

Accounting for differences between crops and regions reduces estimates of nitrate leaching from nitrogen-fertilized soils

Corresponding Author: Professor Yanjiang Cai

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Professor Cai,

Your manuscript titled "Improved accuracy in global nitrate leaching inventories by refining regional and crop-specific leaching factors for applied nitrogen" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Somaparna Ghosh, PhD

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and also in our style and formatting guide [Communications Earth & Environment formatting guide](https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

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- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at <http://www.nature.com/sdata/policies/repositories>.

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Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Comments and Suggestions for Authors:

– The article “article_7517_0_art_file_140697_sh2vm2” can be considered for pro publication but with resolution of minor errors. In this study a meta-analysis is developed that measures the effect of various factors (hydrological, soil properties,

crop type) on the NO₃ leaching process. The title should be modified, as it should give an understanding of the study area and contain the word meta-analysis. In the summary section, it fulfills its objective, there are only some inconveniences with some established quantities, which are not clear what the authors are referring to. Introduction: it fulfills its objective, there are only some inconveniences with some very old references, the use and establishment of certain acronyms. Methodology: The methodology is well established and reproducible. Results: In general, it is very good, however, there are some concerns in the results regarding the establishment of the random forest and SEM models, due to the fact that a variable used in the calculation of LF is used, so this variable presents the highest proportion or participation in the models, which should be omitted and these models should be re-established. Discussion. All very well, only the discussion regarding the models should be modified, because of what was mentioned above. Conclusion: No conclusion of the study is presented. Specific comments are shown below:

-
- - L1. The name of the article should mention or contain the area of the study and the word meta-analysis.
- - L34-35. It is not clear why the amounts should be established in this way, unless they are three different regions (which should be established) or three different crops (which should be established).
- - L35. The meaning of the acronym IPCC should be established.
- - L36. Is the term Tier-1 correct?
- - L57. Only use the most recent reference, unless it is very important.
- - L60-61. Same comment as above, check this throughout the text.
- - L74. Up to this moment the IPCC breviture is established, while it has been used before.
- - L170. I do not believe that it is correct to use the variable leachate amount in the prediction of LFs, because these two variables are dependent on each other, since the amount of N leached was used in the calculation of LF (equation 1) and practically in my opinion it is the same. It is necessary to make the analysis without taking into account this variable and observe if they fit the model in the same way.
- - L177. Same as the previous comment.
- - L233. Review the use of this variable in the models.
- - L382. Mention the depth.
- - L405. How many sites are we talking about?
- - L467. Establish the term SEM

Reviewer #2 (Remarks to the Author):

This study from Wang and colleagues explores a bottom-up approach to derive the fractions of N inputs (manure and synthetic fertilisers, but also other organic amendments) that is leached below the root zone. To do so, the authors compiled data from 2602 field observations from 198 studies, including important covariates such as leachate volume and other pedoclimatic variables. The authors distilled those data using a meta-analysis, structural equation modelling and the machine learning random forests.

The study is a valuable addition to refine national inventories, not only in terms of N leaching but also, by extension, of its indirect N₂O emissions. The usefulness of this study is to be commended given the authors provide a computationally inexpensive way to upscale N leaching estimates, which require the use of complex process-based models that often fail to precisely simulate leaching dynamics. The data and models generated in this study can provide a key tool for a wide range of stakeholders across the globe – even acknowledging the biases within the scientific literature that tend to focus on specific countries/regions.

As a potential user of such data/models, I am certain that the authors can (and should!) go a step forward and provide a fully spatially explicit simulation of annual leaching fractions in agricultural land, while openly sharing the data and the models. This would considerably increase the study's relevance and attractiveness. Other than that, the paper reads well and I only have minor concerns. Please see my comments below.

Comment 1: How can you aim to refine inventories if you are not being transparent and following the FAIR principles? To do so, I recommend the following:

- Sharing the dataset and statistical/predictive model that others can use to make predictions
- Derive spatially explicit (10km x 10km) and nationally leaching factors emulating FAOSTAT – this opposes lines 86-87 and l352 as you did not provide spatially explicit leaching factors.
- Providing a comparison, for those spatial scales, between IPCC (i.e., FAOSTAT) and your leaching fractions to see where the largest disagreements area

All geospatial/statistical data are already available (e.g., crop maps from MapSPAM, N inputs from synthetic fertilisers and manure from Tian et al 2022 or FAO, crop-specific N fertiliser inputs from Coello et al 2024, soil data from OpenLandMap, irrigation from Mialy et al 2024). Leachate is trickier but you could use groundwater recharge, which can still be considered a proxy, from Wan et al 2024 or, alternatively, from AQUASTAT at the national level.

Tian, H., Bian, Z., Shi, H., Qin, X., Pan, N., Lu, C., Pan, S., Tubiello, F. N., Chang, J., Conchedda, G., Liu, J., Mueller, N., Nishina, K., Xu, R., Yang, J., You, L., and Zhang, B.: History of anthropogenic Nitrogen inputs (HaNi) to the terrestrial biosphere: a 5 arcmin resolution annual dataset from 1860 to 2019, *Earth Syst. Sci. Data*, 14, 4551–4568, <https://doi.org/10.5194/essd-14-4551-2022>

Mialyk, O., Schyns, J.F., Booij, M.J. et al. Water footprints and crop water use of 175 individual crops for 1990–2019 simulated with a global crop model. *Sci Data* 11, 206 (2024). <https://doi.org/10.1038/s41597-024-03051-3>

Coello et al 2024 [2406.10001] Global Crop-Specific Fertilization Dataset from 1961-2019 (arxiv.org)

Wan et al 2024 <https://doi.org/10.1029/2023WR036182>

Still to this end, given the title emphasis on inventories, the study deserves a “policymaker” figure – one that easily synthesizes the key findings (i.e., confirms how IPCC tier 1 is often useless). Figure 6 could be the figure but requires some changes. For instance, (i) I do not think the cropland adds much, (ii) the regions are unclear (e.g., SEA N leaching is in South Asia). Concerning the strengths, you omitted a very useful thing: bottom-up approaches typically use process-based models, which are very computationally intensive and require a high degree of know-how to setup and parametrize models. Your work distills research data that can be used at different spatial scales: from a grid (provided you follow Comment 1) to the global, while also providing a computationally inexpensive and somewhat transparent tier 3 leaching factors.

Comment 2: The overall structure of the discussion could be improved. For instance, rather than sparsely commenting on strengths (e.g., l295-297) and limitations or recommendations (e.g., l349-350), I suggest the authors to streamline the overall structure so it reads better and it provides more

Minor comments

L141 – Did you run a significance test? If so, provide the required metrics (e.g., p-value, degrees of freedom). Otherwise, reword (e.g., substantially). Please check this across the MS.

L381 – I am curious whether using the sampling depth of N leaching measurements as a covariate could be beneficial. As I am unable to explore the dataset you compiled, I am left wondering if it would be possible, maybe in a future study, to decompose N leaching for different soil depths. This would be particularly relevant for drylands as soils tend to be less “thick”. Furthermore, disaggregating water inputs into precipitation and irrigation could likely be beneficial to better predict N leaching in dry regions where percolation events in rainfed systems are essentially a few days per year. However, I understand doing so can be largely outside of your control since it would depend on how descriptive the studies are. As a reviewer and someone who has somewhat done similar things, I am aware of the limitations of what I highlighted above. Doing so, however, would potentially lead to an even larger attractiveness to a larger audience.

L391-395 – This sentence is a rather important part of the replicability of the methods but it is written in an unclear way. Please specify whether all “covariates” were extracted from the 198 studies or if any other external geospatial sources were used.

L405-406 – Given this could introduce some biases in the model, please specify the number of exact cases where you used the average “background” N leaching.

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Version 1:

Decision Letter:

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Dear Professor Cai,

Your manuscript titled "Improved accuracy in nitrate leaching inventories by refining regional and crop-specific leaching factors for applied nitrogen: a global meta-analysis" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and

therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Somaparna Ghosh, PhD
Associate Editor,
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Comments and Suggestions for Authors:

The article "article_7517_0_art_file_140697_sh2vm2" can be published because the comments made in round 1 were corrected. The specific comments and modifications made by the authors are shown below:

– Modifications made

– - L1. The name of the article should mention or contain the area of the study and the word meta-analysis.

– - L2. Modification made.

– - L34-35. It is not clear why the amounts should be established in this way, unless they are three different regions (which should be established) or three different crops (which should be established).

– - L33, 37. Modification made.

– - L35. The meaning of the acronym IPCC should be established.

– - L39. Modification made.

– - L36. Is the term Tier-1 correct?

– - The authors mention that the term is correct, and that they also included information in the introduction section L57-60. But I can't find anything related to this term in those lines. I found the information up to line 73.

– - L74. Up to this point the IPCC brevity is established, while it has been in use before.

– - L39. Modification made.

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- L170. I do not believe that it is correct to use the variable leachate amount in the prediction of the LFs, because these two variables are dependent on each other, since the amount of N leached was used in the calculation of LF (equation 1) and practically to my way of seeing it is the same. It is necessary to make the analysis without taking into account this variable and observe if they fit the model in the same way.
 - L177. Same as the previous comment.
 - L233. Review the use of this variable in the models.
 - The three previous comments were answered satisfactorily.
 - L382. Mention the depth.
 - L417. Modification made.
 - L405. How many sites are we talking about?
 - L445. Modification made.
 - L467. Establish the term SEM
 - L509. Modification made
- Modifications not made
- L57. Only use the most recent reference, unless it is very important.
 - L60-61. Same comment as above, revise this throughout the text.
 - In the two previous comments, it is not clear which of the references was left out.

Reviewer #2 (Remarks to the Author):

I congratulate the authors for such detailed revisions over such a short period of time. I am very pleasantly surprised with the new additions to the manuscript. It is now much stronger and more appealing to a wider audience: now it can be used for national inventories but also for spatio-temporal modelling of N leaching hotspots. From my end, the study is now ready for publication.

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Response to reviewers

Communications Earth & Environment manuscript 7517_0_art_file_140697_sh2vm2

Title: Improved accuracy in nitrate leaching inventories by refining regional and crop-specific leaching factors for applied nitrogen: a global meta-analysis

We sincerely thank all the reviewers for their critical reading and constructive comments and suggestions on our manuscript. Following the reviewers' comments and suggestions, we have carefully revised the manuscript. We have also addressed all the comments and questions, which are detailed below with our responses in blue. The line numbers indicated here are consistent with those in the clean version of the revised manuscript.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Comments and Suggestions for Authors:

The article “article_7517_0_art_file_140697_sh2vm2” can be considered for pro publication but with resolution of minor errors. In this study a meta-analysis is developed that measures the effect of various factors (hydrological, soil properties, crop type) on the NO₃ leaching process. The title should be modified, as it should give an understanding of the study area and contain the word meta-analysis. In the summary section, it fulfills its objective, there are only some inconveniences with some established quantities, which are not clear what the authors are referring to. Introduction: it fulfills its objective, there are only some inconveniences with some very old references, the use and establishment of certain acronyms. Methodology: The methodology is well established and reproducible. Results: In general, it is very good, however, there are some concerns in the results regarding the establishment of the random forest and SEM models, due to the fact that a variable used in the calculation of LF is used, so this variable presents the highest proportion or participation in the models, which should be omitted and these models should be re-established. Discussion. All very well, only the discussion regarding the models should be modified, because of what was mentioned above. Conclusion: No conclusion of the study is presented.

We sincerely appreciate the reviewer's positive comments and her/his recognition of the importance of our work. We also thank the reviewer for taking the time to provide insightful and constructive comments that are helpful for us to improve this manuscript.

For the Conclusion section, we added subheadings and revised the conclusion to address your concerns and to better reflect on the key findings and implications of our study (see Lines 332-345).

Detailed responses to the other comments of reviewer can be found below.

Specific comments are shown below:

– - L1. The name of the article should mention or contain the area of the study and the word meta-analysis.

Thanks! We have added the area of the study and the word meta-analysis in the title (see Lines 1-2).

– - L34-35. It is not clear why the amounts should be established in this way, unless they are three different regions (which should be established) or three different crops (which should be established).

We thank you for pointing this out. We have added the explanation for the reason why the NO₃⁻ leaching were separated into different regions or crops (see Lines 32-33).

– - L35. The meaning of the acronym IPCC should be established.

Revised as suggested (see Line 38).

– - L36. Is the term Tier-1 correct?

Yes, this is a technical term from the 2019 Refinement to the 2006 IPCC Guidelines, which is the basic method, neglecting different regions or crops. We have provided an additional explanation for Tier 1 method in the Introduction section (see Lines 57-60).

– - L57. Only use the most recent reference, unless it is very important.

Thank you for pointing this out. We have deleted the old reference (see Line 44).

– - L60-61. Same comment as above, check this throughout the text.

Thanks! We have reviewed all the references throughout the manuscript and deleted the old ones, except for eleven important and classic references (i.e., Bakhsh et al., 2010, Di & Cameron, 2002, Hastie et al., 2005, Hu et al., 2011, Koricheve et al., 2013, Kramer et al., 2006, Qiu et al., 2011, Rosenberg et al., 2000, 2005, Syakila et al., 2011, USDA, 1999 and Whittaker et al., 1962) (see the References section).

– - L74. Up to this moment the IPCC abbreviature is established, while it has been used before.

The IPCC abbreviation here is established for the first time in the main manuscript, which should be excluded from the abbreviation use in the Abstract, so we kept it (**see Lines 57-60**).

– - L170. I do not believe that it is correct to use the variable leachate amount in the prediction of LFs, because these two variables are dependent on each other, since the amount of N leached was used in the calculation of LF (equation 1) and practically in my opinion it is the same. It is necessary to make the analysis without taking into account this variable and observe if they fit the model in the same way.

Sorry for the ambiguous description of the leachate amount in the manuscript. Leachate amount used in the random forest model refers to the volume of water percolating through the soil profile, rather than the amount of N leached used in equation 1. We have replaced “leachate amount” with “leachate volume” throughout the manuscript to make it clearer (**see Lines 81, 141, 155, 158, 163, 219, 220, 377, 406, 408, 463, 487, 761 and 780**).

– - L177. Same as the previous comment.

Following your above comment, we have revised the term to make it clearer (**see Lines 81, 141, 155, 158, 163, 219, 220, 377, 406, 408, 463, 487, 761 and 780**).

– - L233. Review the use of this variable in the models.

Thank you for pointing this out. We have replaced “leachate amount” with “leachate volume” throughout the manuscript for clearer clarification based on the above comment (**see Lines 81, 141, 155, 158, 163, 219, 220, 377, 406, 408, 463, 487, 761 and 780**).

– - L382. Mention the depth.

We have added an example that measured multiple depths (**see Lines 364-365**).

– - L405. How many sites are we talking about?

We have added the number of the sites that lacked control treatment in the revision accordingly (**see Lines 393-395**).

– - L467. Establish the term SEM

Revised (**see Line 453**).

Reviewer #2 (Remarks to the Author):

This study from Wang and colleagues explores a bottom-up approach to derive the fractions of N inputs (manure and synthetic fertilisers, but also other organic amendments) that is leached below the root zone. To do so, the authors compiled data from 2602 field observations from 198 studies, including important covariates such as leachate volume and other pedoclimatic variables. The authors distilled those data using a meta-analysis, structural equation modelling and the machine learning random forests. The study is a valuable addition to refine national inventories, not only in terms of N leaching but also, by extension, of its indirect N₂O emissions. The usefulness of this study is to be commended given the authors provide a computationally inexpensive way to upscale N leaching estimates, which require the use of complex process-based models that often fail to precisely simulate leaching dynamics. The data and models generated in this study can provide a key tool for a wide range of stakeholders across the globe – even acknowledging the biases within the scientific literature that tend to focus on specific countries/regions.

As a potential user of such data/models, I am certain that the authors can (and should!) go a step forward and provide a fully spatially explicit simulation of annual leaching fractions in agricultural land, while openly sharing the data and the models. This would considerably increase the study's relevance and attractiveness. Other than that, the paper reads well and I only have minor concerns. Please see my comments below.

We sincerely appreciate the reviewer's positive feedback and her/his recognition of the importance of our work. We also thank the reviewer for taking the time to provide insightful and constructive comments that are helpful for us to improve this manuscript. Detailed responses to each of the reviewer's comments can be found below.

Comment 1: How can you aim to refine inventories if you are not being transparent and following the FAIR principles? To do so, I recommend the following:

- Sharing the dataset and statistical/predictive model that others can use to make predictions

Thank you for pointing this out. We have supplemented the full dataset and list of references for publications used in our study in the DRYAD repository (<https://doi.org/10.5061/dryad.612jm64dr>, since the article is still under peer review, this link has not been activated, the dataset can be browsed through http://datadryad.org/stash/share/3h9HNxvzoCwTll0Ld26EHDf6vaoCxaaqIGAH49vO_Rqg at present), as well as the predictive model (see **Data availability and Supplementary Fig. 8**).

- Derive spatially explicit (10km x 10km) and nationally leaching factors emulating FAOSTAT – this opposes lines 86-87 and l352 as you did not provide spatially explicit leaching factors.

- Providing a comparison, for those spatial scales, between IPCC (i.e., FAOSTAT) and your leaching fractions to see where the largest disagreements area

All geospatial/statistical data are already available (e.g., crop maps from MapSPAM, N inputs from synthetic fertilisers and manure from Tian et al 2022 or FAO, crop-specific N fertiliser inputs from Coello et al 2024, soil data from OpenLandMap, irrigation from Mialy et al 2024). Leachate is trickier but you could use groundwater recharge, which can still be considered a proxy, from Wan et al 2024 or, alternatively, from AQUASTAT at the national level.

Tian, H., Bian, Z., Shi, H., Qin, X., Pan, N., Lu, C., Pan, S., Tubiello, F. N., Chang, J., Conchedda, G., Liu, J., Mueller, N., Nishina, K., Xu, R., Yang, J., You, L., and Zhang, B.: History of anthropogenic Nitrogen inputs (HaNi) to the terrestrial biosphere: a 5 arcmin resolution annual dataset from 1860 to 2019, *Earth Syst. Sci. Data*, 14, 4551–4568, <https://doi.org/10.5194/essd-14-4551-2022>

Mialyk, O., Schyns, J.F., Booi, M.J. et al. Water footprints and crop water use of 175 individual crops for 1990–2019 simulated with a global crop model. *Sci Data* 11, 206 (2024). <https://doi.org/10.1038/s41597-024-03051-3>

Coello et al 2024 [2406.10001] Global Crop-Specific Fertilization Dataset from 1961-2019 (arxiv.org)

Wan et al 2024 <https://doi.org/10.1029/2023WR036182>

Thank you for these insightful comments and valuable databases! We used the Random Forest models and the recommended gridded dataset to predict the spatially explicit NO_3^- LFs at a resolution of $0.5^\circ \times 0.5^\circ$ (this resolution is set to match the resolution of groundwater recharge data). We also compared our spatially explicit NO_3^- LFs and the associated NO_3^- leaching with the IPCC Tier 1 method. We have added corresponding methods, results, discussion and figures in the revised manuscript (**see Lines 126-133, 181, 186-197, 258, 265-267, 270-271, 277-278, 292-295, 475-478, 482-496, 501, 508-514, Fig. 3, Fig. 7, Supplementary Fig. 1, Supplementary Fig. 4 and Supplementary Fig. 8**).

Still to this end, given the title emphasis on inventories, the study deserves a “policymaker” figure – one that easily synthesizes the key findings (i.e., confirms how IPCC tier 1 is often useless). Figure 6 could be the figure but requires some changes. For instance, (i) I do not think the cropland adds much, (ii) the regions are unclear (e.g., SEA N leaching is in South Asia). Concerning the strengths, you omitted a very useful thing: bottom-up approaches typically use process-based models, which are very computationally intensive and require a high degree of know-how to setup and parametrize models. Your work distils research data that can be used at different spatial scales: from a grid (provided you follow Comment 1) to the global, while also providing a computationally inexpensive and somewhat transparent tier 3 leaching factors.

We appreciate this suggestion. Following your suggestion, we have revised Figure 6 (now Fig. 7) to provide more key findings of this study (**see Fig. 7**).

Comment 2: The overall structure of the discussion could be improved. For instance, rather than sparsely commenting on strengths (e.g., 1295-297) and limitations or recommendations (e.g., 1349-350), I suggest the authors to streamline the overall structure so it reads better and it provides more.

Thank you for this comment. In the Discussion section, we mainly addressed three issues: first, the factors affecting NO_3^- leaching factors; second, the total amount of cropland NO_3^- leaching from global synthetic N fertilized soils and potential mitigation efforts, and finally, a focused discussion on uncertainties and implications for future research. We reorganized the content of the Discussion section and added subheadings to make the expression of each part clearer in terms of this comment (see Lines 203, 252, 314 and 332).

Minor comments

L141 – Did you run a significance test? If so, provide the required metrics (e.g., p-value, degrees of freedom). Otherwise, reword (e.g., substantially). Please check this across the MS.

Yes, we conducted significance test for all analysis in our study. For more precise expression, we have reworded the related description throughout the whole manuscript based on this suggestion (see Lines 136-138, 145-148, 159-162, 170-173 and 478-479).

L381 – I am curious whether using the sampling depth of N leaching measurements as a covariate could be beneficial. As I am unable to explore the dataset you compiled, I am left wondering if it would be possible, maybe in a future study, to decompose N leaching for different soil depths. This would be particularly relevant for drylands as soils tend to be less “thick”.

Thank you for this insightful comment. We checked the literature again from which we compiled the *sampling depth* dataset and used it as a covariate for predicting NO_3^- leaching factor in the analysis in this study (see Lines 148-150, 159-162, 379, 409, 412-413, 464, 486-488, 761, Fig. 2, Fig. 4, Supplementary Table 1, Supplementary Fig. 4 and Data availability).

Furthermore, disaggregating water inputs into precipitation and irrigation could likely be beneficial to better predict N leaching in dry regions where percolation events in rainfed systems are essentially a few days per year. However, I understand doing so can be largely outside of your control since it would depend on how descriptive the studies are.

As a reviewer and someone who has somewhat done similar things, I am aware of the limitations of what I highlighted above. Doing so, however, would potentially lead to an even larger attractiveness to a larger audience.

Thank you for this great suggestion. We have disaggregated water input into precipitation and irrigation and used these two variables to predict soil NO_3^- leaching in the revision accordingly (see Lines 154-157, 159-162, 376-377, 460-465, 491-492, Fig. 4 and Data availability).

L391-395 – This sentence is a rather important part of the replicability of the methods but it is written in an unclear way. Please specify whether all “covariates” were extracted from the 198 studies or if any other external geospatial sources were used.

We have provided detailed information about additional relevant data (see Lines 374-381).

L405-406 – Given this could introduce some biases in the model, please specify the number of exact cases where you used the average “background” N leaching.

We have added the number of the sites that lacked control treatment according to the suggestion (see Lines 393-395).

Response to reviewers

Communications Earth & Environment manuscript 7517_1_art_file_171889_snjvyj

Title: Accounting for differences between crops and regions reduces estimates of nitrate leaching from nitrogen-fertilized soils

We sincerely thank the editor and reviewers for their critical reading and constructive comments and suggestions on our manuscript. Following these comments and suggestions, we have carefully revised the manuscript. We have also addressed all the comments and questions, which are detailed below, with our responses in [blue](#). The line numbers indicated here are consistent with those in the clean version of the revised manuscript.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Comments and Suggestions for Authors:

The article “article_7517_0_art_file_140697_sh2vm2” can be published because the comments made in round 1 were corrected.

[We are grateful to you for the constructive suggestions. Your suggestions are of great help in improving the quality of our manuscript. Thank you so much.](#)

The specific comments and modifications made by the authors are shown below:

- Modifications made
- - L1. The name of the article should mention or contain the area of the study and the word meta-analysis.
- - L2. Modification made.
- - L34-35. It is not clear why the amounts should be established in this way, unless they are three different regions (which should be established) or three different crops (which should be established).
- - L33, 37. Modification made.
- - L35. The meaning of the acronym IPCC should be established.
- - L39. Modification made.
- - L36. Is the term Tier-1 correct?
- - The authors mention that the term is correct, and that they also included information in the introduction section L57-60. But I can't find anything related to this term in those lines. I found the information up to line 73.
- - L74. Up to this point the IPCC brevity is established, while it has been in use before.
- - L39. Modification made.
- L170. I do not believe that it is correct to use the variable leachate amount in the

prediction of the LFs, because these two variables are dependent on each other, since the amount of N leached was used in the calculation of LF (equation 1) and practically to my way of seeing it is the same. It is necessary to make the analysis without taking into account this variable and observe if they fit the model in the same way.

- L177. Same as the previous comment.
- L233. Review the use of this variable in the models.
- The three previous comments were answered satisfactorily.
- L382. Mention the depth.
- L417. Modification made.
- L405. How many sites are we talking about?
- L445. Modification made.
- L467. Establish the term SEM
- L509. Modification made

We appreciate the comments made by the reviewer #1 and are happy that we were able to address all her/his questions.

Modifications not made

- L57. Only use the most recent reference, unless it is very important.
- L60-61. Same comment as above, revise this throughout the text.
- In the two previous comments, it is not clear which of the references was left out.

Thank you for pointing this out. For the first comment, we have deleted the Lin et al. (2001) (**see Line 44**); for the second comment, we have reviewed all the references throughout the manuscript and deleted Gu et al. (2013) and (2015) (**see Line 44**), Thomas et al. (2005) (**see Line 63**), Vitousek et al. (2009) (**see Line 65**), Wang et al. (2014) (**see Line 243**), Berdanier & Conant (2012) (**see Line 265**) and Aghaalikhani et al. (2012) (**see Line 304**) in our first revision.

Reviewer #2 (Remarks to the Author):

I congratulate the authors for such detailed revisions over such a short period of time. I am very pleasantly surprised with the new additions to the manuscript. It is now much stronger and more appealing to a wider audience: now it can be used for national inventories but also for spatio-temporal modelling of N leaching hotspots. From my end, the study is now ready for publication.

We appreciate the comments made by the reviewer #2 and are happy that we were able to address all her/his questions.