Answers to reviews on

Soil carbon management in large-scale Earth system modelling: implications for crop yields and nitrogen leaching

Below are the questions and suggestions in italic and blue followed by answers and where the changes are made in the manuscript.

Answers to M. Braakhekke

Thank you for the comments and suggestions for our manuscript. The manuscript became much clearer to follow in many of the sections based on your suggestions.

- I find the introduction, though interesting, a bit long and repetitive. On the other hand, the part on the current study is relatively short. Since many readers prefer to skip the methods section it is advisable to give a bit more information about what was done.

We have revised introduction section (shortened and restructured) and methods section (adding detail) in response to comments also made by the other reviewer and hope these changes address also your concerns.

*- p 1053, l 9-11: "Moreover, the conversion of N to plant-available forms is reduced in untilled soils and can thus lead to lower crop productivity, which could in the long run decrease the soil's ability to store water and nutrients because the reduced release is partly counterbalanced by a reduced input of new organic material." I find the second part of this sentence (from "which...") a bit strange and speculative, since (to my understanding) no-till farming is applied partially to *improve* water and nutrient retention. I also could not find this in the included reference (Lal, 2004a).*

We have now changed the sentence to read as follows (page 5, line 21-25):

Moreover, the conversion of N to plant-available forms is reduced in untilled soils and can thus lead to lower crop productivity. Although no-till farming is applied partially to improve water and nutrient retention, the reduced crop productivity and thus reduced input of new organic material could also decrease the soil's organic content in the long run (Lal 2004b).

The Lal reference was wrong, thanks.

- p 1054, l 11-14: It took me several times before I understood this sentence. Please consider revising it.

We decided to split this sentence into two to make it easier to read (page 7, line 2-5).

- Section 2.1.1: please include a brief explanation of the term "developmental stage".

A sentence describing developmental stage is added to the model description on page 8, line 14-17.

- p 1057, l 3: how was the manure application derived from the mineral N fertilizer? By assuring that the total amount of N is the same?

This has been clarified, and now reads (page 10, line 5-7):

The amount of manure is derived using the mineral N application rate, but applying the increase in the metabolic and structural SOM pools, rather than the mineral N pool, with a C:N of 30.

This means that 30 units of C are also added for every N.

- p 1058, l 2: this section suggests that the effect of crop residues on soil evaporation is represented in LPJ-GUESS, while to my understanding it is not. Please make this clear.

We clarified this by adding the following sentence to the end of the paragraph (page 11, line 6-7):

While this affects soil C content, the effect of crop residues on soil evaporation and hence soil water content is not represented in the model.

- Section 2.2: 1) this section is somewhat unclear since descriptions for the different simulation experiments are mixed. Please include a brief but clear overview of the

simulations that were performed, possibly identified by labels, which can be referred to later. 2) Please indicate briefly why the CMIP5 simulations started from 1850, while the CRU simulations started from 1901. 3) For the future simulations based on CMIP5 it is not clear if the GCM output was used for the complete simulation or for the future part only (with CRU being used for the historic part)
We made some clarifications regarding the different time periods, throughout the section and added a summary table (Table 1) to make the different experimental set-ups easier to compare.

- p 1059, l 27 – p 1060, l 1: This sentence is not completely clear. Do you mean that a longer transition period for land use would reduce the spin up to such an extent that steady state is not reached for the natural vegetation?
This sentence was a bit vague, we have now changed it to read as follows with an additional sentence to clarify the difference in the transition period length (page 12, line 27 to page 13 line 5):
During spin-up, cropland fraction was linearly increased from an assumed baseline of zero at 1750 to the first historic value (1901 for CRU and 1850 for CMIP5). The number of years for this transition (150 years for the CRU-based and 100 years for the CMIP5 simulations) was chosen to ensure that the soil C and N pools of the natural vegetation fraction of each grid cell reached steady-state by the beginning of the transition period. The different period lengths were chosen to make the simulations comparable in terms of land-use change prior to 1901.

- p 1060, l 10: The WISE dataset comprises both a collection of soil profiles around the globe and a global gridded product derived from this. It seems that here you refer to the latter, while in p 1060 l 14 you refer to the former. Please clarify this.
Clarified in the manuscript on page 13, line 17-18.

- section 2.2.1: 1) this section is somewhat unclear. I think a few introductory sentences about what was done would be helpful. Further: 2) Where results from a single simulation compared to both the the WISE soil carbon data and the data from Stockmann et al.? 3) Also, I do not understand the classification in to climate zones yields 200 cells per zone (so 800 in total?), out of 60,000 grid cells globally.
- p 1060, l 16: what does the "1000" in parentheses refer to? The number of columns per grid cell?
1000 refers to the number of grid cells with soil cores that were taken from croplands and 200 of these were discarded because they were located outside of the simulated grid (e.g. on a small island) or in the boreal zone. The section is updated (13, line 17-22) addressing also the comments under (1) and (2).

- Section 2.2.2: 1) The wording in this section suggest that some sort of optimization procedure was used to determine the management for optimal soil carbon sequestration for each grid cell. However, from what I understand, this is not the case; instead the results from the management experiments where combined by selecting for each grid cell the optimal management for soil C. Please indicate this clearly. 2) Please explain in this section the labels of the simulations as used in Table 1, and elsewhere in the text
#1 clarified in the text (page 14, line 22), #2 the names are expanded in text (page 14, line 14-15) and also in the caption of the table (now table 2).

- Table 1: what does "scenario" (last line) mean?
Clarified in table caption (now table 2).

- Section 3: Unless I misunderstand, the correlation coefficient diagnostic used to eval-uate model fit to observations does not provide information about model bias (i.e. deviation from the 1:1 line). If this is the case please consider complementing it with another metric such as the (normalized) root mean square error.
True, we complemented with RMSE values on page 15, line 5-10.

- Fig 2. This graph is somewhat unclear. There's quite a few lines and the shading overlaps. I would suggest to replace the lines with bar graphs with errorbars for selected years. Also, I personally think an anomaly graph, i.e. the change in soil C relative to a specific year, is more informative than the rate of sequestration. However, I understand that this would complicate the comparison with the Stockmann et al. data.

We updated the graph, we kept the lines (mean and 2SD) and removed the shading. A bar graph would not give the temporal dynamics, which are important to visualise the decline in C sequestration over time.

- Section 3.2: The (long term) response of C sequestration to management options is much lower than wat is reported by Stockmann et al. However, this is not mentioned in the text nor could I find a discussion on this in section 4.

We added a discussion on this in section 4.1.1 on page 20, line 3-10.

- p 1064, l 5-10: this is a quite remarkable result since croplands generally have lower soil C. I did not see this clearly discussed in the section 4 though. Further, could this also be related to the fact that the land use conversion in the simulations started only on 1750, thereby not giving the soil C in croplands enough time to decrease?

As mentioned in the Results, some of these areas such as Egypt, with large input of both irrigation and fertilisers, the soil C is higher and also that some of the major pasture areas have higher C densities than the PNV that they replaced, consistent with observations (Guo and Gifford, 2002). But also the the reason you mentioned, that the simulated time under agriculture is too short could be a good reason for this discrepancy. We updated the discussion on page 19, line 10-14.

- Section 4: only three functional types are used to represent the full global spectrum of crops. I understand this was a necessary simplification but it likely adds considerable uncertainty to the results. However, it is not mentioned in the discussion. I'm sure it's possible to say something about the crops and regions for which this may lead to incorrect results (rice comes to mind).

We added the following sentence to the discussion on yields (page 22, line 24 to page 23, line 3):

The modelling approach taken here to represent all crops globally with three CFTs, introduces an uncertainty in the estimates of global food production and thus also on the carbon cycle.

We expect that this would be most prominent for crops whose growing seasons, water requirements, or physiology differ substantially from the functional types used here, e.g. regions where rice (South East Asia) or tubers (Africa) are are grown over a large portion of harvested area.

- Section 4.1.1: could the low predicted response of C sequestration to management also be caused by the fact that soil C in croplands is over estimated due to the short period of land use conversion?

In some regions this may be the case, but it would require in-depth testing against for instance long term experiments to confirm and evaluate the response since this would be affected also by the available N in the soil. We have now made mention of this in the manuscript.

- p 1066, l 23-25: please consider revising this sentence

We revised the sentence by removing the word global (page 20, line 10).

- p 1067, l 6-8: this sentence is difficult to follow. Please consider revising.

We revised the sentence to read as follows (page 20, line 21-22):

The authors found that CLM without accounting for tillage practices underestimates the emissions caused by agricultural practices.

- Appendix: please include units of the allocation variables, and explain the DS acronymn

We added explanation in the model description (page 8, line 14-17). As DS is without unit, the parameters are also unitless.

Technical corrections

- In many places citations are completely (authors + year) enclosed in parentheses where only the year should be enclosed. I suspect that the authors used latex and wrote "ncitep{}" where "ncitet{}" was intended.

Thanks for spotting this, we now believe that they are correct.

- p 1061, l 13: I assume you mean "Table 1" rather than "Table 2.1.1"

Yes, corrected.

We have also corrected the following:

- p 1063, l 18: consider replacing "over" with "for" in "competition over available N"

- p 1064, l 12: please insert "in" before "1996-2005", or similar modification Table 4, caption: please remove comma in "Also listed are,"

- Fig 2, caption: do you mean "vertical", instead of "horizontal"?

- Fig 5, caption: consider replacing "on" with "of" in "response on"

Reply to W. Wieder.

Thank you for the comments and suggestions. We have addressed most of them with changes in the manuscript. We believe that the manuscript became much clearer in the parts that you gave comments on.

1. How well can the model replicate temporal changes in crop yields through increases in cultivated land + changes in management practices? Can it capture anything like the Green revolution? Maybe it's documented elsewhere, but there's never any quantification of the climate change of elevated CO2 effects that underlie all of the results presented here? Step back and acknowledge what you've accomplished (adding crops and management to a global land model that represents terrestrial C-N biogeochemistry)!

It seems important to document (and validate?) some of the basics.

I ask because the study seems to be motivated by the need to increase agricultural productivity to meet food, fiber, and fuel needs for a growing population (Introduction), but a hasty read would conclude that productivity has declined over the historical period and remains flat over the next century (Fig. 5c). I realize that's not the focus of this study, but some basic information about projected yields under a business as usual (control) management practice over the historical period and future scenarios would be helpful.

In Olin et al. 2015, the N fertiliser response for wheat was evaluated. We have added text to the beginning of the discussion section to provide some more detail. There is also a short reference to that work in section 4.2. (p 22, line 1-4). In this manuscript, Fig. 5C provides an overview of relative response on yields when a management other than the "standard" is considered. The suggestion to also check our simulations against the FAO statistics over the last few centuries is of course a very good one, and we have added a figure (Fig. B2). Related text was added to section 4.2:

We also compared historical global crop yields against numbers found in FAOSTAT . Yields in the early 1960s were similar (ca.1.5, t ha-1yr-1) but increase in yields were faster in the reported statistics compared to the model output. Whether or not this related to a missing process in yield simulations (e.g., lack of double cropping (Waha et al. 2013)) or uncertainty in the fertiliser hindcast product used needs to be explored in future work.

2. Second, greater context for the management practices tested here seems warranted. How may these results inform suggested agricultural management practices at large (i.e. National) scales? I realize that's not the aim of this class of models (section 4.1)- but I know of no other tool that allows us to start asking important questions about the long-term, broad-scale impacts of land management decisions. As in #1, with quick read one could conclude that cover crops, no-till, etc. cost more than they are worth because they decrease yields and don't do much to sequester additional C, besides nobody outside of Europe is really doing anything about N management so who cares?

Perhaps this is the intended message? If not, what are the uncertainties or processes that need further development in the model.

It is our belief that it would be wrong to use the model in its current state to make any suggestions on land management decisions apart from these very general conclusions drawn here. Detailed local knowledge on current practices are needed to parameterise and interpret output from these models in order to be able to derive conclusions at the national scale. The short-comings and uncertainties of the model are now discussed in the Discussion and also in the Conclusions.

Specific comments Abstract: The abstract is very hard hitting and direct. A sentence or two about the larger questions being asked may be helpful to introduce the topic for readers. Also the organization of the abstract makes me wonder what the focus of the paper will be (agriculture, N management, or DGVM). The results highlighted on P 1047, L 5-8 regarding effects of land use dynamics make me think it's on the DGVM, but to my reading the agriculture x nitrogen management questions are much more interesting

Thanks a good point. We have revised the abstract and placed the work into a larger context (see track changes indicated in the revised abstract).

Introduction: Is a bit long, but I feel like it nicely provides an overview for the questions being asked here. If anything greater focus could be given to the novel aspects of this work (P 1051, L 23) and the full biogeochemical estimates that the study provides for tradeoffs between agricultural productivity, terrestrial C storage, and potentially N effects.
By the bottom of page 1051 through 1053, the organization (any my attention) started to waiver. Yes, agricultural management practices present a host of challenges and uncertainties on biogeochemical fluxes in the field and in models. The text provided is a nice review, but not really focused on the management practices that are the focus of this manuscript. Could revisions note challenges related to land use tillage practices, harvest residue removal, etc- but then focus on the experiments presented in this work (N management and effects on yield & biogeochemical cycles)?
One glaring omission in this work is estimates of N2O emissions. I'm not suggesting they be included in this study, and recognize that such estimates would be highly uncertain. Previous work, however, demonstrates that the climate consequences of N2O emissions may largely offset C benefits of fertilizer application (Butterbach-Ball et al. 2011; Pinder et al. 2012, 2013; Zaehle et al 2011). In my mind this limitation of the study should be directly addressed.
This is of course true, and indeed N2O was intentionally left out, as this is development work to be included in the model (a statement for clarification has been added).
Re-reading the introduction and trying to take into consideration the reviewers comments we have shortened and restructured the abstract – hoping that the logic is now more easy for the reviewer to follow.

Methods & Results: Lots of minor questions listed below that generally ask for greater clarification or details about the model approach and assumptions related to aspects of the work presented here (e.g. N leaching, agricultural model, management, etc. For example, I feel a simple schematic of the soil biogeochemical model (similar to the one in Smith et al. 2014) that highlights where in the model different cropland management practices are used would be helpful. Moreover, aspects of the experiment and results need to be more transparent (e.g. stating depth of SOC observation, total N application rates, etc).
We have added additional sentences describing the SOM dynamics in the model (page7, line 15-24). The N application rates are from data sets which vary both in space and in time and thus in our view providing a single number would not add value. In appendix, the global N application is shown. We had forgotten to add a reference for this figure in connection to the references of the data sets, thanks for spotting this, it has now been added (p 12, line 14). The depths of the SOC are reported in the manuscript, 1.5 m, see page 13, line 19

The decision to use a fistful of different climate simulations Cru and four CMIP5 climates under two future scenarios makes this manuscript harder to follow. In my mind this is a paper that documents & evaluates the crop and management featured developed in LPJ-GUESS. Layering on the nuances of multiple climate simulations seems overly ambitious and potentially distracting. Moreover, potential intermodal variation driven by different GCMs is normalized away in the results (Fig 5), making me wonder why different GCMs are really necessary?

Thanks for these comments – since part of the model developments and manuscript are published in Lindeskog et al (2013) and Olin et al (BG, 2015) we felt it would be important to also add some aspects of application to the present work. Applying different GCMs allows us to show some of the associated uncertainty, given in figure 5, as shades surrounding the mean. The effect is only visible for cover crops, because the difference between the GCMs affects the vegetation more than the soil processes. There might also be non-linear effects that are not apparent when only using a single model.

Discussion & Conclusion I'd like a more prospective, positive, reflective tone throughout the discussion. Right now certain sections (e.g. 4.1) reads more like an apology for wasting the readers time than a thoughtful discussion of the results presented, their implications, and limitations. Similarly section 4.2 starts off by summarizing data from another paper by the same author group- but not on data presented here (see general comment #1, above)

We thank the reviewer for encouraging us to be more positive about our own work! We moved the last sentence to the beginning of the paragraph and changed the first sentences in section 4.1 (page 18, line 17), section 4.2 does not start of by summarizing data from another study.

Technical corrections P 1050, L 3-6. At first a first read the math behind these statistics didn't add up, how can we increase cropland more than 3 fold, when it's already 35% of the land surface? Then I realized that crop AND pasture made up this value. I wonder if it's worth focusing more narrowly on crop representations here, since that's the focus of the model results presented in this paper?
P 1050, L 25-29. Again the statistics aren't used clearly or effectively. Maybe get rid of the 70% value?

We removed the sentence on cropland expansion; although interesting, we agree that it is not really the focus of this paper. The sentence about N management in Europe was changed to the following (page 4, line 9-11): Even in Europe, where

environmental regulations are relatively advanced, a large portion of the population live in areas with high levels of nitrate in the drinking water (Grizetti, 2011).

P 1054, L 27-27. As NO3 leaching is a major finding of the work presented I find the description of how the model handles N transformations, competition (between soil immobilization and plant uptake) and losses astoundingly brief. I'm not suggesting that work presented by Smith et al. 2014 (Appendix C), be repeated. It it's worth briefly summarizing the approach LPJ-GUESS takes for readers not familiar with the model.
The SOM dynamics description is expanded; page7, line 15-24.

P 1055, L1-20. As above, for readers not familiar with the crop model applied in LPJGUESS there a lot of uncertainty here. For example, I'm familiar with some models that have a grain fill stage of their life-cycle, that is eventually harvested (C & N pools). The idea of 'heat sum requirements' is not intuitive. Also, What happens to the harvested fraction of crops (i.e., where does the C and N go)? What happens to crop residues?

The following clarification is added to the model description (page 8, line 20-23):

At harvest, the grains together with a portion of the residues are removed from the field. In the model, harvest is not perfect, 10\% of the grain and residue C and N is left as litter and decomposes, see section *Residue removal* below.

The crops in LPJ-GUESS have an explicit C allocation scheme, which is described in detail in Olin et al. (2015), which is also mentioned in the manuscript.
Heat sums are explained in detail in Lindeskog et al. (2013), we added a short explanation on page 8 line 5

P 1056, Tillage. Century has a well-tested and described tillage scheme that has been applied in agricultural models (Levis et al. 2014). I'm surprised the authors seem to set out on their own to parameterize this model with what appears to be little validation.

We agree with the comments that the implementation in Levis et al. (2014) has important advantages to represent the spatial variation in current tillage practices. However, in our views, many of the details of tillage practices, and their parameterisations, are very uncertain for present day and even more so in the future. This was our reason to apply a more simple approach, which we perceive as being more transparent.

P 1056, N application. Nowhere are total fertilizer amounts stated to illustrate if N application rates are simulated realistically. A reference is eventually provided (P 1059) and even displayed (Fig B1, but N application is never referenced in the text), but approximate fertilizer application rates would be helpful along with this reference. Between sections 2.2, 3.1, and Fig B1, it's not clear what data are being used, Zaehle et al. 2010, Elliott et al. 2014). Please clarify what data were used to force these simulations. See also comments related to Table A2 (below).

N application rates are not simulated, they are taken from the input data sets. The reference for figure B1a had been omitted, which naturally caused confusion; it is now added (see response to comment, above).

Page 1059, lines 13-23 (page 12, lines 14-24), describe that the Zaehle et al. (2010) data set is used for all simulations and extended for future simulation with the Stocker et al. (2013) data set; in addition (section 3.1) we also explored a second global-scale N fertiliser data set from Elliott et al. (2014).

P 1060 L 16. What is 1000, the number of grid cells averaged?

The number of simulated grid cells, corrected, see page 13, line 21.

P 1061 L 3, why did management only start in 1990? I guess you have to start somewhere, but

We have added the following to the description of the simulations (page 14, line 7-9):

To be able to show the effect of the studied management practices during the historic period were these enabled starting from year 1990 and kept throughout the remaining simulation period.

Section 2.2.1, 3.2, & Table 2. References don't match Batjes 2005 (text) vs. 2014 (table). Also text and table should clearly state the depth of sampling for observations and models (which I don't think match...)

The numbers in table 2 (now table 3) are taken from Batjes (2014) and for the site comparisons Batjes (2005) is used. Batjes (2005) is the reference both for the gridded and the soil column data, this has been clarified in the text (page 13, line 17-18).

P 1064 L 5-10, wait, the model does simulate pastures? How, all of the description in the methods related to crop management? Where are pastures locaed? How are they different from the natural vegetation they replace? There really is a lot going on here that's hard to keep track of...

We have added a brief description on how pastures are managed (page 8, line 3-5) and a more explicit formulation that include pastures (page 12, line 25-27):

Land cover information was adopted from (Hurtt et al. 2011), with data on historic and future cropland, pasture and natural vegetation. The additional land cover classes in the data set: forested, rangeland and urban classes were treated as natural land cover.

P 1064 L 11-15, I apologize, but at this point my head is really starting to spin. This analysis (and Fig. 4) suggest how much additional C could be stored in agricultural soils using different management practices over a 30 year period using management practices that optimize soil C storage? If so, what's the number? If we radically alter management practices, what kind of payoffs for C storage does this analysis suggest? More broadly, all the abbreviations of different management practices (e.g. Fcc, Fnt) are convenient for the authors, but impede understanding from readers who are trying to keep track of a complicated story.

The number is in the caption of figure 4, we have added a reference to figure 3 where the trade-offs are shown and also a reference to the table header for an explanation of the abbreviations. We updated the caption for figure 4 as follows:

Optimal carbon sequestration practice (F_opt) around the year 2000, as simulated by LPJ-GUESS, based on the different management practices and trade-offs shown in Figure 3 (see also Table 2 for the abbreviations). The standard setup (F_std, blue) was selected when none of the other managements gave an increase in the amount of carbon sequestered. The C sequestered compared to F_std for choosing the optimal practice in each grid cell is 7.7 Pg C from 1750 to 2000, the reduction in global N leaching for best C sequestration practices is 11.9 Tg N yr-1.

P 1069 L 19-24. Do either of the N fertilizer schemes align with actual agricultural practices? I thought the trick w/ N management and precision agriculture was assessing likely N demand early in the growing season so appropriate amounts of N could be applied.

These are important points, but in our views a global model such as LPJ-GUESS would (certainly in the current configuration) not be a tool to be applied to questions such as precision agriculture . The distribution of N throughout the year, as a function of developmental stage had been developed and described in Olin et al (2015) due to the need to distribute annual N fertiliser sums (which is the format the data on N application is usually given) throughout the year in a way that is both sufficiently simple but still seeks to mimic widely found practices (see also *P 1092,* below).

P 1087 A description of the abbreviations used in Table 1 would be helpful in the table heading. Given that Table 3 references the abbreviations described in Table 1, this seems very important.

Good point, sorry for the oversight. Table (now Table 2) caption changed to the following:
Simulation settings used for the comparison of soil C, yields and N leaching with different agricultural managements. For implementation of full vs. moderate tillage, see Sect. 2.1.1.
In the row *scenario*, the managements that are included in the different scenario simulations are indicated. Abbreviations: NT; no-tillage, MN; manure application, CC; cover-crops and NR; leaving all residues.

P 1089. What is the time period for these simulations? Also, are leaching values for organic and inorganic N, if so, what is the relative contributions of these two loss pathways? If this is just for inorganic N, what are the DON exports estimates from the model?
At the moment we can not differentiate between organic and inorganic leaching. To provide clarification on these questions we changed the table caption to:
Modelled global, total land and cropland soil C and N stocks and total N leaching (inorganic and organic) for the time period 1996--2005, compared to estimates from literature. References for the studies and explanations of how some of the values were derived can be found in the notes of this table. See Table 1 for abbreviations.

P 1092 Table A2. I don't see any information for DS = 0.9 in the table. How does the fraction of N supplied relate to the timing of fertilizer application in the real world? It makes it seem as though the model needs lots of fertilizer at the end of plant growth, which seems unrealistic? As mentioned above, what is the total amount of N supplied to different crops in different regions? How does it compare with actual N application rates? How does N demand change in the future?

We added a description of DS=0.9 in the caption (see below), the fractions compare well to observed patterns, see Olin et al. 2015. DS=0.9 is still in the vegetative phase, and the application during this period makes the plant (in the model) delay senescence, thus prolonging the period with green leaves. The actual N application rates in the model is not based on N demand, the plants get the N that is supplied by the input data sets.

Changed caption:
CFT specific parameters of Specific Leaf Area (SLA), minimum C:N value of the leaves and the amount of the total N that is applied at the different developmental stages (DS), where DS=0 is sowing, and DS=0.5 is half way into the vegetative phase. The remainder of the total fertiliser application (not listed in the table) is applied at DS=0.9 which describes the vegetative phase just before flowering, see Olin et al. (2015) for more details.

P 1095, why is the change in N leaching axis reversed? This does seem intuitive, unless the upper right quarter or each graph is suppose to be a win-win scenario?

Correct. We changed the caption to read as follows:
The simulated relative response (%) of soil carbon to management options (Table. 2) compared to the standard setup, averaged for 1996--2005 and displayed as the global response (filled symbol) and per climatic region.
Note the reversed axes for N leaching (all axes display scales from reduced to enhanced ecosystem services with the upper right corner representing a win-win situation).