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.

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.

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.

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?

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 rather than catchment-scale modelling. The authors should also consider the scale differences when they directly applied results from this work to explain observations at the catchment scale.

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.

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.

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.

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.