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Biogeosciences
Discussions

Interactive comment on "Insights on nitrogen and phosphorus co-limitation in global croplands from theoretical and modelling fertilization experiments" *by* Bruno Ringeval et al.

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General comments

We thank the Referee #2 for his/her comments. Answers are provided below in orange. The draft initially submitted is called "previous draft" while the one after taking into account the two Referees comments is called "new draft". The numbers of lines given below refer to lines of the "new draft" with track changes.

The current paper presents a theoretical study of nitrogen and phosphorus co-limitation in croplands, based on two common theories of nutrient limitation interaction, Liebig's

Law and multiple limitation hypothesis. They then use the co-limitation categories of Harpole et al (2011) to classify the results from these two theories. They then extrapolate these theoretical results to global scale to predict crop nutrient co-limitation for maize.

In my opinion the main problem of this study is its justification. Croplands are highly managed systems, often heavily fertilised and the authors fail to explain why a study of co-limitation is necessary in such a system. It is possible that the reason there are very few nutrient addition experiments in cropland systems is that the question is not relevant. The authors need to make a better case for why their approach is important and relevant in the study system.

First, nutrients limit the crop yield in many places in the World and it is relevant to investigate nutrients limitation in cropland at the global scale. Indeed, croplands could be over-fertilized but this concerns only few countries in the World. E.g. global P fertilizer application averages 10 kgP/ha/yr but with a large continental variability: ∼25kg/ha/yr in Europe vs ∼3kg P/ha in Africa (Liu et al., Journal of Industrial Ecology, 2008). MacDonald et al. (2011, PNAS) showed that negative soil P budget occurs for a large fraction of cropland at the global scale. As a result, literature showed that nutrient-limitation is a major limitation for croplands at regional (Guilpart et al., 2017 Schils et al., 2018) or at the global scale (see e.g. Fig.4 of Mueller et al., Nature, 2012). We modified the introduction to explain what it matters to study nutrient limitation in cropland at the global scale (L108-113).

Second, our theoretical framework aims to understand which nutrient limitation categories defined by Harpole et al. are prevented and which ones are more or lesss promoted by the interaction formalism assumed (LM or MH). We clarified the conditions (about the limitation of each nutrient when considered alone) required to make an ecosystem in each category as function of the interaction formalism assumed. For instance, we showed that synergistic co-limitation could occur even using Liebig's formalism (LM) and we provided the conditions required to be in that case: e.g. the ecosystem has to be N-limited in the control and the amount of N added in the fertilization experiment has to be enough to switch the ecosystem into P-limitation. This theoretical framework can be used in both natural ecosystems or cropland. We made clearer this rational of our work in the new introduction (L84-107).

Finally, it is true that our framework is particularly relevant for cropland as crossed single fertilization additions are not so common in cropland. Or at least, they can be exploited with difficulties. For instance, long-term trials (common for P in cropland) represent with difficulties surrounding limitations as the same application rate is used each year for many years in long-term trials. This prevents us from having a global picture of N and P limitation based solely on observations, contrary to what was done in natural ecosystems (Elser et al. 2007, Harpole et al. 2011). We clarified this in the new introduction (L117-136).

Guilpart, N. et al. Rooting for food security in Sub-Saharan Africa. Environ. Res. Lett. 12, 114036 (2017).
Schils, R. et al. Cereal yield gaps across Europe. Eur. J. Agron. 101, 109–120 (2018).
Liu, Y., Villalba, G., Ayres, R. U. and Schroder, H.: Global Phosphorus Flows and Environmental Impacts from a Consumption Perspective, Journal of Industrial Ecology, 12(2), 229–247, doi:10.1111/j.1530-9290.2008.00025.x, 2008.
MacDonald, G. K., Bennett, E. M., Potter, P. a and Ramankutty, N.: Agronomic phosphorus imbalances across the world's croplands., Proceedings of the National Academy of Sciences of the United States of America, 108(7), 3086–91, doi:10.1073/pnas.1010808108, 2011.
Mueller, N. D., Gerber, J. S., Johnston, M., Ray, D. K., Ramankutty, N. and Foley, J. A.: Closing yield gaps through nutrient and water management, Nature, 490(7419), 254–257, doi:10.1038/nature11420, 2012.

Furthermore, the upscalling method includes a large number of assumptions, which to their credit, the authors themselves discuss at length. However, the successive approximations made (single parameter for biomass allocation and tissue concentrations, simplified soil processes) lower my confidence in the results. The exact methods for the global calculation are only briefly described in the methods, with most of the details found only in the supplementary material and the assumptions of the study become

evident only when reading the discussion.

To reply to this comment, we:
- added some explanations in the Method section to be more explicit about the computation of the demand. In our mind, most important approximations were already given in Section 3.1 (e.g. "fixed" used at L287 means that the parameters are constant in space) but we have added few sentences to be more explicit (L300-303).
- moved the main caveats of the modelling approach from the Discussion to Section 3.1 (L335-347). This also helps to shorten the part of the discussion dealing with the caveats of the modelling approach following one comment of Referee #1.

Specific comments

Eq. 7 Is this a multiplication?

Yes, the dot of this equations mean a multiplication. We replaced it by a star in the new draft to be clearer. We also added one sentence to explain Eq.7 and Eq.8 at L184-186.

L 160 Does productivity here refer to vegetative biomass or yield?

In real fertilizing experiments (mainly performed in natural ecosystems), the response of the ecosystem is measured through a change in vegetative biomass. Thus, the change in productivity in our theoretical framework (Δpro) represents vegetative biomass.
When we applied our theoretical framework to cropland, we moved to yield as it is the variable of interest for cropland. Under the assumption of no plant adjustments (see our reply to above comment), the harvest index is constant in space and implies that our theoretical framework can be applied.
We added a sentence (L391-393) to clearly state that our theoretical framework is applied to yield in Section 3.3.

L 161 "pro is here expressed relatively to the potential productivity" But in the previous sentence is the response to nutrient addition, so the exact opposite

Δpro is expressed relatively to the potential productivity: it means that Δpro is not expressed in absolute change in productivity but is express through the ratio (change in productivity):(potential productivity). The aim is only to make Δpro vary between 0 and 1 such as the nutrient limitations (R_N, R_P, R_{NP}). We modified this sentence to be clearer (L203).

Eq. 9 - 11 this is a very big assumption and it is not justified by either a physiological explanation or references

It is true that Eq.9-11 are based on strong assumptions. But these assumptions are described in details at L222-242. The need to make such assumptions as well as what they imply on the results are discussed at the same lines.

L 208 "a reversed bracket used in an interval means here that the corresponding end-point is excluded from the interval" I think the correct mathematical notation is ()

Thanks, we modified this in both the Main Text and the Supp.Inf.

L 236 is there no leaching of N compounds?

Following Bouwman et al., 2011 and Bouwman et al. 2017, N input in the annual soil agronomic budget corresponds to deposition, fixation and fertilization. Output corresponds to N withdraw through N in crop harvest. In Bouwman et al., a surplus represents a potential loss to the environment (in particular through leaching) as there is no N accumulation in soils from one year to the other. The reasoning behind this approach is that leaching concerns only what Bouwman et al called surplus and occurs after the growing season.
Here, we used soil input minus volatilization to approach the N available for the plant. Following the reasoning of Bouwman et al., we assumed that leaching occurs only after the growing season while volatilization occurs when fertilizers are applied. We added one sentence to clarify this (L335-338).

Fig. 4 - It would help to define the categories again in the caption, so the reader doesn't

have to go backwards and forwards between the table and the figure

We have added the categories definition in the Figure 4 caption in the new draft version.

L 311 What do the values after the +/- sign represent? My first assumption would have been standard error or standard deviation, but some of the values are zero?

Yes, they are standard deviation, as stated in Table 2 caption of the previous draft. The standard deviation is derived from 1000 replications. Replicates differ between them by considering uncertainties in parameters at the basis of the computation of supply and demand (Text S5). We added this information in the Main Text (L374-377).
Some standard-deviations are equal to 0 as the error is very small at the global level but not at the grid-cell level (as we can see on Figures S4 and S6; and as explained in Text S6).

L 320 Check the grammar in this paragraph

Done

L 322 It's unclear what numerical fertilization experiments are

We modified it to "modelling fertilization experiments".

L 377 Since croplands are routinely fertilised, are there any fertilisation experiments as such?

Following our reply to the 1st main comment, we modified the Introduction in the new draft to clarify this point. Basically, we explained that crossed single fertilization addition are not so common in cropland as in natural ecosystems, or at least, they can be exploited with difficulties.

L 391 "organ concentrations derived from field experiments in stressed conditions" I don't understand why this information is buried in the discussion and the supplementary tables. The tissue nutrient concentration is essential for calculating plant nutrient demand and hence limitation. Also the reference used is a study from 1992 in West

Africa, the use of which needs to be justified.

First, we moved this sentence earlier in the Main Text where the methods is described (L300-303).

About the justification of the reference used: it is true that “West Africa” in mentioned in the title of van Duivenbooden et al. (1992) but this paper in fact compiled fertilizer trial data on nutrient uptake and yield response, coming from a multitude of climatic and socio-economic environments. Van Duivenbooden et al. reported both P and C contents of maize organs which is not so common in the literature. A broad compilation of recent studies would be more appropriate but is behind the scope of our current analysis. And, as mentioned earlier, we took into account the uncertainty related to the parameters derived from van Duivendbooden et al. (among others parameters) in our uncertainty analysis.
We modified the sentence where the reference is quoted (L300-303). We also included a sentence about our uncertainty analysis in the new draft (L346-347).

L 418 “ the N supply budget encompasses an term for N fixation by leguminous occurring in the same grid-cell as cereals” This is an unrealistic assumption and needs better justification and discussion

From Bouwman et al., we used one value of soil N budget per grid-cell without crop distinction. This variable encompasses different terms: fertilization, manure application, fixation, volatilization (see above). Each term has been initially computed for different crop categories (upland crop, rice, leguminous) then averaged to get an averaged term for each grid-cell. Bouwman et al. performed this average with a weight corresponding to the contribution of each crop type to total crop area of each grid-cell.

Ideally, it would be better to use the terms of the budget corresponding to the upland crop only but we did not get this information. Thus, we used the average among crop categories. We think that it is an acceptable assumption as we can assume that the N fixation for leguminous should compensate lower N fertilizer applied to leguminous

than to upland crop. We removed the sentence about crop rotation as we now think that this caveat is only of second order. In Table S1, we report if the different variables (demand and supply) are crop dependent or not.

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