Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-298-AC1, 2020

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

Bruno Ringeval et al.

bruno.ringeval@inra.fr

Received and published: 21 January 2020

We thank the Referee #1 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 manuscript by Ringeval et al. presented a theoretical framework to quantify the different formalizations of nutrient interaction (N and P) for global croplands. The authors linked their theoretical framework with the nutrient limitation categories defined by Harpole et al. (2011). The authors then applied the framework with global maps

of soil supply and plant demand for croplands to quantify the degree of (co-)limitation, and showed that "a true co-limitation could affect a large proportion of the global crop area if multiple limitation hypothesis is assumed".

The theoretical framework presented here is a great highlight of this study, but the justification as to why it is important, in particular for cropland systems, was not well articulated. What I can extracted from the Introduction was that the author attempted to perform a meta-analysis on nutrient interaction effect on global cropland but found out that it was not possible, so they had to switch to a theoretical analysis. I personally don't think that this is a good rationale for this sophisticated work. To start with, why is a meta-analysis needed for global cropland where these managed systems are mostly over-fertilized and subject to human perturbation? Then, I find it hard to convince myself that there is indeed no "cross fertilization experiments" for croplands in general. The authors probably need to spend more time convincing their readers that this is indeed the case – it currently just doesn't convince me. The lack of good justifications for their rationale leave with me the question as to why this work is needed – as it currently stands, I find the application of the theoretical work to cropland not justifying the sophisticated mathematical framework presented in the main text and the supporting materials.

To reply to this comment, we deeply modified the introduction (L84-153). In the new draft, we first explained why we decided to develop a theoretical framework (L84-107): the aim is to understand which nutrient limitation categories defined by Harpole et al. are prevented and which ones are more or less 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.
Then, we explained why we applied our framework to the case of nutrient limitations in croplands (L108-148). The first reason is that nutrient limitation occurs in many places of the World, even if some other places (as mentioned by the Referee) are over-fertilized (L109-113). Second reason is that crossed single fertilization addition are not so common in cropland. Or at least, they can be exploited with difficulties (L117-136). 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 never aimed to perform a meta-analysis and this has been clarified in the new introduction.

The other major weakness of this study is the Discussion section. It seems that the authors spend the majority of the Discussions discussing the limitations of this work. While it is absolutely needed to acknowledge the limitation of the presented work, I would still like to see more discussion on the findings itself, e.g. how does it compare to other analyses, what are the implications, what knowledge do we get in terms of cropland management, etc.

We modified the Discussion as follows:
- first, the weight of the caveats of our modelling approach in the Discussion has been slightly reduced. We moved a part of the caveats of the modelling approach from the Discussion to the methods (Section 3.1; L339-347) to reply to one comment of the Referee #2. These caveats are still summarized in the Discussion (L472-475). We also removed the caveat about crop rotation as following one comment of the Referee #2, it appears now to us as of second order
- second, we increased the discussion about the findings themselves. We added L436. We also modified L532-565 to be less negative and show the advances allowed by our approach: while the comparison of ΔR_N and ΔR_P between our study and previous estimates is limited as the methodologies are really different, our work shows that

many couples $(\Delta R_N, \Delta R_P)$ exist when MH formalism is assumed and we put more emphasis on this point.

We also changed the first paragraph of the Discussion (L426-463) to make the link with meta-analyses performed on natural ecosystems clearer (following the previous Referee comment).

Specific comments:

L 24: I don't think the citation is needed here. These definitions of nutrient interaction categories existed before Harpole et al. 2011.

We removed the reference in the abstract.

Also, it is hard to understand what you meant by "the implications". Can you be more specific about on what the implications apply to? Is it productivity, or something else? How does this justify your rationale? It is hard to extract any useful information from this sentence along to justify the purpose of this study (in fact, this also applies to the introduction section). L 29: What do you mean "the corresponding between most Harpole categories"? L 32: what "certain conditions"? Can you be more specific? Basically, up to this point reading the abstract, I learnt nothing. L 38: Again, nothing can be learnt from this sentence.

All above comments are related to the fact that the abstract in the previous draft version was not explicit enough. We deeply modified it in the new draft (L20-47).

L 36: The word "true" is misleading here. If co-limitation occurs under a certain assumed hypothesis (i.e. multiple limitation), then it is not a "true" co-limitation, right?

In categories as defined by Harpole et al., a co-limitation could be either i) true and synergistic or ii) synergistic alone. "Synergistic" means that $\Delta pro_{+NP} > \Delta pro_{+N} + \Delta pro_{+P}$. A co-limitation is "true" when the ecosystem is observed to respond to combined N and P addition only, or to both N and P when added separately. These terms are defined in the Section 2 (L204-215). A true co-limitation is also defined in the Intro-

duction (L87-89). For purpose of simplicity, we would prefer to not define "true" in the Abstract.
Contrary to what is suggested by the Referee comment, we found that a true co-limitation can occur in many cases with multiple limitation formalism (see Table 1). This is one proof that the meaning of each category in terms of nutrient interaction formalism is complex. This is especially what our study tries to assess.

L 40: Normally croplands are over-fertilized, right? Why is it important to "improve our understanding of nutrient limitation in cropland" then?

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).

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.

L 56: You may need to define what is potential growth really. How is potential growth estimated? Is it only an assumption of nutrient limitation? I doubt that. Light, water, soil physical environment, human management, plant acclimation ability, and genetics are all important between potential and actual growth.

Following (Lobell et al., 2009), "the theoretical upper limit to crop yields is dictated by the amount of energy absorbed by a crop canopy and the light-use efficiency of photosynthesis" (. . .) which "varies with CO_2 concentration and temperature in C3 plants". "Yield potential must be defined in relation to a specific planting date and cultivar or hybrid maturity". Thus, "potential yield is determined by prevailing radiation, temperature and atmospheric CO_2 nd cultivar characteristics" (van Bussel et al., 2015).
Differences between potential and real yields (called yield gap) is attributed to lack of water, lack of nutrients and pest/diseases.
We have clarified this in the new introduction (L147).

Van Bussel, L. G. J., Grassini, P., Van Wart, J., Wolf, J., Claessens, L., Yang, H., Boogaard, H., de Groot, H., Saito, K., Cassman, K. G. and van Ittersum, M. K.: From field to atlas: Upscaling of location-specific yield gap estimates, Field Crops Res., 177, 98–108, doi:10.1016/j.fcr.2015.03.005, 2015.
Lobell, D. B., Cassman, K. G. and Field, C. B.: Crop yield gaps: their importance, magnitudes, and causes, Annu. Rev. Environ. Resour., 34(1), 179, 2009.

L 74: I find this "conceptual optimum stoichiometry" very hypothetical. I don't think one can easily estimate this.

We agree with this comment and modified the sentence (L79): "a conceptual <u>and theoretical</u> optimum stoichiometry"

L 87: what is a true co-limitation? Have you defined it anywhere yet?

Yes, "true" co-limitation was defined at the beginning of that paragraph (L87): "there is a true NP co-limitation when the ecosystem is observed to respond to combined N and P addition only, or to both N and P when added separately." It is also defined in Section

2 (L211-215).

L 89: If plants need to mobilize resources to acquire one nutrient, do you call it a single limitation or co-limitation?
The fact that plant could adjust by mobilizing the nutrient in excess to access the limiting one would make the two nutrient limitations closer to each other (than without adjustments). Thus, it would be in favor to co-limitation.
But if the difference between the two limitations is too large and if the plant cannot adapt enough to achieve co-limitation, it would still result in single limitation. In the introduction, we explained that such adjustments lead researcher community to represent the interaction by multiple limitation formalism (MH). And we showed in our study that either co-limitation (in most of cases) or single limitation (in few cases) can occur with this formalism.
I find timescale is an important aspect of nutrient limitation here, which suggest that your study may need to have a more clearly defined temporal scale under which different limitation theories apply to.
We agree with the fact that time-scale is an important aspect of nutrient limitation. We already discussed this in the previous version of the Discussion (L591-594). As mentioned in the draft (L76), Liebig's law of minimum or multiple limitation hypothesis could be considered as macro-properties that reflect the processes of plant adjustments. Such adjustments (change in shoot:root ratio, etc.) are relevant to the growing season scale. Thus, we would assume that our theoretical analysis is appropriate to the annual time-scale. Note that change in the plant community consecutively to fertilizer application that could occur at a longer time-scale in natural ecosystems cannot occur in cropland systems as they are mostly single crop (L465).
Besides, our estimate of limitation category occurrence (section 3) is particularly adapted to the scale of the growing season as our supply estimates relies for N on annual fertilizer application and as our demand estimates are based on the need of plant over a growing season. We have added this information in Section 3.1 (L282).

L 89 – 90: Unclear what you meant here. Are you trying to categorized each nutrient limitation hypothesis into either LM and MH?
Following the first general comment, we modified this sentence (L96): "While these categories are commonly used in literature, what each category implies in terms of formalism of nutrient interaction remains unclear. In particular, we aim here to understand which categories are prevented and which ones are more or less promoted by the interaction formalism assumed."
Also, the next sentence seems to have a big jump with this current sentence.
The next sentence was about cropland. We modified the rationale in the new introduction and now dedicated a separated paragraph to cropland (L108-148).

L 96 – 97: I find myself hard to follow your logic. If the same fertilizer is applied each year for many years, you can still analyze the current limiting nutrient, no?

The issues with long-term field trials are multiple. First, such long-term field trials mainly concern P while single application are commonly studied for N. This makes difficult to decipher the contribution of each nutrient.
Second, in long-term field trials, the same treatment is applied each year for many years. Usually, many application rates are tested (e.g. P0, P1, P2 which means that the P application covered 0, 1 or 2 times the P contained in the exports, respectively). This makes the long-term trials disconnected to what happen in the surrounding fields, and thus it makes the nutrient limitation in the long-term trials not relevant to investigate the real nutrient limitation in the surrounding region.
We modified the manuscript to be clearer on that point (L117-129).

L 99 – 101: The way you phrase it makes me feel that you really just wanted a meta-analysis of N and P limitation in croplands. But why is it important? Croplands are managed systems where nutrients are added to maximize yield. I would normally expect croplands are over-fertilized, and the amount of fertilizer varies depending on crops, climate, and other factors (e.g. financial). Why do you need to compute a NP limitation meta-analysis for croplands? Even for a meta-analysis, there is the need for some clearly defined rationales. The current narrative gives me no answer, and I keep wondering what is the point for performing such a meta-analysis to start with.

As mentioned in our reply to the 1st general comment, we did not aimed to perform a meta-analysis. We have deeply modified the introduction to be clearer on that point. We also explained why it matters to study nutrient limitation in global cropland.

L 120: what about different forms of nutrients?

Both supply and demand are expressed in kgP/ha/yr or kgN/yr/ha. For both nutrients, the supply corresponds to the amount of nutrient that can be used by the plant (so-called available N and P) which does not require to consider different forms within the values of the demand and supply themselves.
However, it seems important to consider different forms during the computation of the supply, especially for P. In our study, the supply of P corresponds to the fraction of labile inorganic P (Fig.1 of Ringeval et al. 2017, GCB) which diffuses to and which is uptaken by the root (L314). For N, the computation was made easier as we did not consider long-term soil N dynamic (as stated at L331).

L 144: What is the symbol in the middle? Is it multiplication? Why? Can to provide some texts to justify these equations?

The symbol in the middle of Eq.7 (L182) is a multiplication. Symbol in the middle of Eq.8 (L183) is a comma (such as in "min (x,y)"). We made Eq.7 clearer by using a star for the multiplication and added some sentences to explain the two equations (L184-186).

L 150: Is there any justification of the arbitrary value of 0.75?

0.75 is commonly used in yield gap analysis (e.g. in Mueller et al. 2012). However, as mentioned in the Discussion, "our nutrient limitation is not straight connected to the yield gap because the actual yield is not used in our computation" (L555) and to prevent any confusion, we prefer to say that value of 0.75 is arbitrary chosen.

L 153 – 155: Any explanation for these results? Similarly, some further explanation of the results showed later would guide the readers to understand your work.

Now L194: "the largest differences in R_{NP} between the LM and MH mathematical formulations are obtained for comparable R_N and R_P values ($R_N \sim R_P$) and both within [0.25-0.75] (Fig. 2c)"
This result is totally driven by the mathematical formulation of LM and MH formalism: x in [0.25-0.75] and x∼y mathematically maximize the difference between x*y and min(x,y). We added L196 to clarify this.

L 258: observed yield for each grid or global mean?

We modified this sentence to clarify: "within a climate bin, the potential yield characterizing this bin is defined as the area-weighted 95th percentile of the grid-cell observed yields." (L309)

L 298 – 301: So your results start with figures and texts in the supplementary materials?

Yes, as we want to focus on the effect of the formalism choice on the global values of R_{NP}, etc. instead of on the global distribution of R_{NP} (now L360) which is quite uncertain (as explained in the Discussion) and while both (distribution and sensitivity) are connected (L470).

L 381: Why? Can you explain a little bit more?

We added some sentences (L454-468) to explain more.

L 384 and onward: So from this point forward, you are discussing the limitations of this study. Can you maybe spend a little bit more space describing your results (i.e. implications, significance, comparison to other studies, etc.)?

We modified the Discussion to improve these points, as replied to the 2nd general comment.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-298, 2019.