



Open Access This file is licensed under a Creative Commons Attribution 4.0 International License, which permits use, sharing, adaptation, distribution and reproduction in any medium or format, as long as you give appropriate credit to the original author(s) and the source, provide a link to the Creative Commons license, and indicate if changes were made. In the cases where the authors are anonymous, such as is the case for the reports of anonymous peer reviewers, author attribution should be to 'Anonymous Referee' followed by a clear attribution to the source work. The images or other third party material in this file are included in the article's Creative Commons license, unless indicated otherwise in a credit line to the material. If material is not included in the article's Creative Commons license and your intended use is not permitted by statutory regulation or exceeds the permitted use, you will need to obtain permission directly from the copyright holder. To view a copy of this license, visit <http://creativecommons.org/licenses/by/4.0/>.

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

2nd May

Dear Dr Wyckhuys,

Your manuscript titled "Pest management science hampered by abstraction" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

Specifically, for publication in Communications Earth & Environment to be appropriate, we will need you to:

- 1) Provide more compelling evidence that pest management research in the Global South is not adequately allocated, and discuss in more detail the underlying reasons and their implications for future research
- 2) Demonstrate that the more holistic approaches you advocate outperform the research methods currently used in the field
- 2) Ensure your interpretations and claims are robust, or alternatively tone down or remove unsupported statements

We also ask that you provide completed meta-analysis and systematic review checklists and study flowcharts, to facilitate peer review. We have attached a blank checklist form and flowchart to this email and ask that you fill in the appropriate 'Author Responses' in the checklist and 'n values' on the flowchart (For more information on reporting standards for systematic syntheses, please see: <https://www.roses-reporting.com/>).

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will 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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not 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 reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any completed checklist:

<https://mts-commsenv.nature.com/cgi-bin/main.plex?el=A2DE2BMw7A5DbT1I6A9ftd8ZXpedqslYYbOh0IKTjitAZ>

**** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first ****

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Alienor Lavergne, PhD

Associate Editor

Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(<https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf>) and formatting guide (<https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf>).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors present a bibliometric approach to assess the status of the current, last ten years, of pest management science. I applaud the effort in analyzing and categorizing the vast number of studies they have found through the search. The paper is very well written, and I enjoyed how fluent it is in presenting results and discussion. Although the study arises the problems of pest management science, I miss a discussion about why the results obtained are what they are. For example, they complain about the low number of species studied but do not give any reason for why such a small number of pest species are studied. This comment can be extended to all the different thematic areas that are considered. Besides this, I do not see any reason for why this paper should not be published.

Some minor comments:

L211 & L 218. I do not understand why a range of percentages is presented here. Please, explain.

L457. Please, indicate which are these taxa.

Reviewer #2 (Remarks to the Author):

The goal of this study is to quantify major trends in pest management research in the Global South (i.e., developing countries in Africa, Latin America, Southeast Asia, and the Middle East), to see whether research is pushing towards major changes versus conforming to long-established, pesticide-based management approaches. The authors use a bibliometric analysis to address this question.

This is a very ambitious undertaking, and I am sympathetic to the challenges facing the authors. But I do have some real concerns about the study, and in particular the degree to which their own data clearly support the conclusions that they draw.

Lines 154-156. I have two questions here. First, what is the source of the species-level total pool of herbivore taxa? Given how many species are undescribed, this seems like a non-trivial task. Even for described species, what rule was used to categorize a given species as an herbivore, as opposed to an omnivore, for example? Second, although the authors suggest that the research attention is focused on a small slice of total herbivore diversity, this seems likely to reflect the fact that most crop damage is generated by a small slice of total herbivore diversity. It is an old tenet of biocontrol theory that the vast majority of herbivores are sufficiently regulated by their community of natural enemies that they do not emerge as economically-damaging pests of our crops. So, is there any reason to think that research effort is being allocated in an inappropriate manner? That would seem to be the key question. The authors state that “taxon-specific research attention is thin . . .”, but I’m not sure what that means.

Lines 159-160, and Supplementary Figure 2. Showing that the number of papers documenting insecticide resistance for a given pest is positively correlated with the total number of literature records is, in my opinion, hard to interpret. More important pests are studied more, less important pests (or non-pests) are studied less; paper counts based on any subset of the published papers (defined by any particular topical focus) will be very likely to correlate positively with the total number of published papers for a given pest species. Honestly, I think this correlation is meaningless; it certainly should not be viewed as evidence that insecticide resistance is a primary driver of the intensity of overall research, however plausible that hypothesis might be. To create a compelling argument that insecticide resistance drives higher research attention, the authors would need to control statistically for overall pest severity, somehow.

Lines 164-166. The authors argue here that scientific effort devoted to different groups of crops is not “in line with” their relative share in the reference diet. But the correlation (shown in the left panel) actually looks quite strong. What is the correlation coefficient here? How high do the authors expect it to be before they would consider it to be evidence of reasonable allocation of research effort? The right-hand panel shows a much noisier relationship between research effort and insecticide mass; but mass of insecticides is an imperfect measure of the burden placed on the environment by the insecticides, because toxicity (to humans and other non-target organisms) often varies by multiple orders of magnitude across different active ingredients. A different response variable that attempts to capture the toxicological impact of the pesticides would be preferable here.

Overall, I think the authors have a difficult task in trying to use these bibliometric data to argue that pest management research is not ideally allocated if our goal is to promote more ecologically-centered pest management. I don’t find their argument, as currently constructed, to be convincing.

Lines 313-317. What is the evidentiary basis of the statement that non-chemical control has only resulted in sustained adoption on 1% of farmland? (And what does ‘sustained adoption’ mean?)

Lines 316-319. Here the authors argue that pest control research “conforms to the prevailing agro-industrial model and fosters input dependencies . . .” This may be true, but I’m not sure how their bibliometric analysis supports this conclusion. Figure 3 shows that the top three “thematic foci” in pest management research are (1) bio-ecology, (2) preventative non-chemical management, and (3) curative non-chemical management. Only in fourth rank do we find curative chemical management. So, maybe fourth place is still too much, but their own data suggest that the vast bulk of pest management research is, in fact, devoted to non-chemical control methods and pest ecology. So, in what way is the current research endeavor fostering input dependencies? I don’t see how their data support this primary conclusion.

The authors conclude by calling for a scientific revolution. I would find this call to be more compelling if they could demonstrate that the more holistic approaches that they are advocating actually outperform the research methods that are currently used (which seem to cover a wide arc of different methods, scales, and degrees of complexity). I don’t think this would be easy to demonstrate, but it should be an important goal.

RESPONSE TO REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors present a bibliometric approach to assess the status of the current, last ten years, of pest management science. I applaud the effort in analyzing and categorizing the vast number of studies they have found through the search. The paper is very well written, and I enjoyed how fluent it is in presenting results and discussion. Although the study arises the problems of pest management science, I miss a discussion about why the results obtained are what they are. For example, they complain about the low number of species studied but do not give any reason for why such a small number of pest species are studied. This comment can be extended to all the different thematic areas that are considered. Besides this, I do not see any reason for why this paper should not be published.

Response: We are grateful to reviewer #1 for seeing merit in our manuscript, and for his/her positive assessment of our submission. As for the small number of species that are currently studied, we add the following: "Given the above, the desire to further scientific understanding for a small set of biota i.e., to 'dig deeper' likely exerts a stronger gravitational pull than that of actually remediating their in-field management (47)." In line 333-341, we already listed some of the (possible) underlying determinants of the observed patterns: "This spatial mismatch, failure to perform wholeness-oriented science and incapacity to meaningfully link to people results from disciplinary specialization and large conceptual divides within agrifood science". In lines 288-292, we equally point at parallels with pest management science in Australian grain systems or with key blind spots that exist in soil biodiversity and ecosystem function research globally. Lastly, in lines 339-347, we also already included the following section: "This spatial mismatch, failure to perform wholeness-oriented science and incapacity to meaningfully link to people results from disciplinary specialization and large conceptual divides within agrifood science (49). A landscape-level framing of pest issues in se can help to remediate this by integrating cross-scale ecological and social dynamics (70,71). However, scientists struggle to slot such complexity into traditional experimental set-ups or cope with needs for extra labor, costs and cross-disciplinary engagement under short-cycle projects and 'publish or perish' imperatives (72,73). To complicate matters further, interdisciplinary science faces lower funding success and outright penalization by scientific peers (74,75)." However, as several of the underlying determinants are unknown and yet to be formally identified, we are hesitant to engage in further speculation.

Some minor comments:

L211 & L 218. I do not understand why a range of percentages is presented here. Please, explain.

Response: We clarify that the range of percentages specifically pertains to the five main herbivore taxa. We have adapted our sentence as follows: "Across the above five taxa, bio-ecology invariably represents the most popular theme (range 37.5-55.9% studies)".

L457. Please, indicate which are these taxa.

Response: As suggested, we now specifically list the five main taxa.

Reviewer #2 (Remarks to the Author):

The goal of this study is to quantify major trends in pest management research in the Global South (i.e., developing countries in Africa, Latin America, Southeast Asia, and the Middle East), to see whether research is pushing towards major changes versus conforming to long-established, pesticide-based management approaches. The authors use a bibliometric analysis to address this question. This is a very ambitious undertaking, and I am sympathetic to the challenges facing the authors. But I do have some real concerns about the study, and in particular the degree to which their own data clearly support the conclusions that they draw.

Response: We wish to thank reviewer for his/her positive assessment of our manuscript and for providing several excellent suggestions. We have tried to rebuild several sections of our discussion and conclusions section, adding in concrete examples of successful initiatives (e.g., hands-on discovery-based learning through the UN-led farmer field schools, or ecologically-underpinned management in Asian rice) and bringing in an additional set of literature references that underline the need for cross-disciplinary efforts.

Lines 154-156. I have two questions here. First, what is the source of the species-level total pool of herbivore taxa? Given how many species are undescribed, this seems like a non-trivial task. Even for described species, what rule was used to categorize a given species as an herbivore, as opposed to an omnivore, for example?

Response: We recognize that improper wording might have created undue confusion. We wish to clarify that the total species pool refers to the 881 herbivores that were covered in the entire literature corpus i.e., 3,407 peer-reviewed publications. To avoid eventual confusion among future readers, we now make explicit mention of this number of taxa and have reworded the sentence as follows: "...with 94.6% of all (881) herbivore taxa featuring in less than one publication per year and only 0.6% taxa receiving more than five publications per year (Fig. 1). For 43% of the (881) herbivore taxa, research covers..."

As our systematic literature review was specifically geared towards pest management science in cultivated plants (i.e., crops), we considered all target animal biota to be herbivores. This is also evident in the WoS search strings that were used to generate the initial literature corpus. While we recognize that some of these herbivores (or pests) may indeed exhibit omnivory, these omnivorous feeding habits likely did not incite scientific inquiry in the pest management domain. Yet, to avoid unnecessary confusion among future readers, we now clarify that "Given that all taxa engage in herbivory and may attain pest status in agricultural settings, eventual other feeding modes e.g., omnivory were not logged."

Lines 154-156: Second, although the authors suggest that the research attention is focused on a small slice of total herbivore diversity, this seems likely to reflect the fact that most crop damage is generated by a small slice of total herbivore diversity. It is an old tenet of biocontrol theory that the vast majority of herbivores are sufficiently regulated by their community of natural enemies that they do not emerge as economically-damaging pests of our crops. So, is there any reason to think that research effort is being allocated in an inappropriate manner? That would seem to be the key question. The authors state that "taxon-specific research attention is thin . . .", but I'm not sure what that means.

Response: This is an excellent remark. We concur with reviewer #2 that, across biomes, plant or natural enemy species richness positively correlates with effective herbivore population regulation. Indeed, many non-pest herbivores are kept at bay through different ecological processes in biodiverse settings. However, such natural pest regulation is comparatively weak in expansive, genetically-uniform monocultures and simplified farming landscapes (i.e., hallmark features of

modern-day agriculture) and -as a result- herbivores do regularly attain the status of economically-damaging pests - as convincingly shown for global agriculture by Dainese et al. (2019). Based upon the above rationale, we inserted the following elements in the Discussion: “Considering how traditional, biodiverse farming systems are increasingly dismantled, chemically intensified or embedded in simplified landscape matrices, it is crucial to assess how ecological regulation underpins resilience and prevents herbivores from attaining pest status (21,62). Doing so can help to anticipate, forestall or even reverse trophic cascades, regime shifts and pest outbreaks that result from plant, animal or habitat diversity loss. For instance, a broader organismal focus beyond the initial target herbivores could have precluded secondary outbreaks of sap-feeding hemipterans in transgenic Bt crops – the latter geared towards lepidopteran pest control. Further, an in-depth study of ecological mechanisms in invasive pests’ native range can yield nature-friendly mitigation options (63).”

Given confusion regarding the amount of taxa-specific research attention has been defused by rephrasing the sentence to “Overall, limited amount research attention is specifically geared towards individual taxa...”.

Lines 159-160, and Supplementary Figure 2. Showing that the number of papers documenting insecticide resistance for a given pest is positively correlated with the total number of literature records is, in my opinion, hard to interpret. More important pests are studied more, less important pests (or non-pests) are studied less; paper counts based on any subset of the published papers (defined by any particular topical focus) will be very likely to correlate positively with the total number of published papers for a given pest species. Honestly, I think this correlation is meaningless; it certainly should not be viewed as evidence that insecticide resistance is a primary driver of the intensity of overall research, however plausible that hypothesis might be. To create a compelling argument that insecticide resistance drives higher research attention, the authors would need to control statistically for overall pest severity, somehow.

Response: We fully concur with reviewer #2 that the detected patterns are hard to interpret and may reflect overall organismal importance. We equally wish to clarify that we do not necessarily argue that insecticide resistance acts a primary driver of research attention/intensity – see line 277: “Whether the added emphasis on IR pests is merited i.e., a genuine reflection of (farm-level) organismal priorities or an attempt at symptomatic control of pesticide-induced issues is unclear”. Given the above, we have opted not to elaborate further on the (eventual) causal linkages between insecticide resistance and pest management science. Instead, we choose to simply present the patterns as such.

Lines 164-166. The authors argue here that scientific effort devoted to different groups of crops is not “in line with” their relative share in the reference diet. But the correlation (shown in the left panel) actually looks quite strong. What is the correlation coefficient here? How high do the authors expect it to be before they would consider it to be evidence of reasonable allocation of research effort? The right-hand panel shows a much noisier relationship between research effort and insecticide mass; but mass of insecticides is an imperfect measure of the burden placed on the environment by the insecticides, because toxicity (to humans and other non-target organisms) often varies by multiple orders of magnitude across different active ingredients. A different response variable that attempts to capture the toxicological impact of the pesticides would be preferable here.

Response: We are thankful to reviewer #2 for emphasizing a need for additional metrics that capture the toxicity of insecticides. In the meantime, we wish to clarify that our original statement read “scientific effort for certain food crops is not in line with their relative share in the global reference

diet” – in this particular case, specifically referring to vegetables. Meanwhile, we wholeheartedly agree with the reviewer that the overall correlation is quite strong.

In Figure 2, we have now replaced the graph on total insecticide mass with one covering total insecticide hazard load for 12 different crop categories. Here, we calculated the hazard load of an insecticide as the applied mass divided by its corresponding no-observed adverse effect levels in mammals and birds, taking into account the toxicity of insecticides. We believe that the new graphs i.e., scientific attention vs. total insecticide hazard load are more convincing. We have also amended our statement as such: “Though scientific effort for food crops is in line with their relative share in the global reference diet (Pearson’s $r = 0.925$, $p < 0.01$), it diverts from their proportional contribution to global insecticide mass or total insecticide hazard load (Spearman Rank’s $\rho = 0.462$, $p = 0.13$; $\rho = 0.315$, $p = 0.32$; Fig. 2, Suppl. Fig. 3).”

Lines 313-317. What is the evidentiary basis of the statement that non-chemical control has only resulted in sustained adoption on 1% of farmland? (And what does ‘sustained adoption’ mean?).

Response: Van Lenteren et al. (2018) estimate that invertebrate or microbial biological control is applied on more than 30 million ha i.e., a mere 0.6% of the 4.8 billion hectares of global farmland. Notwithstanding the use of chemicals such as copper and sulphur in organic production, we opted to strengthen our argument by also referring to the (potential) adoption of non-chemical control under organic production. We have amended this paragraph as follows: “Though a heightened attention to invertebrate or microbial biological control is praiseworthy, this practice is only adopted on less than 1% farmland globally (73). Other forms of non-chemical control both curative and preventative are plausibly implemented on the 77.4 million ha under organic production i.e., 1.6% of global farmland or a mere 0.7% in the Global South – as estimated using FAOSTAT.”

van Lenteren, J.C., Bolckmans, K., Köhl, J., Ravensberg, W.J. and Urbaneja, A., 2018. Biological control using invertebrates and microorganisms: plenty of new opportunities. *BioControl*, 63, pp.39-59.

Lines 316-319. Here the authors argue that pest control research “conforms to the prevailing agro-industrial model and fosters input dependencies . . .” This may be true, but I’m not sure how their bibliometric analysis supports this conclusion. Figure 3 shows that the top three “thematic foci” in pest management research are (1) bio-ecology, (2) preventative non-chemical management, and (3) curative non-chemical management. Only in fourth rank do we find curative chemical management. So, maybe fourth place is still too much, but their own data suggest that the vast bulk of pest management research is, in fact, devoted to non-chemical control methods and pest ecology. So, in what way is the current research endeavor fostering input dependencies? I don’t see how their data support this primary conclusion.

Response: The statement has now been toned down to “Overall, as the scientific enterprise continues to focus on curative tactics while discounting the pivotal role of decision aids or the broader enabling environment, it is questionable whether it will ever break the IPM mirage.” The above statement is supported by three clearly elaborated arguments, which directly emanate from our literature review and are listed in lines 297-310. A first argument illuminates how curative (chemical + non-chemical) measures receive roughly 20% more scientific attention than preventative non-chemical ones. For the five main herbivore taxa, these patterns are exacerbated: our data show that curative (chemical or non-chemical) control receives up to twice as much scientific attention than non-chemical preventative control (Supplementary Figure 3). A third argument points at the virtual non-existence of economic thresholds i.e., vital decision support tools to rationalize the use of insecticides.

Even though the vast bulk of pest management research is indeed devoted to non-chemical control methods, this does not transpire in actual on-the-ground adoption – mirroring major deficiencies in the way agro-ecology science is being conceptualized or conducted. This has been noted by several other authors e.g., Gonzalez-Chang and Wratten indicate that “despite the high number of agro-ecological publications, very few lead to outcomes”. We have reworked this paragraph further to bring those elements forward as such: ““... the unrelenting global proliferation of chemical control (12,44,71,72) reflects an inability to capitalize on the sizable research progress in non-chemical preventative management. Lagging uptake of those practices signals how the underlying areas of scientific inquiry are poorly calibrated and conceptualized. Indeed, by skipping one or more steps in the sequential process to effectively harness the power of biodiversity (36,40), agro-ecological research rarely spawns desirable outcomes... Troubleshooting BCA or agroecology science and resolving its socio-technical adoption hurdles is thus imperative to maximizing the potential of science to transform practice (36,44).”

Overall, I think the authors have a difficult task in trying to use these bibliometric data to argue that pest management research is not ideally allocated if our goal is to promote more ecologically-centered pest management. I don't find their argument, as currently constructed, to be convincing. The authors conclude by calling for a scientific revolution. I would find this call to be more compelling if they could demonstrate that the more holistic approaches that they are advocating actually outperform the research methods that are currently used (which seem to cover a wide arc of different methods, scales, and degrees of complexity). I don't think this would be easy to demonstrate, but it should be an important goal.

Response: Our results show, within the limitation of the methodology, that the majority of the pest management research output from the target regions is suffering from limited pest- and crop- centric foci with poor inclusion of broader farming variables and strata. We argue that ecologically-centered pest management, which entails the need for in-depth, context-specific insights into the biology of target herbivores, wider assortment of (social-ecological) farming variables and ecological processes is a better target to aim for, as illustrated in the case of pest management in Asian paddy rice and its demonstrated impact on pesticide use reduction, facilitation of farming system diversification and yield improvement/ conservation (In 274-280). Furthermore, the mismatch between research output on preventative non-chemical tactics and their adoption calls for broader inclusion of the social strata to understand the processes and environment that enable large-scale adoption of sustainable pest management. This is supported by the globally-acclaimed impacts of the Farmer Field Schools, where hands-on discovery-based learning led to pesticide cuts up to 80-90% on millions of hectares of farmland across tropical Asia – and subsequently the global tropics e.g., in Peru or Central America (In 399-402).

We have incorporated several of the above elements in a revamped concluding paragraph, in which we emphasize how more holistic approaches on both the social as well as ecological angle outperform the research methods currently used in the field. We close this paragraph with the following: “Only when pest management science duly and fully accounts for the multiple farming system variables and strata can we expect to see real-world impacts in safeguarding food security, halting biodiversity loss and upholding human health.”

2nd Jun

Dear Dr Wyckhuys,

Your manuscript titled "Pest management science hampered by abstraction" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your article one last time to ensure that your manuscript remains balanced and stimulates discussion on the importance of rethinking the science of pest management. Since this is a meta-analysis, your manuscript should be less opinionated. 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.

EDITORIAL REQUESTS:

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

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files listed at the end of the Editorial Requests Table; the list of required files is also available at <https://www.nature.com/documents/commsj-file-checklist.pdf>.

OPEN ACCESS:

Communications Earth & Environment is a fully open access journal. Articles are made freely accessible on publication under a [CC BY license](http://creativecommons.org/licenses/by/4.0) (Creative Commons Attribution 4.0 International License). This license allows maximum dissemination and re-use of open access materials and is preferred by many research funding bodies.

For further information about article processing charges, open access funding, and advice and support from Nature Research, please visit <https://www.nature.com/commsenv/article-processing-charges>

At acceptance, you will be provided with instructions for completing this CC BY license on behalf of all authors. This grants us the necessary permissions to publish your paper. Additionally, you will be asked to declare that all required third party permissions have been obtained, and to provide billing information in order to pay the article-processing charge (APC).

Please use the following link to submit the above items:

<https://mts-commsenv.nature.com/cgi-bin/main.plex?el=A7DE1BMw1B4DbT2I6A9ftd8ZXpedqslYYbOh0IKTjitAZ>

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

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

Best regards,

Alienor Lavergne, PhD

Associate Editor

Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I think that the authors has addressed my concerns and I do not see any reason for not accepting this manuscript in its current form.

Reviewer #2 (Remarks to the Author):

The authors have carefully considered the feedback from the reviewers. I do not agree with some of their decisions; but, given that this manuscript is largely an opinion piece, that's fine -- there should be no expectation for total concordance of viewpoints. I think the paper will stimulate others to grapple with the issue of how best to allocate research effort to enhance agricultural production while decreasing externalities associated with pesticide use. Given the burden that pesticides impose on environmental health, this is a very important issue.