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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Aged streams: Time lags of nitrate, chloride and tritium assessed by Dynamic Groundwater Flow Tracking" *by* Vince P. Kaandorp et al.

Anonymous Referee #1

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General comments:

In this study the authors use a physically-based, spatially distributed, 3D groundwater model with particle tracking to explore how dynamic TTDs influence stream concentrations of nitrate, chloride and tritium (excluding transport through the unsaturated zone). To this end a sensitivity analysis is conducted where a) an unsaturated zone is added, b) travel times are increased by a factor of 5, input time series are altered in c) time and d) space and e) denitrification is added. Definitely an interesting approach that seems to gain interest in recent years. Still the analysis/discussion of the results could use some more work. At the moment, results are reported but not discussed

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Discussion paper

in sufficient depth concerning the potentially responsible hydrologic processes. Thus far the discussion can even be condensed as it contains several repetitions. Also the model lacks measures to evaluate its performance/fit. Likewise the sensitivity analysis does not include a comprehensive evaluation of the parameter importance. Addressing these issues would prevent the results and discussion to sound as vague as they currently still do. The overall conclusions are not that surprising and will benefit from a more in-depth discussion. The authors manage to write comprehensively and with a good overall structure. Just every now and then they should try to be more consistent when using short versions (Cl/NO3) or the words “chloride”/”nitrate”. When reading the title of this manuscript I was hoping for more novelty in the results and discussion sections. Yes, the study is well-written and has a good structure, but somehow I feel that the authors do not go much beyond reporting the results. There is neither a lot of analysis nor discussion on which controls are important and specifically why they are more or most important. I like the modeling approach although it bothers me that it is purely a groundwater model without unsaturated zone processes. However, I believe that the authors can manage to add more analysis and discussion to merit and justify a publication in HESS. Please find specific comments in the annotated PDF as well as below.

Specific comments:

Abstract: Try to underline the innovative parts of your study more. Think about more precise results (like TT results, R2) Isn’t it important to at least name the investigation area?

p.2, 33: "through“ instead of "though“?

p.2, 38: how do you mean "land-use“ in this context?

p.2, 41: "Land-use determines the timing and quantity of nitrate input“ would be more general

p.2, 42: ".depending on the source and timing of infiltration“

p.2, 52: What time is meant by "historical“?

p.3, 74: If 20 % of the river are not baseflow, is this not an important NO3 source due to surface runoff with high solute concentrations?

p.3, 85: How big are the sub-catchments up- and downstream?

p.3, 71: How can you differentiate between up- and downstream, when having only one gauging station?

p.3, 89: Do you know rates of drainage? How much percent of the agricultural area are artificially drained? What is the difference of these rates between up- and downstream? I think it's an important number as later on it seems to be a large factor (Fig.9).

p3, 91: I cannot find groundwater data or cannot differentiate which collected data are from the surface and which are not.

p3, 93: What time period is covered by "historical“ data?

p.3, 96: Where is the first explanation of the abbreviation?

p.3, 91- p.4, 101: As you join different data sources, it would be helpful to add a table consisting of the time of observation, the resolution of the data, the amount of measurements. . .

p.4, 102: Can you give more information on the "dynamic TTD“?

p.5, 140: What are the distances of the two stations to the corresponding measurement station?

p.5, 148: “following Meinardi (1984)”

p.5, 154: How can you exclude land-use change?

p.5, 156: How can you assume NO3 to be constant over time although atm. N-

deposition changed strongly within the last decades?

p.5, 159: What is the size and resolution of the data set? How did you manage data gaps?

p.5, 160: Abbreviation of N?

p.5, 160: Later on you write about seasonal N contribution, which is highest in winter. Do you know the time of fertilizer application within the year? Are not the spring and potentially the fall also highly predestined to show N-peaks?

p.6, 175: Cl and NO3 concentrations or input? When did the concentration change then, at January 1st or when fertilizer application (e.g. April) would be realistic?

p.6, 178: What is the resolution of N input in the model? How can it be constant over the year?

p.6, 190-191: Could you revise this sentence? It is hard to understand.

p.7, 195-206: Does it makes sense to test for these parameters? What is the innovation?

p.8, 228: "partly by oxidation of organic C (1) or pyrite (2)". So it is easier for the reader to assign the two equations.

p.8, 236: It is also assumed as a first-order decay (e.g. van Meter et al., 2017) in literature. Why do you assume zero-order?

p.8, 240: Do you have information about the denitrification potential assuming it as a finite process?

p.8, 249: Figure 3d as well? In the following sentence, I propose to add "Figure 3d".

p.8, 252: Highest contribution in winter although highest N inputs can be assumed in spring and are washed out until winter? And how can you see seasonal NO3 patterns assuming constant NO3 input over the year? So I think "agricultural con-

tribution" (assumed as constant) is not the appropriate wording, perhaps NO3 transport/mobilization?

p.9, 268 I cannot associate the word "later" with the mentioned years.

p.9, 273: What is a reasonable fit? Do you have a measure for this, like R2?

p.9, 284-286: Would not denitrification cause an earlier peak/decay than Cl instead of the surprisingly later peak. Is the changing N-Cl ratio not excluded by the input data of the farmer (chapter 2.5)? Do you have any hints for potential point sources?

p.9, 286: What point sources are realistic in a non-urban area?

p.10, 295: Can you adjust the headings in this chapter or in the according method section so that it is easier to assign?

p.10, 297: What is meant by "did not completely fit"? Think about using a measure to show the actual fit.

p.11,321: Think about adding (Figure 6, "red line") and later on (. . ., "green line"). Than it is easier to match. But this is probably a matter of taste.

p.12, 349-351: Do I need a larger area for N variations? Isn't it only caused by varying flow paths? And is "diluted" the appropriate wording? This implicates lower N concentrations in young water, which is often not the case.

p.12, 346-352: Did you consider a combination of unsaturated zone and denitrification, so not only delay but at the same time decay?

p.12, 356: Is it not "decreased" instead of "increased"?

p.12, 371: ". . .when the MTT is rather long or the input decreases slowly"

p.12, 377: "It is however known"

p.13. 379: Also "temporal" (temperature dependence), not only spatial differences?

p.13, 408: Can you explain these non-linear reactions?

p.14, 441: So you assume transport limitation instead of source limitation, otherwise the wet period would cause dilution.

p.15, 445: Can you describe the changing discharge in Figure 3a still as “relatively stable year-round”? Are the NO3- differences caused by different Q ages (stable Q assumption, but different paths) or Q amounts (wet and dry period assumptions)?

p.15, 466-473: This information could be also important earlier in the methods.

p.17, 510-511: How reliable is the model if you cannot constitute one (downstream) of two (up- and downstream) catchments?

p.17, 522-540: Could you underline the innovative management implications?

p.18, 564-565: What is new in this statement?

Figures & Tables:

p.27, Figure 1: I miss an x-axis label for the figure bottom left. Think to add also a,b,c,d to the different plots. This would also benefit the figure caption. Where is the border between up- and downstream catchments in bottom right?

p.27, Figure 2: According to the caption, the tritium curve is based on measurements from Groningen and Emmerich, but isn’t this the case only for values later than 1970?

p.29, Figure 4: Think about plotting also an input curve in the same plots. This would make the in-output-comparison much easier.

p.30, Figure 6: Is there a superfluous “the” in the end of the caption?

p.31, Figure 8: I am surprised that the upper RCA does not contribute any water to the GCA. And in the text overland flow is also included in the RCA. Could you revise the figure to show that?

p.32, Figure 9: When the colors are similar to Figure 3, where does the purple come

from?

Supplement

p.1, 24: I do not understand the “just”. Do I really need combinations of TTDs and reduction rates to determine a lag between in and output? I don’t think so. Perhaps in your special case, but not in general.

p.2, Table S1: What is the definition of the delay? The peak matching? In Figure S2 you write “time to peak”. This could be helpful as a column in Table S1 too.

p.3, Figure S2: The same colors as in Figure S1 but with different meanings are confusing. Think about another color or line code.

Literature

van Meter, K. J., Basu, N. B., and van Cappellen, P.: Supplement,TwoCenturiesofNitrogenDynamics:LegacySourcesandSinks 105 in the Mississippi and Susquehanna River Basins, Global Biogeochem. Cy., 31, TS41, 2017b.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-552/hess-2019-552-RC1-supplement.pdf

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