Reviewer 1
General comments

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.

Specific comments

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. To test significances of treatment effects, the t-tests or the non-parametric Mann-Whitney test if samples were not normally distributed. We also fitted a linear mixed model and applied ANOVA to quantify how much of the variance in nitrate leaching could be explained by treatment difference, precipitation amounts and soil parameters, respectively.

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 or the non-parametric Mann-Whitney tests (where the assumption of normality was not met). In the original manuscript, the p-value was reported only for the treatment differences in cumulative nitrate leaching. 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.

➔ ANOVA 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.

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

➔ We refered to equation 1 in line 79 before.

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

➔ The table summarizing soil profile descriptions was moved to the main text. Depths of each described soil layer are mentioned in this table. Soil samples were taken within each of the described layers.

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.

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 or Mann-Whitney 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. Cumulative values are only reported in the text. We added p-values to cumulative values mentioned in the text.

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.

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. In this case, Mann-Whitney tests were applied, since the assumption of normality was not always met.

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.

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.

➔ Li et al. conclude that reduced tillage is most likely to lead to reduced nitrate leaching on soils with medium texture, SOC contents > 1%, temperate climate and long duration of tillage practise and all these characteristics largely apply to our case study. In that respect, our results are not in conflict with the findings from Li et al.. This is now clarified in the revised manuscript.

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. The table is now in the main text and we included SOC in the ANOVA, where it turned out to be a relevant driver, paricularly in interaction with the treatment effect.

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) were 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.