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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Real-time monitoring of nitrate transport in deep vadose zone under a crop field – implications for groundwater protection" *by* T. Turkeltaub et al.

T. Turkeltaub et al.

tuviat@post.bgu.ac.il

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Anonymous Referee #1

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We would like to thank the reviewer for his/her quick and helpful review. The comments were constructive and helped us to improve our manuscript. All comments were addressed in reply to reviewer comments.

Specific comments: Comment 1: Line 30 – "and these isotopes were barely affected by natural soil or industrial nitrogen components." It is not clear how this conclusion is reached. I suggest revisiting the interpretation of the d15N results. Reply to comment

1: The reviewer's comment was accepted and the manuscript was revised accordingly (Lines 27-29). To strength up the result and show robustness of our conclusions we added to figure 4 ïĂăïĄđ'18O VS ïĄđ'15N measurements. The ïĂăïĄđ'18O -ïĂăïĄđ'15N isotopic composition method was used in previous investigations to assess sources of nitrate in groundwater (e.g. Wassenaar 1995), and in vadose zone as well (e.g. Turkeltaub et al., 2015b). According to Figure 4b, all water samples are situated within the manure range.

Turkeltaub, T., Kurtzman, D., Russak, E. E., and Dahan, O.: Impact of switching crop type on water and solute fluxes in deep vadose zone, Water Resour. Res., 51, 9828–9842, doi:10.1002/2015WR017612, 2015b. Wassenaar, L. I.: Evaluation of the origin and fate of nitrate in the Abbotsford Aquifer using the isotopes of 15N and 18O in NO3, Appl. Geochem., 10, 391–405, doi:10.1016/0883-2927(95)00013-A, 1995.

Comment 2: For Figure 4, please provide a reference for the arrows showing ranges of 15N ratios for different sources. They seem to be from Figure 16.9 in Kendall, 1998, (Tracing Nitrogen Sources and Cycling in Catchments, in Isotope Tracers in Catchment Hydrology (1998), C. Kendall and J. J. McDonnell (Eds.). Elsevier Science B.V., Amsterdam. pp. 519-576). Because there are no d18O measurements in this study, a better source of information about typical ranges of d15N in sources might be figure 16.4 in that same chapter, which shows manure separate from septic: http://wwwrcamnl.wr.usgs.gov/isoig/isopubs/itchfig16-4.html or Fogg et al., 1998, Figure 1 https://info.ngwa.org/GWOL/pdf/981563606.PDF In either case, it seems that many of the measurements shown in this figure could be from soil organic N, and that all the measurements could be mixtures of soil N and manure. I suggest revising the discussion accordingly. Reply to comment 2: The reviewer's comment was accepted, figure 4 and the manuscript were revised accordingly (Line 295; Lines 513 - 514). Another graph was added to figure 4, which displays the ïĂăïĄđ'18O measurements as well. The discussion was not revised, since it was based on the analysis following the Dual Nitrate Isotopes method.

Comment 3: Also, there is an interesting trend in the d15N values with depth (from 0 to 12.6 m), and the sample at 12.6 m has relatively low d15N. Did the source of N vary over time? Given the estimated vertical velocity of 0.9 m/yr and the date that these samples were collected, it should be possible to estimate the time at which the NO3− in the 12.6 m sample entered the soil. What was going on at this site at that time? Reply to comment 3: As the reviewer mentioned, the water sampling obtained at 12.6 m depth is different and diverts from the general trend displayed by the rest of the water samples obtained by the sampling ports. This might be explained by the spatial variability which the vadose zone monitoring system captures. Each monitoring unit faces a different undisturbed sediment column that extends from land surface to the probe or sampling port depth. Therefore, the patterns of the measured processes as water percolation and nitrate transport might be somewhat different for each sampling port or water sensor. However, earlier studies with the vadose zone monitoring system measurements indicate that 1D vertical analysis could explain significant processes which occur in the deep vadose zone. A new estimation of the pore water velocity (see reply to comment 10 and lines 341-349), after including the nitrate time series obtained at the 9.5, 15.3 and 18 m depths, is 3.05 m/y. The nitrate origin from manure source (dairy slurry) reached to 12.6 m depth after 3.8 - 4.1 years, which is in the range of the current study.

Comment 4: 33 – "excluded the possibility of lateral nitrate input". It's not clear how the model results lead to this statement. How does a 1-D model, fitted to breakthrough at a single depth, "exclude" the possibility of lateral nitrate input? Reply to comment 4: We accepted the reviewer's comment that the model results and the total nitrate mass calculation could not exclude lateral nitrate input and revised the manuscript accordingly (Line 33). Generally, the driving forces for vertical flow component in the unsaturated zone are significantly higher compared to the lateral vectors. Moreover, there are no other potential sources of nitrate and water on surface in the vicinity of the monitored field. Hence it is likely that lateral contribution of nitrate is negligible. The vertical pore-water velocity was estimated with 1-D transport model. Subsequently, the veloc-

ity value was compared with results from earlier study on the same site, Turkeltaub et al. (2014), where the recharge fluxes were estimated with 1-D flow transient numerical model. The models were calibrated to different data sets. In Turkeltaub et al. (2014) the flow model was constrained to climate data as top boundary condition and calibrated to water content measurements obtained from multiple depths of the deep vadose zone. The data used for the 1D model in the current study were the nitrate-concentration time series. Nevertheless, both models estimated similar velocities. We would expect that any lateral nitrate or water contribution would affect the models results, which did not occur. Moreover, the total nitrate mass calculations indicated that the nitrate mass observed in the upper parts, remained in the vadose zone cross section and reached the deeper parts of the vadose zone.

Comment 5: 88-91 – In terms of the novelty of the current study, this seems to be an important point that there are few 5+ year monitoring studies of NO3− in the deep vadose zone. Consider moving this to a more prominent location and/or adding a similar statement in the abstract. Reply to comment 5: The reviewer's comment was accepted and the manuscript was revised accordingly (Lines 23 - 24). Furthermore, the Introduction section was reorganized and some parts were omitted (Lines 42 -106).

Comment 6: 98 – 106 – I suggest adding a sentence to clarify how the existing studies relate to the current study. How does this study build on, or differ from, the previous VMS studies of long-term monitoring of NO3− in the unsat zone, especially those at the same site (Turkeltaub et al., 2014)? This will help to clarify the novelty of the current study. Reply to comment 6: The reviewer's comment was accepted and the manuscript was revised accordingly (Lines 83-94).

Comment 7: 122 – Please add brief info about samples taken for N and for d15N. How were the samples collected? When were the d15N samples collected? Reply to comment 7: The reviewer's comment was accepted and the manuscript was revised accordingly (Lines 160 - 173).

Comment 8: 126 – Please give the start and stop dates of the study in this sentence or in the first sentence of the paragraph below (line 136). Reply to Comment 8: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 117).

Comment 9: 136 – Please explain - what was the land use of the study site before 2009? Reply to Comment 9: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 125-127).

Comment 10: 320 – Meaning is unclear for "none of these assumptions could be found in the field". Presumably this means that the assumptions were not violated, but that seems inconsistent with figures showing $NO3^-$ breakthrough that is not consistent with a uniform homogeneous medium, e.g. in 2013 there is breakthrough at depths of 9.5m and 15.6m but not at the intervening port at 12.6m. I suspect that the 1-D analysis approach would not do a good job of fitting all the depths simultaneously, and that different sample depths/locations have different effective transport properties. Consider discussing this issue in more detail, relating to other unsat zone studies, and possibly proposing measures to address the related uncertainty. Reply to comment 10: We intended to explain the discrepancies between observed and simulated values, and indicated that the gaps might be a product of the analytical solution assumptions. However, this sentence was misleading and therefore was deleted (Line 334). The nitrate time series from the 9.5, 15.6 and 18 m depths were included in the calibration efforts. Although there is a relatively good agreement between simulated and observed nitrate concentrations, there are still discrepancies that related to the CDE assumption of steady average velocity and homogeneous medium. Obviously, the analytical solution does not stand alone and it is compared with earlier numerical simulation efforts. Moreover, we added a discussion concerning the calculated effective dispersivity coefficient. We agree with the reviewer's comment that there are horizontal and vertical distributions of the effective transport properties in the soil. However, comparison between the observations from all depths indicated that only the observations obtained

at the 12.6 m depth divert from the general observed trend. Yet, since each and every monitoring unit is located under a different soil profile, we relate this deviation to spatial variability that was not considered in this work. It can be investigated only with 3d model simulations, which are not in the scope of this work.

Comment 11: 326-327 – Please clarify - The model was calibrated against data from the depth of 6.3m, so for consistency it would seem to make sense to use the water content at that depth (rather than at 3.1m) to calculate the annual water flux. Reply to comment 11: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 341). Since we used water content observations from different depths within the sandy layer of the vadose zone, weighted average water content was calculated.

Comment 12: 329 – Section 3.5 – I suggest revising to emphasize the most novel results of this study. Some additional analysis may be necessary in previous sections to identify the most novel contributions. The first paragraph of this section is very generic, more like introductory material than discussion. The second paragraph mentions the current study, but does not explain how the results of this study add to our knowledge of vadose zone monitoring as a tool to understand $NO3^-$ delivery to groundwater. Readers already know that applying manure slurry contaminates groundwater. They will be more interested to read about what this study (maybe in combination with previous studies) tells us about time-scales of transport, interactions of 15N with soil N, magnitude of dispersivity, or other open questions about vadose zone N. Reply to comment 12: The reviewer's comment was accepted. We revised section 3.5 - 'Practical implications of vadose-zone monitoring', since both reviewers indicated that this section should emphasize and elucidate the novelty of this study (Lines 359-391). Technical issues: Comment 13: Comment 55 – "the land surface" Reply to comment 13: The reviewer's comment was accepted. However, we rephrase the last sentence of this paragraph and these words were excluded from the manuscript (Line 54).

Comment 14: 56-64 – Consider combining this single-sentence paragraph with another

paragraph, or expand to clarify how these various methods relate to the current study. E.g. Is there a lack of studies that characterize long-term NO_3^- concentration profiles in the vadose zone? Reply to comment 14: The reviewer's comment was accepted, the Introduction section was reorganized and some parts were omitted (Lines 55 -67).

Comment 15: 77 – I suggest "concentrations" or "levels" instead of "level". Is the point of this sentence that it can advantageous to study NO_3^- transport in the unsaturated zone, closer to the source, before mixing occurs in groundwater? Reply to comment 15: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 77).

Comment 16: 80 – Consider substituting "common practice" in place of "easy". Reply to comment 16: The reviewer's comment was accepted, however since the Introduction section was reorganized this sentence was deleted (Lines 42 -106).

Comment 17: 108 – "patterns" Reply to comment 17: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 97).

Comment 18: 176 – should be "were analyzed" Reply to comment 18: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 185).

Comment 19: 186 – Throughout the paper consider using "vadose sampling port", "sampling port" or just "port", which will have more meaning to readers than the abbreviation "VSP". Reply to comment 19: The reviewer's comment was accepted and the manuscript was revised accordingly (Lines 147, 150, 161, 163, 196, 213, 214, 241, 295; table 1 and figure captions of figures 1 and 6).

Comment 20: 219 – "From September 2009 to the end of the study in January 2015"? Reply to comment 20: The reviewer's comment was accepted; however the sentence was deleted following the second reviewer's comment (Line 222).

Comment 21: 236 – Consider giving an approximate interval between sampling (e.g. "approximately 4 times per year") in place of "frequent" Reply to comment 21: The

reviewer's comment was accepted; however the sentence was deleted following the second reviewer's comment (Line 236).

Comment 22: 256 – "as compared to" Reply to comment 22: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 267).

Comment 23: 277 – missing "e" at end of author's name Reply to comment 23: The reviewer's comment was accepted and the manuscript was revised accordingly (Line 292).

Comment 24: Fig 1 – Is there any significance to the color distinction for blue versus red arrows? Reply to comment 24: We accepted the reviewer's comment and Figure 1 was redesigned. All arrows were painted with blue color to avoid any misunderstanding. Comment 25: Fig 3 – The different vertical scales on the panels makes it difficult to interpret the data. Consider plotting a few different depths on a single panel. Reply to comment 25: We accepted the reviewer's comment and Figure 3 was redesigned.

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