**Response letter**

We thank both reviewers for their constructive and insightful comments on our manuscript. The manuscript has now been revised extensively in accordance with all reviewers' suggestions. Major changes implemented in the revised version of the manuscript include:

1. Extensive revisions of the results section, where we now present observations by soil and seepage period instead of cumulative values. This allowed to gain deeper insights into treatment impacts on N leaching, seepage, yield and N export. We also added a new results graphic that presents the timing of the first tipping (i.e. onset of seepage water generation), which occurred significantly earlier under NIT than under CT under soils G and S (Fig. 5 A, B). And we added results from a mixed linear model, which revealed that variance in seasonal $NO_3^-$ leaching between 2013 and 2016 was best explained by the interactive effect between precipitation amount and treatment, the treatment effect, precipitation and the interaction between treatment and SOC (Section 3.7).
2. Extensive revisions of the discussion section in accordance with reviewer recommendations. In particular, section 4.2 "Likely mechanisms underlying observed tillage impacts on $NO_3^-$ concentrations" was revised to improve clarity in the line of argumentation.
3. Adjustments in all other sections to align with the revised content of results and discussion sections and to account for all reviewers' comments as detailed below.

Following these revisions we are confident that our manuscript is now in a form that is acceptable for publication.

---

**Reviewer 1**

This manuscript evaluates the effect of non-inversion tillage on nitrate leaching in soil. This is, in my opinion, a relevant and original topic. The manuscript is concise and well written, and the study behind it was conducted throughout a good timespan. My main concern is related to the statistical analysis which, in my opinion, should be extended and deepened to support (or disprove) some of the conclusions. In addition to this, I understand that the main focus was given to the effect of treatments on nitrate leaching. However, and considering the importance of yield to farmers, I think that the focus that was given to yield should be increased throughout the manuscript. I include my specific comments below.

➔ Thank you very much for the thorough and constructive review! Your comments have helped us a lot to reflect on and improve the content of the manuscript. After careful consideration of all your comments and also considering comments from the second reviewer, we have substantially revised the manuscript, in particular the results and discussion sections.

Methods: In my opinion, a subsection regarding data analysis or statistical analysis should be added at the end of the Methods section.

➔ We agree to this and have added a section on statistical analysis at the end of the method section. We fitted a linear mixed model to quantify how much of the variance in nitrate leaching could be explained by treatment difference, precipitation amounts and soil parameters, respectively.

➔ To test significances of treatment effects, t-tests were applied.

Results: The comparisons between treatments should be supported by statistical analysis such as t-tests or their non-parametric equivalents. Yield should be given more relevance

throughout the paper, considering its importance to the farmer. In addition to this, I believe that a PCA or correlation analysis may reveal more information from the data.

➔ Significance of treatment difference was tested based on t-tests. In the revised manuscript, we report statistical significances (i.e. p-values) wherever appropriate. Text sections referring to graphics have been revised in accordance with new graphics.

➔ The linear mixed model revealed that variance in annual $NO_3^-$ leaching between 2013 and 2016 was most strongly influenced by precipitation amount, the treatment effect, SOC and their interactive terms.

Discussion: Similarly to what I have referred about the results, the effects of treatments on yield should be given a higher weight.

➔ We revised all graphics and moved information on treatment impacts on yield and exported N from the appendix to the main text (Fig. 6, Fig. 7)

L80: ETa equation is shown. However, it is not mentioned in the text.

➔ We refered to equation 1 in line 95 before.

L88: Do the authors have information regarding soil sampling depth?

➔ The table summarizing soil profile descriptions was moved to the main text (Table 1). Depths of each described soil layer are mentioned in this table. Soil samples were taken within each of the described layers. Methods of soil analyses are described in ll. 151-153.

L119-120: A diagram representing the distribution of crop rotations and cover crops throughout the time of the experiment would increase the clarity of the explanation.

➔ Good suggestion. We revised this part and added a table to list crops in the rotation and respective mineral fertilizer inputs (Table 2).

L141-142: This information should be included in the aforementioned subsection about data analysis.

➔ This was entirely revised. We now show impacts of treatments also by soil type as we found this supports the interpretation of results even though differences between soil types were not statistically significant.

L145-146: This should be checked by a statistical approach such as a t-test or a non-parametric alternative.

➔ We used t-tests and added the information on statistical significances of treatment differences.

L145: Please indicate the Figure/Table where the cummulative ETa is shown.

➔ The figure showing daily evapotranspiration was revised. Fig. 8 now presents seasonal sums of ETa by soil type and growing season and across soil types by growing season.

L146-147: Please add here the reference to the Figure where the yield results are presented. If I am not mistakened, it is Figure F.1 (Appendix F). Maybe transfer it to the main results? In any case, the possible differences between treatments should be confirmed (or disproved) by a statistical analysis. Information about the number of replicates per treatment should be added in the legend of this (similarly to what the authors did in the legend of Figure 3).

➔ Yes, we transferred this graphic to the main text now and added information on statistical significances of differences. Also the information on number of replicates was added to the figure caption (Fig. 6)

L147: Please see my comment about the need for significant differences to be checked by statistical analysis.

➔ See responses to earlier comments on this: significance levels are now provided whereever appropriate. A section on statistical analysis has been added to the methods section.

L157-159: The authors stated that “nitrate concentrations were significantly higher in CT than NT”. Is this confirmed by a statistical analysis? If so, please show that information in a Figure or Table or refer to one where that information is shown.

➔ A new figure shows bi-weekly nitrate concentrations over the observation period and indicates significant levels where concentrations differ significantly between treatments.

L165-167: In my opinion, this hypothesis (and others that the authors find appropriate) should be included at the end of the Introduction. If the authors did not find this appropriate, the sentence should be re-written.

➔ Good point, thanks! We expanded the introduction section to provide more detail on our initial hypotheses (ll. 65-72).

L200-202: Authors claimed that ”Results presented in this study are generally in line with Li et al. (2023), who concluded (...) that the benefits of reduced tillage for nitrate leaching reduction tend to be higher on soils with medium texture and SOC contents >1% in temperate climate zones and with longer durations of reduced tillage practices”. In my opinion, this is not accurate as the authors did not compare different soil textures, different SOC levels or different climate zones.

➔ This was clarified (ll. 379-386): As previously highlighted by Li et al. (2023), the SOC content of soils seems to be an important determinant of tillage impacts on N leaching. Our results support this statement: the tillage treatment had the strongest impact on N leaching under the soil type with the largest SOC content (i.e. Stagnic Cambisol from Reckenholz). This could be explained by the fact that SOC contributes to structural coherence within soil aggregates, promoting more continuous and interconnected pore networks as for example found by Deuer et al. (2009) or by Liu et al. (2025) in Cambisol samples. This would imply that the “bypass effect” described above is amplified by SOC content, which is in line with our observation that no treatment impact on N leaching was observed with the Cambisol from Grafenried with SOC of 0.99%, while the treatment impact was largest on the Cambisol from Reckenholz with SOC of 1.45%.

L221-223: The authors referred that “nitrate leaching was not significantly higher under CT than NIT during days directly after tillage, but only after the termination of sugar beet in 2014”, which seems to indicate that a statistical test (such as a t-test or Mann-Whitney test) was used. However, if I am not mistaken, the results of such test are not shown in the paper.

➔ Significance levels are now indicated in all figures where this is appropriate.

L249-257: The authors discuss the relationship between SOC and nitrate leaching. This is an interesting relationship which, however, was not tested. Do the authors have data available regarding the SOC content of the studied soils? If so, maybe a relationship between SOC and N leaching could be tested.

➔ Yes, very good point. Thank you. We added information on initial SOC and N contents to the table summerizing soil properties of the three soil types (Table 1). We also included SOC in the mixed linear model, where it turned out to be a relevant driver in interaction with the treatment effect (Table 3. Fig. 11)

L265-289: In subsection 4.3 (Unresolved links), the authors discuss different knowledge gaps regarding the approached subject that exist in literature. In my opinion, the limitations and strengths of their present study should also be approached in this or in other subsection. This could also include suggestions for future work on this subject.

➔ The discussion section was revised extensively to improve readability and clarity in the line of argumentation. In the last subsection of the revised discussion (outlook) wer now refer to limitations of this study and suggest possibilities to overcome them in future studies.

Technical corrections

L115-118: Scientific names should be written in italic.
➔ done

Throughout the manuscript, nitrate is identified as NO3. I would suggest using its correct chemical formula (NO3-). In some places, it is also identified by its name (nitrate). Therefore, I would also suggest to homogenise this either by identifying it by its name or by its formula.
➔ Done. We now use $NO_3^-$ throughout the manuscript.

L159: The non-inversion tillage treatment is identified as NIT throughout the manuscript. However, here and in some other places it is identified as NT.
➔ Thanks for noting the mistake. That was corrected now.

---

**Reviewer 2**

The manuscript presents lysimeter results comparing conventional tillage (CT) and no-inversion tillage (NIT) across three soil types under the same crop rotations over a 2–3 year period (2013–2016). The topic is timely, and the results clearly indicate differences in nitrogen losses between CT and NIT. That said, the manuscript would benefit from substantial revisions to improve clarity, structure, and the strength of its mechanistic interpretation.

➔ Many thanks for the thorough and constructive review! We appreciate the your feedback and valuable suggestions and have revised the manuscript extensively to improve its clarity, structure and mechanistic interpretations.

In several places, the writing reads as a sequence of observations rather than a cohesive argument that explicitly links proposed mechanisms to the reported patterns. In addition, key pieces of evidence are sometimes introduced late in paragraphs, where readers would instead expect a clear synthesis or “take-home” message for the section.

➔ The structure of the results section was revised completely to improve clarity of presented results. We also added more relevant information on initial soil properties for the three soil types (Table 1). We now show results also by soil type to support the interpretation and we present results from a mixed linear model of annual leaching loads following recommendations from reviewer 1.

➔ To strengthen the linkage between observations and mechanisms, also the discussion section was revised and restructured extensively.

Relatedly, the manuscript includes considerable discussion of carbon’s role in mediating nitrogen loss under different tillage practices, but it is not clear that carbon content was directly measured in the study soils. The contribution could be strengthened considerably by incorporating additional measurements—such as soil carbon quantity and forms of nitrogen (and, ideally, complementary water chemistry where feasible)—to better support the claims about nitrogen retention and the drivers of observed losses.

➔ Very good point. We added information on initial SOC and N-contents for the three soil types (Table 1). Inclusion of measured soil organic carbon contents in the mixed linear model allowed to quantify the influence of SOC on seasonal N leaching.

At present, I think the manuscript requires major revision before it can be considered for publication. While the dataset is interesting and potentially valuable, the current presentation feels closer to a field report of patterns than a study that systematically evaluates multiple factors shaping nitrogen dynamics. Several important variables (e.g., carbon content, aggregate stability, nitrogen species, litter quantity/quality, rooting depth, and the impacts of plowing) are discussed as likely explanations but do not appear to be directly measured, aside from porosity. Where porosity is reported (notably in the appendix), it would also help to clearly indicate which values correspond to each treatment (NIT vs. CT), and to ensure those results are integrated into the main narrative as appropriate.

➔ We agree that the presentation of results and interpretations lacked clarity in the submitted manuscript. We therefore restructured and rewrote the results section completely, also improving readability of graphics. The revised results section starts with a presentation of the main finding: nitrate leaching is higher with CT than with NIT. In the following subsections we present and describe results that support our interpretation of mechanisms driving the observed treatment effect. Our data shows that the observed treatment effect on nitrate leaching is driven by differences in nitrate concentrations in seepage water and not seepage water amounts. We argue that this difference in nitrate concentrations is attributable to a difference in flow pathways, differences in crop residue amounts and presumably differences in denitrification. While our data supports the hypotheses that differences in flow pathways and residue amounts have contributed to the observed difference in N leaching, it is not suffiencient to provide evidence on the relevance of either of these mechanism in the experiment. In the last sub-section of the discussion, we refer to these limitations of our study and points at possible ways to overcome them in future studies.

➔ In general, the discussion section was revised extensively to improve readability and clarity in our line of argumentation.

Finally, the manuscript would benefit from careful attention to consistency in figures and text, including standardized styling (colors and formatting) and consistent treatment labeling/abbreviations throughout.

➔ Previous figures were replaced by new ones to improve readability, also paying attention to consistency in formatting/labelling/colors.

Overall, I see real promise in this work, particularly in highlighting management differences and the importance of site context. To make the manuscript publication-ready, I encourage the authors to (1) strengthen the linkage between mechanisms and observations in the writing, (2) add or more directly incorporate key supporting data (especially soil nitrogen, and carbon if central to the interpretation), and (3) more fully integrate relevant supplemental information (for example, the temperature data) into the analysis and discussion.

➔ By restructuring, rewriting the results section with the additions of the mixed linear model, we strengthen the link between observed treatment differences and influencing mechanisms

➔ To support the line of argumentation some of the material from the appendices was moved to the main text:

- Climate information was moved to section 2 (Fig. 1)
- Soil profile information was moved to section 2 (Table 1)
- The graphic on dry matter yields by harvest year and treatment was moved to the results section (Fig. 7)

➔ Figures showing soil moisture, water tension and soil temperature in 10 cm depth were integrated into the results section. In our interpretations of observed treatment effects and influencing mechanisms, we refer to these graphics specifically.

Lines 35-40: I think it would strengthen the authors argument if you can add a line about how widespread these tillage practices are (i.e. what proportion of farms or what percent of land undergoes method X or Y via management). This would strengthen your paper's relevance and impact.

➔ We added a paragraph to the introduction section providing this information (ll. 59-64): "Reduced tillage practices are widely adopted around the world (Porwollik et al 2019). The highest levels of adoption are observed in South and North America, followed by Australia and New Zealand, Asia, Russia and Ukraine, Europe, and Africa (Kassam et al. 2019). In the European Union, reduced tillage is subsidized through the EU CAP "eco-schemes" and the share of conservation tillage was 22% in 2016 (Eurostat). According to the Swiss Federal Office for Agriculture (FOAG), the adoption rate is even higher in Switzerland with 32% of all arable land was under conservation soil management in 2019."

Graphical abstract: It currently does not suggest based on the drawing that one method goes deeper than the other. Furthermore, the abstract doesn't show contrast with regards to changes to soil structure which appear to be central in the author's argument.

➔ There was no difference in tillage depth. Tillage depth was 20cm both in CT and NIT. The graphic was modified to indicate differences in macropore continuity and aggregate stability

Line 42: N2O should be subscript (similar for all other chemical formulas brought into the text and used in your graphs/figures, use correct notation and subscript!). The sentence after this makes no substantial addition to the introduction nor does it connect to the next paragraph. Reconsider structure here.

➔ This part on reported co-benfits of reduced tillage practices was shortended and revised to improve clarity

Line 49: "addressed the inconsistency in the impacts of tillage on nitrate leaching currently documented." Or rewrite another way, its very awkwardly phrased at the moment.

➔ Was revised accordingly

Line 51: This last sentence does not connect well with the next paragraph nor is it a good concluding sentence.

➔ True, this is actually not a concluding sentence. The concluding sentence was in the next paragraph, which was now moved and revised.

Line 53: Why is this sentence separate from everything else?

➔ Exactly. This sentence had to be moved upwards (see previous comments)

Line 55: This is not a complete paragraph. Plant uptake is something that could decrease the amount of leachate correct? Then this wouldn't be a driver but instead a factor that affects the magnitude of nitrate leachate. In this case calling these "processes that influence nitrate leaching dynamics" would be more accurate.

➔ Thanks for the recommendation. This was revised accordingly (ll. 67/68)

Line 79: Any time you place the variable from your equation into your main text, keep the same text style as the one you used in the equation description (is it subscript a or normal a?).

➔ We thoroughly checked the whole manuscript to eliminate all such inconsistencies in style and formatting.

Line 95-97: this is exactly why you need to have a section in the introduction that can speak to how widely used this practice is globally. It enhances your research's relevance to global community while emphasizing its importance and makes sure content like this is already introduced so that it doesn't take up space later in the manuscript where you need to focus on the experimental details instead.

➔ We added a paragraph to the introduction section

Line 105: Did each treatment receive the same cover crop treatment? It would be helpful to have a schematic here of the experimental design or a table in the SI.

➔ Yes, the rotation was the same in each treatment. This is now clarified through the inclusion of Table 1, listing all crops in the rotation. The treatment difference was only related to the soil management (NIT vs. CT.). The experimental plan was added to the appendix (Fig. A.1).

Line 115: The listing of the cover crop rotations is confusing with some have a (+) and others not having this. Improve the clarity of what you are trying to express here. Is there a particular order (numbering may be better). Is there a set duo? (note this in the text). At the moment this listing is somewhat disorienting and difficult to parse. Additionally, italize scientific names.

➔ This section was revised to improve its readability. A table was added listing crops by year and N fertilization amounts.

Line 120: These values should be supplied in the SI or some note on using these to normalize your results should be made (or if they weren't that different from each other between the treatments, this should be noted).

➔ This was revised. A new table lists this information. There were no differences in fertilization levels between treatments.

Line 125: Be consistent throughout your text with regards on how you will be referring to your nitrogen species (chemical formula? Written name? Also if you choose chemical formula add respective charges and subscripts – be consistent!).

➔ We thoroughly checked the whole manuscript to eliminate all such inconsistencies in style and formatting.

Figure 1 & similar style comments for figure 2

➔ All results figures were redone to improve readability and consistency in style.

Where is the key for the first top figure? (monthly precip and mean temp)

➔ Climate information was removed from the results and is now presented in section 2 (Fig. 1).

Label these figures as Fig 1a 1b 1c etc  same for figure 2

➔ Revised figures were labelled as recommended.

Figure axes labels are blurry and so is a few of the graphs included here (fix please!)

➔ All figure were revised to improve readability and are provided in vector format.

Label evapotranspiration as evapotranspiration.

➔ The graphic was redone (Fig. 8)

Monthly precipitation data is provided in Month:Year format and the following figures that are meant to be aligned over the same time periods as this precip data has Year:Month:Day. Change these to have the same formatting or change these to all be over one axes if appropriate (which I believe it is).

➔ The revised graphics are aligned over a common time axis (i.e. bi-weekly in accordance with water sampling intervals in Fig. 4).

I would further suggest placing the name of the crop that was collected over the top of the colored boxes.

➔ Crop names were placed inside colored boxes.

Be consistent in all of your graph labelling – will you capitalize or not? Don't short hand write the labels, use full words to convey what you are showing and explain in detail the units in the captions if necessary.

➔ We thoroughly checked the whole manuscript to eliminate all such inconsistencies in style.

Line 185: reformat the figure caption:
coloured background shading is in reference to cropping periods: purple (phacelia), yellow (sugar beet) … ; grey dashed vertical lines denote ploughing times; …etc

➔ Done.

Get rid of the "=" they are hard to read/decipher with everything else that's being listed in this caption.

➔ Done.

For figure 2 make one large legend for parts b, c, d (label these figures accordingly)

➔ The figure was replaced by new figures, which are labelled where appropriate.

Figure 3 should be colored to match earlier graphs. However, choose a color palette that is color blind safe. Also denote significant differences on your figure in some manner (asterisks or letters).

➔ A color-blind palette is now used and where appropriate, p-values or asterisks were added to the new graphics to indicate statistical signifcances.

Line 205: Your mechanistic connection here between pore structure and matrix water flow is not very clear. Are you saying that increased tillage under CT would reduce pore connectivity or increase pore presence? What about pore structure is facilitating the matrix water flow in CT vs NIT. You leave your readers without a clear connection between the two and then just citing various literature that suggest similar trends. It will really help drive your point if you make the connection clearer here.

➔ This part of the discussion was extensively revised for clarification: "The intense topsoil disturbance in CT is likely to have reduced macropore continuity and with that the share of macropore flow though the soil. This hypothesis is supported by Roseberg and Mccoy (1992), Buczko et al. (2003) and Chakraborty et al. (2022), who found that macropore continuity can be reduced through ploughing even if total porosity is increased through tillage. This tillage impact on water fluxes could explain higher $NO_3^-$ concentrations in CT: With a higher share of matrix water flow in the nutrient-rich topsoils of CT, more $NO_3^-$ would be carried through the soil matrix as it is known that more homogenous flow through the soil matrix favours the transport of highly soluble substances such as $NO_3^-$ (Radolinski et al. 2022). This mechanism termed "bypass effect" has been described by several previous studies (e.g. Bjorneberg et al. 1996, Shipitalo et al. 2000, Hess et al. 2020, Miranda-Vélez et al. 2022)." (ll. 361-372)

Line 210: This sentence is just a list of literature, integrate it into the earlier sentence and cut down on the number of citations or still include them but not as an in-line reference just in the ().

➔ In the revised version of this discussion sub-section we make clear in what respect we refer to these references (see response to previous comment)

Line 215: “In our study, also..” the structure of this sentence is awkward, edit this to improve delivery of the point you are trying to make here.

➔ As part of the extensive revisions of the discussion, this was revised and improved.

Line 225: I think what your results may be suggesting is that it is the combination of crop and soil management technique that directly affects the rate and timing of your N leachate. You are implying here that the nutrient quality and quantity of the sugar beet reincorporated into the soil after harvest had an impact in N mobilization. Do you have data on rooting depth? If roots are going deeper than what the plow can do this may add to your story here.

➔ Unfortunately, we do not have data on rooting depth from this experiment, but with the addition of information on SOC in the mixed linear model we could point out that the treatment effect is significantly influences by precipitation and soil organic carbon content.

Line 247 to 252: This sentence is way too long

➔ This sentence was revised to improve readability.

Line 252: Include Dr. Colunga’s meta-analysis work on conservation (reduced) tillage and conventional tillage in sandy arid environments with carbon deprived soils https://doi.org/10.1016/j.still.2024.106310

➔ We added this reference in the introduction section where we refer to benefits of reduced tillage for carbon sequestration.

Section 4.2 – I think the big message here is that the benefits of CT or NIT with regards to N mobilization and leaching are extremely context dependent. You can improve the delivery and conclusion of this section by coming towards a conclusion like this rather than having a paragraph that just details a variety of different impacts from these practices in different arid and low C environments. This makes this section currently just feel like a list rather than a cohesive argument coming to a point. Enhance the delivery of that context dependence message by incorporating the literature suggested earlier but also noting that this is dependent on regional practices (crops of cultivation, rooting and biomass size, etc).

➔ Yes, thank you for the comment. We agree that the structure of the discussion was not well established and we have revised and re-written the discussion to improve readability and clarity in the line of argumentation.

Figure E1: labels are upside down and poor resolution of axes and repetitive keys make this super messy.
Figure E2: similar as E1 but it is super blurry and almost impossible to read.
Figure E3 is similar as E1

➔ All these figures were redone with consistent color-coding and labelling. We include vector graphics in the revised manuscript.

Figure F1 has a completely different color scheme. There is no indication here of significant different between sugar beet harvest in the NIT or CT. The bottom axes has the labels off centered and the double ticks seem unnecessary and messy.

➔ This figure was removed and replaced by a new figure in the main text in alignment with the previous color scheme (Fig. 7).