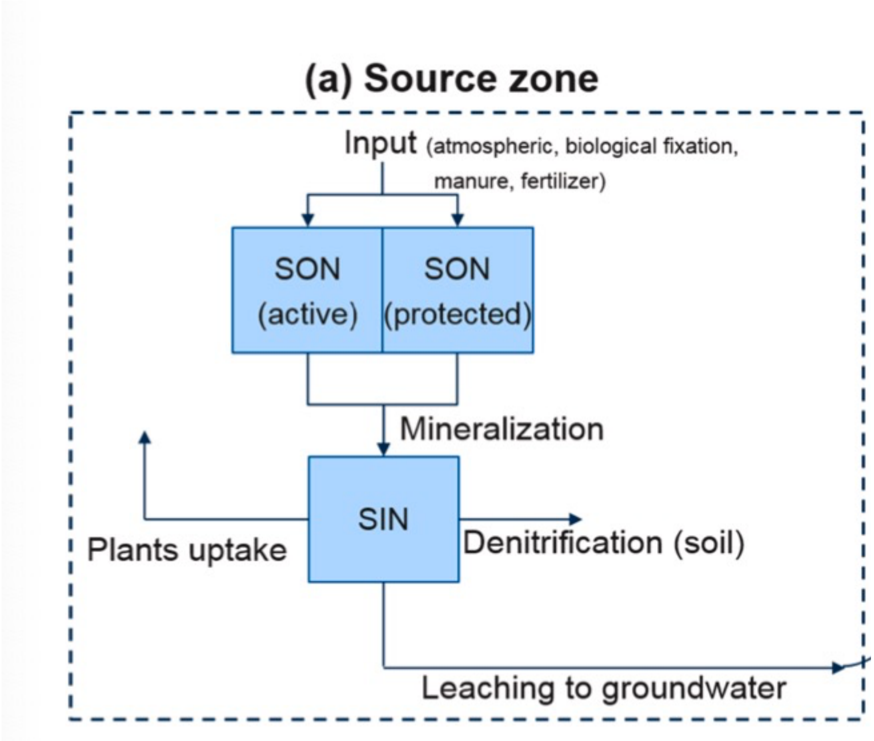
2) It was not always clear throughout the manuscript what assumptions are made, why the authors made such assumptions, and their implications. For instance, the authors used annual- average precipitation and monthly-averaged potential ET as the input to

simulation the hydrological dynamics across the year. This leads to the inconsistent of time scales for precipitation and pET, especially for this work with a focus on seasonal variations. I don't understand why not to use **monthly-averaged precipitation** to keep consistent with pET. For the nitrate transport, to simply the model, the model does not include the evapoconcentration effect for nitrate transport. If that is the case, how does the model handle the nitrate concentration and fluxes from the input source with precipitation (i.e., before ET) to soil water after ET occurs? It seems to be mass imbalance for nitrate transport.

Response #2:

Thank for the suggestion. We will change to monthly-averaged precipitation to ensure consistence in the revised version. We used constant precipitation but seasonally changing ET because the seasonality of the catchment is main driven by the ET and the precipitation is more uniformly distributed over the year.

For the N mass balance issue, we used an approach of N source concentration curve, which force the N concentration in the rainfall to change along the curve at the moment of entering the soil. The assumption was that the variation of N source concentration caused by the evapoconcentration was already considered in the N source concentration curve. This assumption did cause mass imbalance of the system. To overcome this limitation, we will use a different approach, which has been used in the study of *Yang et al.,* 2021, to simulate the nitrogen fluxes in the soil, including the input, mineralization, degradation in soil, crop-uptake and leaching into groundwater (see the figure below). This approach will increase model complexity by introducing new parameters, however, ensure a mass balance. This work will be updated to the manuscript in our revised version.



Reference:

Yang, J., Heidbüchel, I., Musolff, A., Xie, Y., Lu, C.*, Fleckenstein, J.H. (2021). *Using nitrate as a tracer to constrain age selection preferences in catchments with strong seasonality*, Journal of Hydrology, 603, 126889.

3) Additional results are needed to better support the conclusion. The objective of the manuscript is to explore the influence of topographic slope on nitrate export. However, the authors only showed the effluent nitrate concentration and its temporal variations. How about the **overall export rate of nitrate** (concentration * effluent flow rate)? I think this may better reflect nitrate export. The main reason is that the topographic slope alters the water content and water table depth (Figure 4d), and further change the simulated ET and how much water infiltrates into the subsurface and eventually enter the stream. Besides, the authors assumed (Line 220) that ET does not alter the nitration concentration in the subsurface. However, if ET varies with topographic slope, this would lead to the inconsistency of the **overall nitrate mass** into the subsurface for different scenarios. Therefore, the conclusions based on in-stream concentrations might not really hold true.

Response #3:

Thanks for the suggestion. We will discuss also the exported nitrate mass flux in the revised version to better support the conclusions.

For the mass balance issue caused by the ET. We will use a different approach to simulate the nitrogen fluxes in the soil (see the last response). This approach will ensure that the input of overall nitrogen mass into the subsurface is same for all scenarios.

4) The assessment of source contribution and the terms of source- and degradation-dominated needs clarification. I remained unclear about the physical meaning of the equation the authors applied to calculate the source contribution. Line 284: why does 0 and 100% represents degradation-dominated and source-dominated, respectively? I think this is conflicted with the main assumption that nitrate transport is dominated by the transit time or flow path (i.e., hydrology-dominated). Besides, Line 284-297: the authors introduced Damköhler number in the method but did not really use it in the rest part of the manuscript. I guess they intended to define transport- and reaction-limited system?

Response #4:

Thanks for pointing out that. What we want to express was that the variation of in-stream concentration may attribute to two aspects: (i) the availably of nitrite source changes over time, and (ii) the reaction (degradation) along the flow path. The term "Degradation-dominated" may be more accurate expression than the term "hydrology-dominated", because the degradation is a combination of transit time (influenced by flow paths) and degradation rate (influenced by various factors such as temperature). We will clarify that in the revised version.

Thanks, we will simply delete the unused content of Damköhler in the revised version.

5) In-depth discussion about nitrate export is needed. The main goal of this work is to build a connection between hillslope topography and in-stream nitrate concentration through flow paths and water age. However, the current discussion mainly focused on the influence of hillslope topography on subsurface flow paths and water age. Their linkages with nitrate export and how the numerical results here are related to previous field and modelling work are largely missing. Besides, this work is based on **hillslope-scale numerical experiments r**ather than catchment- scale modelling. The authors should also consider the s**cale differences** when they directly applied results from this work to explain observations at the catchment scale.

Response #5:

We agree with that. We will add more discussion about the nitrate fluxes between different scenarios. We will try to relate our simulated result with previous studies. We will also clarify that our study was based on the hillslope-scale and discussion the influences of the scale differences. This work will be updated to the manuscript in our revised version.

Other comments with line number:

Line 215-219 and Figure 2: Is the constant source calculated from flow averaged or time-variant concentration averaged? I think it needs to be flow averaged. Otherwise, the overall input mass is not consistent between the two scenarios.

Response #6:

We used a time-weighted average rather flow-weighted average. Because we used a constant rainfall over the year, that means same amount of nitrate was inputted into the system. However, we plan to use a different approach to define the nitrate source zone (see response #2), replacing the nitrate source concentration curves. This work will be updated to the manuscript in our revised version.

Figure 5: Horizontal flow and vertical flow: how to calculate the percentage? Even in the saturated zone, the water can still flow horizontally and vertically.

Response #7:

For each cell, we check the velocity to see if it is more horizontal or more vertical. Then we summed up all the volumes of all cells where velocity is more horizontal (or vertical) and calculate the portion to total aquifer volume. We will clarify that in the revised version.

Figure 5 and 7: what about the spatial distributions of nitrate concentrations? I think adding such figures may help the readers understand the modelling and results.

Response #8:

Thanks for the suggestion, we will add that figure during the revision.

Line 355-360 and Figure 5: The flow fluxes into the land surface take a high proportion in the case of slope 1:20 and 1:60. Does nitrate continue to degrade actively in the land surface? If it does, this may not really hold true for real nature systems. Please clarify and discuss the potential implication.

Response #9:

Thanks for pointing out that. The degradation was not considered on land surface. We will clarify that in the revision.