nature portfolio

Peer Review File

Warming demands extensive tropical but minimal temperate management in plant-pollinator networks

Corresponding Author: Dr Udit Bhatia

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

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

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Bhatia,

Your manuscript titled "Warming demands extensive tropical but minimal temperate management in plant-pollinator networks" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

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

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

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

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
Link Redacted
** 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 receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

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

Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment

Consulting Editor
Communications Sustainability

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• Life sciences
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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

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In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

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

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

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

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

In the manuscript “Warming demands extensive tropical but minimal temperate management in plant-pollinator networks”, Datta et al. test whether plant–pollinator networks exhibit different responses to warming and management interventions depending on their ecological context, by comparing and simulating temperature changes across three regions. They did a good job, and their results were interesting. However, it is necessary for the authors to revise and include definitions of some key concepts within the introduction and throughout the manuscript to provide greater coherence to the study.
I offer some comments, questions, and suggestions below that may help improve the manuscript.

Major comments

It is necessary for the authors to clearly define some of the key concepts mentioned in the introduction and to link them with research questions and the discussion. This would provide the reader with better context regarding the relevance of the study. For example, it is not clear what is meant by a "management strategy." Although four management strategies are mentioned (Single Plant, Multiple Plants, Single Pollinator, and Multiple Pollinators), their significance is not defined in the introduction. This makes it difficult to follow the manuscript's logic across its sections (introduction, results, and discussion). I suggest establishing this connection early on to highlight the importance of this type of research.
I find a lack of structure in the introduction, results, and discussion sections. This lack of organization made the manuscript difficult to follow, so I suggest reorganizing these sections to improve clarity. Are the statements in lines 111, 151, 175, and 212 logically aligned with the content they introduce? I perceive a lack of connection between sections; some parts seem to address methodological aspects (even though there is already a dedicated methods section), while in others it is unclear whether results are being presented, discussed, or both.
The conclusions section seems repetitive in relation to what has already been discussed in the manuscript. Beyond stating that ecosystems may benefit in different ways depending on the management strategies, what do these results tell us from a biological perspective? Biological implications such as the decline of pollinators or the importance of certain key plant species, as well as the dynamics among the three regions, could be addressed through the approach proposed in your research. I suggest reframing the conclusions based on the two research questions posed and exploring these elements.
It would be advisable to consider the use of a repository to store the data used and generated in this research. This would promote the reproducibility of the results and allow future studies to utilize this data for additional or comparative analyses

Minor comments

L31In this context, would the use of the term 'negligible' be correct?
L43 Which ones?
L46 I suggest changing 'mutualistic plant-pollinator networks' to 'interactions between plants and their pollinators'; remember that networks are the analytical tools that allow us to interpret, to some extent, the interaction between the two groups.
L49 or floral visitors
L52 The terms 'specific management strategies' and 'uniform approaches' require clarification — what do they refer to, and which ones specifically? From this point onward, such concepts should be clearly defined and integrated logically into the text, or at least briefly contextualized.
L68: What exactly is an ecoregion? Does this study consider it as a relatively large biogeographic unit?
L69 Again, concepts such as 'multi-species management strategies' and 'single-species approaches' appear in the introduction without a clear definition. I suggest that each concept be briefly defined, as this will provide the reader with more context.
L71 see comment L69, 'current "universal" management strategies.
L65- 73 This paragraph is important, but context should be provided and what differentiates it from other studies should be highlighted.
L78 Active management' of what?
L 86-89 For this idea to be coherent within the text, the concept of 'active management' should first be defined.
L101- 106 This part should be integrated into the discussion. I suggest adding it to the Discussion section.
L110. Does the results and discussion start on this line? If so, please add the title 'Results and Discussion'.
L307 – 310 In this line, the concept of 'management interventions' is described. See previous comments and the suggestion to provide definitions for this type of concept starting from the introduction.
L397 - 400 This information should appear initially in the introduction.
L397 -427 This information is important in the Methods section, but I suggest extracting the most important parts (e.g., the four strategies in L399) and adding them in a 5–7 line paragraph in the Introduction. This would help and highlight the significance of your work.
L 474 For the statistical analyses carried out in this study, was any program or software used? For example, R or Python. If so, please mention it in this section."
L111 – 131 It may be the way these two paragraphs are written, but these lines read more like methodological information rather than results. I suggest organizing this section more clearly within the manuscript.
L138 References are missing.
L144- 148 The arguments in these lines are valid, but which studies are they being contrasted with? What other research addresses the variability of thermal changes and management strategies?
L165-167 References are missing.
L18 Does it show statistically significant variation?
L207 – 210 What explains the pattern of this result? Biologically, is it solely driven by temperature, or is it a synergistic effect between temperature and the abundance of both levels (plants and pollinators)
L245 -246 It is an interesting statement that would gain greater importance if several key concepts were defined from the introduction. I recommend linking it to the two research questions
L264-267 This point should be more thoroughly developed in the introduction
L290 Throughout the manuscript, the term 'ecosystem health' is used. Specifically, what does this refer to in the context of your research?

Reviewer #2 (Remarks to the Author):

The manuscript focuses on the impact of climate warming on plant-pollinator networks. Through integrated modeling, it analyzes responses of these networks across different ecoregions under warming conditions, addressing urgent ecological questions regarding ecosystem resilience and management strategies. This provides a theoretical foundation for region-specific adaptive strategies in biodiversity conservation.
Comments：
1.Although the model incorporates complex parameters, it overlooks:
(i) Evolutionary adaptation processes (e.g., thermal niche shifts, genetic regulation)
(ii) Lifecycle/reproductive cycle variations affecting species' thermal responses
(iii) Behavioral differences among pollinators (e.g., thermoregulation, migratory traits).
These exclusions may directly impact results (Lines 270-279).
2.The 1°×1° ESM simulations ignore microclimatic heterogeneity (e.g., forest edge vs. interior), reducing local-scale predictive accuracy (Line 280)
3.While advocating multi-species management, the manuscript fails to provide operational cases or feasibility analyses (e.g., implementing "species abundance maintenance" or criteria for selecting the top 10% managed species), limiting translational relevance (Lines 401-427).
4.Bonferroni-corrected *t*-tests lack variance/confidence interval disclosure, hindering robustness assessment. Visual analyses are inadequate (e.g., RAC curve differences across strategies require deeper interpretation) (Lines 442-484).
5.Networks M_PL_012 and M_PL_050 (Atlantic islands west of Gibraltar Strait) do not represent typical Köppen Csa ecosystems. Reanalysis with authentic Mediterranean networks is recommended (Lines 121-126).
6.M_PL_040 and M_PL_035 likely fall within the same 1° grid cell, potentially violating spatial independence assumptions (Lines 305-310).
7.M_PL_026 (island location) cannot typify tropical savanna climate (Aw). Justification for site selection is needed (Lines 301-304).

Reviewer #3 (Remarks to the Author):

Comments to the Author
Review for Communications Earth & Environment
Manuscript COMMSENV-25-2335

In this manuscript entitled “Warming demands extensive tropical but minimal temperate management in plant-pollinator networks”, Datta and colleagues address a highly relevant question in global change ecology: how warming impacts plant–pollinator networks differently across climatic regions, and whether management strategies should be adapted accordingly. By integrating temperature-dependent species parameters, Earth system climate projections (CMIP6), and mutualistic network dynamics, the study provides timely insights into the effectiveness of conservation strategies in tropical, temperate, and Mediterranean ecosystems.

In general, I think this study is well conducted and made good use of future/current global ecological and conservation impacts. While understanding plant-pollinator interactions under introducing the concept of Management Efficiency Ratio (MER) and emphasizing the differential benefits of multi-species management, is important and timely, there are several key issues in the current manuscript.

Major comments:
1. I noticed that the authors selected a total of 11 plant–pollinator networks from the Web of Life database (Table S1) , covering three ecoregions. As a non-specialist in Earth science but someone with a background in Ecology, I had a few questions regarding the representativeness of these networks, could you please clarify the criteria used for selecting these networks? Specifically:

A. What factors were considered (e.g., data quality, structural completeness)?
B. Do the selected networks adequately represent the ecological and climatic diversity of each region?
C. Some climate types, such as tropical rainforest and Mediterranean semi-arid systems, appear to be underrepresented—was there a specific reason for their exclusion?

Clarifying these points would help evaluate the representativeness of the data and the generalizability of the conclusions.

2. The management scenarios in the study assume that the abundance of selected plant or pollinator species is fixed at 2014 levels. While this provides theoretical insight, I would appreciate clarification on the ecological realism of this approach, such as: what real-world actions (e.g., habitat restoration, species translocation) would be needed to sustain fixed abundances over decades? Moreover, while the study concludes that temperate systems may require minimal management could be less robust under longer-term scenarios or in more fragmented landscapes; therefore, this point may benefit from further discussion.

3. The manuscript reports that Mediterranean networks show limited or insignificant responses to management, especially at moderate mutualistic strength. However, this outcome is under-discussed. Could this be due to greater functional

redundancy, phenological flexibility, or network structural buffering? A more explicit ecological interpretation would enhance the explanatory power of the results.

4. About statistical and ecological significance, I have some suggestions. Several results (e.g., ~5% decline in temperate pollinators) are statistically significant ($p < 0.05$), but may have limited ecological impact. The manuscript should distinguish statistical from biological significance, and report effect sizes (e.g., R^2 or MER values with confidence intervals) where appropriate.

5. Table S2 shows that region-specific biophysical parameters (e.g., T_{opt}, σ, β_{opt}) were used to simulate thermal responses of pollinators in tropical, Mediterranean, and temperate zones, which is appropriate. However, it remains unclear whether all species within a region share the same parameters, or whether intra-regional variation (e.g., among tropical specialists vs. generalists) was considered. Given known variability in thermal sensitivity within ecoregions, some discussion or justification of this simplification would strengthen the modeling framework.

The manuscript addresses an important and timely topic using an innovative modeling framework. However, revisions are needed to clarify model assumptions, address representativeness issues, and strengthen ecological interpretation of the findings. Once these concerns are addressed, the manuscript will make a strong contribution to the field.

Minor comments：
1. I recommend ensuring that the description of the formula parameters matches the notation used in the equation itself. Maintaining this consistency would enhance clarity and help readers more easily follow the methodological details.

2. In line 155, the labels “Fig. 2b,c” are missing a space after the comma. For consistency and improved readability, I suggest checking the manuscript for similar issues and ensuring proper spacing after punctuation. Additionally, using a semicolon (e.g., “Fig. 2b, c; Fig S6b, c”) instead of a comma might improve clarity when referring to multiple distinct figures.

3. Lines 463–464: I suggest ensuring consistency in the notation of the Shannon index (H') throughout the manuscript. In particular, the expression in line 464 appears to differ slightly from that in line 472. Aligning these notations with the formula previously introduced would improve clarity and avoid potential confusion for readers.

Version 1:

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Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

EDITORIAL POLICIES AND FORMATTING

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and also in our style and formatting guide Communications Earth & Environment formatting guide .

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In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)

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

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

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear Editor,

I have carefully reviewed the modifications made to the manuscript: “Warming demands extensive tropical but minimal temperate management in plant-pollinator networks” and I consider that the authors have adequately addressed the observations raised in the first round of review.

I agree with the responses and changes incorporated into the text, and therefore I recommend that the manuscript be accepted for publication.

I remain available for any further comments.

Reviewer #2 (Remarks to the Author):

The revised manuscript has addressed all my concerns, and I believe it is now acceptable for publication.

Reviewer #3 (Remarks to the Author):

Comments to the Author
Review for Communications Earth & Environment
Manuscript COMMSENV-25-2335A

The ms is much improved from the earlier submission. With some additional clarification, it will be a useful contribution. I had the following review.

1. I remain concerned about the authors' selection of only 11 plant–pollinator networks from the Web of Life database, particularly regarding the sampling of Mediterranean regions, which may affect the result for the first research question. From a statistical perspective, a minimum of three or more replicates per region is generally required to ensure meaningful representation. The inclusion of only two Mediterranean networks raises the possibility of pseudoreplication and may introduce substantial variability into the overall analysis.

I would suggest two potential approaches to address this issue:

A. The authors could perform rarefaction analysis to demonstrate that sampling effort was sufficient;

B. Alternatively, adding at least one additional Mediterranean network would help substantiate the robustness of the sampling. The dataset provided in this manuscript may be useful in this regard. (Allen-Perkins A, Magrach A, Dainese M, et al. CropPol: A dynamic, open and global database on crop pollination. Ecology. 2022. DOI: 10.1002/ecy.3614)

I consider this a matter that should be resolved to strengthen the study's conclusions.

2. I have noticed an apparent inconsistency between Figure S1 and Figure S2 regarding the representation of the pollinator networks. Although Figure S1 is intended as a schematic illustration, the number of networks depicted seems to differ from that shown in Figure S2, where more pollinator networks appear to be included. Ensuring consistency between these figures, even when one is schematic, would improve clarity and facilitate reader interpretation.

3.L128-130 I would suggest that the authors include a more detailed explanation of the key structural of the plant–pollinator networks in this study—specifically, connectance, web asymmetry, and nestedness. Clarifying the ecological implications of these metrics would significantly enhance the reader's understanding of how network structure influences plant–pollinator interactions under climate warming. If space constraints in the main text are a concern, this information could be presented in a supplementary table.

4. L149 I would appreciate some clarification regarding the term "network stability" as used in this study. Could the authors please specify how they define stability in these plant–pollinator networks? it would be valuable to explain which structures were used to quantify stability. A more explicit definition and description of the stability measures would greatly enhance the interpretability and reproducibility of the findings.

5. Have the authors attempted to compare structural differences—such as connectance, web asymmetry, and nestedness—across the different regions examined? Such a comparative analysis could help clarify how pollinator networks vary geographically under climate warming and provide deeper mechanistic insights into their responses to projected temperature changes. Furthermore, it would strengthen the interpretation of network stability and its regional determinants.

6. L284-286 In tropical ecosystems, the conclusion that climate warming leads to altered pollinator foraging behavior, thereby impairing plant reproductive fitness, requires support from the literature.

7. Since the authors have adopted the Vancouver style of reference citation, it would be preferable to list the cited references in numerical order throughout the text to ensure consistency with this convention.

Minor comments：

1. L46-48 The actual ecological significance represented by the pollination network refers to plant-pollinator interactions. Therefore, I suggest modifying it as follows: “Understanding these responses is critical for plant-pollinator interactions, which form networks that not only influence species reproduction and survival but also underpin ecosystem health and food security—yet are increasingly vulnerable to rising temperatures.”

2. L164 I would recommend avoiding the use of two consecutive parentheses, as the information they contain could be effectively combined into a single set without compromising the intended meaning or clarity. By the way, could you please clarify whether the different colors used in “Fig. 2d…” represent distinct types of information or groupings?

3. L 211 The labels “Fig. S9, Fig. S10” are missing a semicolon. For consistency and improved readability, I suggest checking the manuscript for similar issues.

Version 2:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Bhatia,

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We therefore invite you to revise your paper one last time to edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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

Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

Dear Editor,

I have carefully reviewed all the revisions made to the manuscript and believe that the authors have adequately addressed all the comments and concerns raised during the review process.

I agree with the responses and modifications provided in the manuscript and therefore recommend that it be accepted for publication.

I appreciate the authors' efforts and contributions—Congratulation !

Comments to the Author
Review for Communications Earth & Environment
Manuscript COMMSENV-25-2335

In this manuscript entitled “Warming demands extensive tropical but minimal temperate management in plant-pollinator networks”, Datta and colleagues address a highly relevant question in global change ecology: how warming impacts plant–pollinator networks differently across climatic regions, and whether management strategies should be adapted accordingly. By integrating temperature-dependent species parameters, Earth system climate projections (CMIP6), and mutualistic network dynamics, the study provides timely insights into the effectiveness of conservation strategies in tropical, temperate, and Mediterranean ecosystems.

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Manuscript COMMSENV-25-2335A

The ms is much improved from the earlier submission. With some additional clarification, it will be a useful contribution. I had the following review.

1. I remain concerned about the authors' selection of only 11 plant – pollinator networks from the Web of Life database, particularly regarding the sampling of Mediterranean regions, which may affect the result for the first research question. From a statistical perspective, a minimum of three or more replicates per region is generally required to ensure meaningful representation. The inclusion of only two Mediterranean networks raises the possibility of pseudoreplication and may introduce substantial variability into the overall analysis.

I would suggest two potential approaches to address this issue:

A. The authors could perform rarefaction analysis to demonstrate that sampling effort was sufficient;

B. Alternatively, adding at least one additional Mediterranean network would help substantiate the robustness of the sampling. The dataset provided in this manuscript may be useful in this regard. (Allen-Perkins A, Magrach A, Dainese M, et al. CropPol: A dynamic, open and global database on crop pollination. *Ecology*. 2022. DOI: 10.1002/ecy.3614)

I consider this a matter that should be resolved to strengthen the study's conclusions.

2. I have noticed an apparent inconsistency between Figure S1 and Figure S2 regarding the representation of the pollinator networks. Although Figure S1 is intended as a schematic illustration, the number of networks depicted seems to differ from that shown in Figure S2, where more pollinator networks appear to be included. Ensuring consistency between these figures, even when one is schematic, would improve clarity and facilitate reader interpretation.

3.L128-130 I would suggest that the authors include a more detailed explanation of the key structural of the plant – pollinator networks in this study — specifically, connectance, web asymmetry, and nestedness. Clarifying the ecological implications of these metrics would significantly enhance the reader's understanding of how network structure influences plant – pollinator interactions under climate warming. If space constraints in the main text are a concern, this information could be presented in a supplementary table.

4. L149 I would appreciate some clarification regarding the term "network stability" as used in this study. Could the authors please specify how they define stability in these plant–pollinator networks? it would be valuable to explain which structures were used to quantify stability. A more explicit definition and description of the stability measures would greatly enhance the

interpretability and reproducibility of the findings.

5. Have the authors attempted to compare structural differences—such as connectance, web asymmetry, and nestedness—across the different regions examined? Such a comparative analysis could help clarify how pollinator networks vary geographically under climate warming and provide deeper mechanistic insights into their responses to projected temperature changes. Furthermore, it would strengthen the interpretation of network stability and its regional determinants.

6. L284-286 In tropical ecosystems, the conclusion that climate warming leads to altered pollinator foraging behavior, thereby impairing plant reproductive fitness, requires support from the literature.

7. Since the authors have adopted the Vancouver style of reference citation, it would be preferable to list the cited references in numerical order throughout the text to ensure consistency with this convention.

Minor comments：

1. L46-48 The actual ecological significance represented by the pollination network refers to plant-pollinator interactions. Therefore, I suggest modifying it as follows: “Understanding these responses is critical for plant-pollinator interactions, which form networks that not only influence species reproduction and survival but also underpin ecosystem health and food security—yet are increasingly vulnerable to rising temperatures.”

2. L164 I would recommend avoiding the use of two consecutive parentheses, as the information they contain could be effectively combined into a single set without compromising the intended meaning or clarity. By the way, could you please clarify whether the different colors used in “Fig. 2d…” represent distinct types of information or groupings?

3. L 211 The labels “Fig. S9, Fig. S10” are missing a semicolon. For consistency and improved readability, I suggest checking the manuscript for similar issues.

Dear Editor,

I have carefully reviewed all the revisions made to the manuscript and believe that the authors have adequately addressed all the comments and concerns raised during the review process.
I agree with the responses and modifications provided in the manuscript and therefore recommend that it be accepted for publication.

I appreciate the authors' efforts and contributions—Congratulation！

Point-by-point response: Warming demands extensive tropical but minimal temperate management in plant-pollinator networks

Dear reviewers,

We deeply appreciate the thoughtful and encouraging comments made by all the reviewers. We have carefully addressed each comment and have revised the manuscript to reflect the suggested improvements. In doing so, we believe that the clarity, rigor, and overall quality of our work have been substantially enhanced. Below, we present a point-by-point response to each comment. For clarity, the reviewers' comments are shown in **blue** and followed by our responses in **black**. Where applicable, we also indicate how and where the manuscript has been updated to incorporate these changes.

We have also carefully reviewed the entire manuscript and made minor refinements to improve clarity and readability. All such minor changes are documented in the Track Changes version.

Response to Reviewer 1

In the manuscript “Warming demands extensive tropical but minimal temperate management in plant-pollinator networks”, Datta et al. test whether plant–pollinator networks exhibit different responses to warming and management interventions depending on their ecological context, by comparing and simulating temperature changes across three regions. They did a good job, and their results were interesting. However, it is necessary for the authors to revise and include definitions of some key concepts within the introduction and throughout the manuscript to provide greater coherence to the study. I offer some comments, questions, and suggestions below that may help improve the manuscript.

Thank you for your valuable review of our study and for finding our results interesting. We also appreciate your constructive comments, questions, and suggestions to improve the manuscript. We have revised our manuscript accordingly, and a detailed point-by-point response to each of your comments is provided below.

Major Comments

It is necessary for the authors to clearly define some of the key concepts mentioned in the introduction and to link them with research questions and the discussion. This would provide the reader with better context regarding the relevance of the study. For example, it is not clear what is meant by a "management strategy." Although four management strategies are mentioned (Single Plant, Multiple Plants, Single Pollinator, and Multiple Pollinators), their significance is not defined in the introduction. This makes it difficult to follow the manuscript’s logic across its sections (introduction, results, and discussion). I suggest establishing this connection early on to highlight the importance of this type of research.

Thank you for pointing this out. In the revised manuscript, we have clarified the key concepts of this study in the Introduction to better link them with the research questions and the discussion. Specifically, for “Management Strategies,” we have introduced a clear definition and described the four types tested in this study: single-pollinator, multi-pollinator, single-plant, and multi-plant management (Lines 69-73).

We have also added examples of targeted conservation actions (Lines 43-44) to improve coherence in the introductory paragraph and introduced the concept of “active management” (Lines 83-85) to enhance the logical flow and ensure consistent framing throughout the manuscript. Below, we include the revised manuscript text to facilitate the review process:

Revised manuscript text lines 70-76: “This thermal sensitivity, which varies across ecoregions, suggests the need for region-specific management strategies, as generalized approaches may fail to address the distinct challenges posed by different ecological and climatic conditions. Management strategies are direct interventions aimed at conserving the abundance of plant or pollinator species under environmental stress, either by targeting individual keystone species (single-species management) or a broader subset of interacting species within a network (multi-species management). Furthermore, while a few studies have explored the benefits of single-species management, the potential of multi-species management approaches has largely been overlooked in the context of promoting network persistence under different climate scenarios.”
Revised manuscript text lines 43-44: “While targeted conservation actions, such as species habitat protection and population management, can potentially mitigate biodiversity loss, the effectiveness of these measures hinges on our ability to predict how ecological systems will respond to environmental changes.”
Revised manuscript text lines 88-90: “Such interspecific variability suggests that active management, whereby deliberate interventions such as the conservation of target plant or pollinator species abundance to buffer ecosystems from environmental stressors, may produce different outcomes depending on the region.”

I find a lack of structure in the introduction, results, and discussion sections. This lack of organization made the manuscript difficult to follow, so I suggest reorganizing these sections to improve clarity.

We have reorganized the Introduction, results, and discussion sections to make the structure better and also added the points raised by reviewers to make it concise and clear.
We have mentioned it thoroughly in your following comments.

Are the statements in lines 111, 151, 175, and 212 logically aligned with the content they introduce? I perceive a lack of connection between sections; some parts seem to address methodological aspects (even though there is already a dedicated methods section), while in others, it is unclear whether results are being presented, discussed, or both.

We appreciate the reviewer’s observation regarding the alignment and flow of statements in Lines 111, 151, 175, and 212. These lines refer to subsection headings that we have newly added within the *Results and Discussion* section (starting at Line 113), which did not exist in

the earlier version of the manuscript. We agree that clearer alignment between the headings and their content is important for readability and logical flow.

Accordingly, we have rephrased these subsection headings to more accurately reflect the analyses they introduce: Line 126 (Regional variation in temperature projections and network structure), Line 156 (Differential future warming scenarios and their impact on pollinator populations), Line 182 (Efficacy of different management strategies under warming scenarios across ecoregions), and Line 219 **(**Pollinator abundance distribution and evenness shifts under management strategies).

We have also improved the section's flow by reorganizing the opening portion of the *Results and Discussion* (Lines 113-125). This now begins with an introductory paragraph that sets the overall context and outlines the sequence of analyses, followed by the revised subsections in a logical order that connects the results with their discussion.

It is important to note that because the "Methods" section appears at the end of typeset articles in *Communications Earth & Environment*, adding a 1-2 line summary of the key methods relevant to each subsection in the "Results and Discussion" is critical to orient the reader and describe how the results were obtained.

The conclusions section seems repetitive in relation to what has already been discussed in the manuscript. Beyond stating that ecosystems may benefit in different ways depending on the management strategies, what do these results tell us from a biological perspective? Biological implications, such as the decline of pollinators or the importance of certain key plant species, as well as the dynamics among the three regions, could be addressed through the approach proposed in your research. I suggest reframing the conclusions based on the two research questions posed and exploring these elements.

We thank the reviewer for this valuable suggestion and agree that the conclusions would be strengthened by explicitly discussing the biological implications of our findings. In the revised manuscript, we have added specific explanations of how different management strategies influence pollinator-plant interactions in each ecoregion and how these effects translate into ecological benefits or limitations (Lines 285-295). Furthermore, to broaden the scope and future utility of our work, we have included a section on potential future research directions (Lines 306-314).
We have also reframed the conclusions in line with the two research questions originally posed, ensuring that the conclusions not only summarize results but also emphasize their biological meaning and practical relevance for management strategies of species.

Revised manuscript text lines 285-295: “In tropical ecosystems, where most pollinators are adapted to narrow thermal ranges, even modest warming may exceed optimal foraging temperatures, reduce floral visitation, and impair plant reproduction. The resulting dependence on a few dominant species increases the risk of cascading species losses if those key taxa decline. Multi-species, habitat-focused management in these systems enhances both abundance and evenness by extending the diversity and continuity of floral resources, thereby supporting both specialist and generalist pollinators. In contrast, mediterranean regions face peak summer warming that can shorten flowering periods, reduce nectar production, and create seasonal resource bottlenecks, particularly for species with short activity periods. Temperate regions, with broader thermal tolerances and higher baseline evenness, are better able to sustain foraging activity and reproductive output despite warming.”

Revised manuscript text lines 306-314: “Furthermore, with their 1° spatial resolution, the climate projections employed may miss microclimatic variability40, for example, contrasts between forest edges and interiors, which can substantially influence localized responses and overall ecosystem resilience. Incorporating spatially explicit land-use and land-cover data could further refine predictions by capturing habitat fragmentation and landscape heterogeneity, which may influence network dynamics and management outcomes. Additionally, the use of region-averaged biophysical parameters does not account for potential intra-regional variation in species’ thermal sensitivities, which could influence local-scale projections.”

It would be advisable to consider the use of a repository to store the data used and generated in this research. This would promote the reproducibility of the results and allow future studies to utilize this data for additional or comparative analyses

We agree. To promote the reproducibility of the results and allow future studies to utilize this data for additional or comparative analyses, we have stored the data in Zenodo, and its link is now included in the Data Availability section.

Link- https://doi.org/10.5281/zenodo.16934435

Minor comments

L31In this context, would the use of the term ‘negligible’ be correct?

We agree that the term “negligible” may have implied a complete lack of effect, which was stronger than intended. To better reflect our results—where small but measurable changes were observed—we have replaced “negligible” with “marginal” (Line 32). This term more accurately captures the limited yet non-zero gains observed under management in temperate networks (Fig. 3c) and aligns better with the quantitative outcomes presented.

L43 Which ones?

We thank the reviewer for pointing this out. In response, in Line 43-44, we have revised the sentence to include specific examples of targeted conservation actions—namely, species habitat protection and population management—to provide clearer context for “these measures.”

L46 I suggest changing 'mutualistic plant-pollinator networks' to 'interactions between plants and their pollinators'; remember that networks are the analytical tools that allow us to interpret, to some extent, the interaction between the two groups.

We understand the reviewer's point of view. In the revised sentence, L47, we have removed “mutualistic” and now refer to “plant-pollinator interaction networks” to more accurately convey that we are discussing the structured set of interactions between plants and pollinators.

L49 or floral visitors

In L50, we have revised the sentence to specify that ectothermic insect pollinators are the focus of our study and to clarify their relationship to the broader group of floral visitors.

L52 The terms ‘specific management strategies’ and ‘uniform approaches’ require clarification — what do they refer to, and which ones specifically? From this point onward, such concepts should be clearly defined and integrated logically into the text, or at least briefly contextualized.

In this context, “specific management strategies” refers to region-specific interventions tailored to the ecological and climatic characteristics of each network, while “uniform approaches” refers to the application of the same management strategy across all regions regardless of context. To improve clarity, we have replaced the term “uniform” with “generalized” in the main text (Line 68). Additionally, we have revised L67-78 to more clearly define and integrate these concepts into the logical flow of the manuscript.

L68: What exactly is an ecoregion? Does this study consider it as a relatively large biogeographic unit?

In this context, we do not use “ecoregion” to refer to large, formally defined biogeographic units. Rather, we use it to describe regions that share similar climatic regimes, such as those defined by Köppen climate classifications, which we adopt in our study to group networks by their temperature variability and ecological context in Lines 336-339.
Also, to avoid confusion with formally defined ecoregions, we have replaced the term “ecoregions” with “climatic regions” throughout the manuscript.

L69 Again, concepts such as ‘multi-species management strategies’ and ‘single-species approaches’ appear in the introduction without a clear definition. I suggest that each concept be briefly defined, as this will provide the reader with more context.

We have added brief definitions of both single-species and multi-species management strategies in L69-73 of the Introduction.

Revised manuscript text lines 69-73: “Management strategies are direct interventions aimed at conserving the abundance of plant or pollinator species under environmental stress, either by targeting individual keystone species (single-species management) or a broader subset of interacting species within a network (multi-species management).”

L71 see comment L69, ‘current “universal” management strategies.

We acknowledge that the phrase “current ‘universal’ management strategies” may have implied the existence of widely implemented real-world interventions, whereas our intention was to refer to generalized or idealized strategies often assumed in theoretical and modeling studies. To address this, we have revised the wording in L76-77 to “theoretically generalized management strategies,” which more accurately reflects the conceptual nature of these approaches in the literature.

L65-73 This paragraph is important, but context should be provided, and what differentiates it from other studies should be highlighted.

We have revised the paragraph (L64–78) to provide clearer context by highlighting how previous modeling studies often relied on fixed mean temperature inputs and overlooked temporal variability, thereby limiting their applicability across regions. In contrast, our work

emphasizes the importance of region-specific thermal sensitivities and explicitly compares single- versus multi-species management approaches, underscoring how our study differs from earlier efforts.

L78 Active management of what?

We have clarified sentence L83-85 to specify that “active management” refers to direct interventions aimed at maintaining or stabilizing the abundance of plant or pollinator species, such as those modeled through single- or multi-species management strategies in our study.

L 86-89 For this idea to be coherent within the text, the concept of ‘active management’ should first be defined.

To improve clarity and coherence, we have added a sentence defining “active management” in L83-85. This addition aligns with earlier references to single- and multi-species strategies and ensures consistency throughout the manuscript.

L101-106 This part should be integrated into the discussion. I suggest adding it to the Discussion section.

The section previously in the Introduction has been removed and reworked for clarity. The revised text has been placed in the Conclusion section (Lines 281-283), where it now synthesises the main findings and links them to their conservation implications.

L110. Does the results and discussion start on this line? If so, please add the title ‘Results and Discussion’.

We have updated the manuscript by adding title ‘Results and Discussion’ (Line 113).

L307 – 310 In this line, the concept of ‘management interventions’ is described. See previous comments and the suggestion to provide definitions for this type of concept starting from the introduction.

We have described the concept of ‘management interventions’ in the introduction (L69-73).

L397 - 400 This information should appear initially in the introduction.

We have added those (L69-73, L82-85)

L397 - 427 This information is important in the Methods section, but I suggest extracting the most important parts (e.g., the four strategies in L399) and adding them in a 5–7 line paragraph in the Introduction. This would help and highlight the significance of your work.

We have added those in the introduction (L69-73).

L 474 For the statistical analyses carried out in this study, was any program or software used? For example, R or Python. If so, please mention it in this section."

We have used Python for the statistical analyses carried out in this study. We have included the information in L520.

L111 – 131 It may be the way these two paragraphs are written, but these lines read more like methodological information rather than results. I suggest organizing this section more clearly within the manuscript.

We have actually started with some methodological details to build the contexts (L114-125). We have reorganized the sections to improve readability.

Revised manuscript text lines 114-125: “Understanding how plant-pollinator networks respond to climate change requires a detailed analysis of network structural properties and the environmental factors that shape them. In this study, we quantified the impact of differential terrestrial warming in 11 plant-pollinator networks (Fig. S1) across tropical, temperate, and mediterranean regions, assessing changes in mean pollinator abundance and the implications for ecosystem health under different climate scenarios. We selected these three ecoclimatic regions (Fig. 1d) based on the Köppen climate classification system, with temperate oceanic climate (Cfb) representing temperate regions, tropical savanna climate with a dry winter/dry summer (Aw) representing tropical regions, and hot summer mediterranean climate (Csa) representing mediterranean regions. The following sections examine how variations in network structure and regional temperature dynamics together shape potential responses to warming and the efficacy of different management strategies.”

L138 References are missing.

References have been added at L138. We have also carefully reviewed the entire manuscript and ensured that all figures and tables are appropriately cited where needed.

L144- 148 The arguments in these lines are valid, but which studies are they being contrasted with? What other research addresses the variability of thermal changes and management strategies?

While no previous study has simultaneously incorporated real-world temperature projections and management strategies into a network-dynamics framework, we note that one theoretical study has explored network dynamics under a fixed temperature range (see L64-67), and another examined the role of single-species management strategies in shifting tipping points without accounting for temperature (see Line 74). These studies were referenced in the Introduction, but to improve clarity, we have now added the references to make the contrast more explicit in the relevant section of the text.

L165-167 References are missing.

We have added the references.

L18 Does it show statistically significant variation?

We believe the reference to Line 18 may be a typographical error, as that line pertains to author affiliations. However, we understand the reviewer is likely referring to Line 180. We confirm that statistical significance for pollinator decline across all regions has already been presented in Lines 211-217.

L207 – 210 What explains the pattern of this result? Biologically, is it solely driven by temperature, or is it a synergistic effect between temperature and the abundance of both levels (plants and pollinators)

As the networks used here are binary (i.e., presence or absence of interaction only), the observed differences are not due to species abundance *per se*, but rather emerge from the interplay between climatic conditions and mutualistic network architecture.

L245 -246 It is an interesting statement that would gain greater importance if several key concepts were defined from the introduction. I recommend linking it to the two research questions

We agree that the statement gains greater importance when placed in the context of our research aims. The relevant concepts, such as thermal sensitivity, management strategies, and network structure, are defined in the revised Introduction (L58-61, L67-73, L82-85).

L264-267 This point should be more thoroughly developed in the introduction

We have included the key concepts of management strategies in the introduction (L69-73).

L290 Throughout the manuscript, the term 'ecosystem health' is used. Specifically, what does this refer to in the context of your research?

In the context of our study, “ecosystem health” refers to the stability and functioning of mutualistic plant-pollinator networks, which we quantify using mutualistic strength, as first conceptualized by Jiang et al[1]. This metric is explicitly defined and discussed in the Methods section (Lines 376-378), where we explain how it reflects the capacity of the bipartite network to support species’ abundance. We have also included the relevant references at L192 to ensure clarity.

Response to Reviewer 2

The manuscript focuses on the impact of climate warming on plant-pollinator networks. Through integrated modeling, it analyzes responses of these networks across different ecoregions under warming conditions, addressing urgent ecological questions regarding ecosystem resilience and management strategies. This provides a theoretical foundation for region-specific adaptive strategies in biodiversity conservation.
Comments
1.Although the model incorporates complex parameters, it overlooks:
(i) Evolutionary adaptation processes (e.g., thermal niche shifts, genetic regulation)
(ii) Lifecycle/reproductive cycle variations affecting species' thermal responses
(iii) Behavioral differences among pollinators (e.g., thermoregulation, migratory traits).
These exclusions may directly impact results (Lines 270-279).
2.The 1°×1° ESM simulations ignore microclimatic heterogeneity (e.g., forest edge vs. interior), reducing local-scale predictive accuracy (Line 280)

We sincerely thank the reviewer for their time and effort in reviewing our manuscript and for raising thoughtful points that enhance the clarity and scope of our work. The aspects highlighted, namely, the exclusion of evolutionary adaptation processes (e.g., thermal niche shifts, genetic regulation), lifecycle or reproductive cycle variations, behavioral differences among pollinators, and fine-scale microclimatic heterogeneity, are indeed noted as limitations and have been clearly stated in the revised versions of the Conclusions (L301-308).

We acknowledge that, epistemically and from a data availability perspective, these limitations are inherent when modeling complex plant-pollinator networks across multiple ecoregions. The absence of spatially and temporally resolved datasets capturing these processes at regional to continental scales precludes their direct integration into the current modeling framework. Accordingly, we have explicitly identified these aspects as important directions for future work and have reframed our conclusions to reflect these limitations (L308-310). At the same time, we note that within the scope of these constraints, the study’s

experimental design, coupled with the datasets selected and the statistical analyses employed, was deliberately structured to ensure robustness and reproducibility of results across ecoregions under the evaluated warming scenarios. The use of the latest generation climate model inputs, ecological networks from open-source data repositories, and consistent statistical testing provides a reproducible framework that can be applied to other regions under similar conditions. While the omission of certain biological and microclimatic processes may influence absolute projections at fine scales, the comparative patterns and emergent trends identified remain robust within the defined scope and data resolution of this study.

Revised manuscript text lines 301-308: “While our network-based modeling approach provides essential insights, it comes with certain limitations. Expanding these models to include dynamic, time-dependent interactions would require comprehensive data on life stage-specific responses, thermoregulation, acclimatization, and adaptation—factors that are challenging to scale but could be crucial for accurately predicting ectotherm population dynamics. Furthermore, with their 1° spatial resolution, the climate projections employed may miss microclimatic variability, for example, contrasts between forest edges and interiors, which can substantially influence localized responses and overall ecosystem resilience.”

Revised manuscript text lines 308-310: “Incorporating spatially explicit land-use and land-cover data could further refine predictions by capturing habitat fragmentation and landscape heterogeneity, which may influence network dynamics and management outcomes.”

3.While advocating multi-species management, the manuscript fails to provide operational cases or feasibility analyses (e.g., implementing "species abundance maintenance" or criteria for selecting the top 10% managed species), limiting translational relevance (Lines 401-427).

We agree that providing operational considerations would improve the translational relevance of our findings. In our simulations, the “top 10% most connected species” were identified based on degree calculated from empirical plant–pollinator interaction networks. Hence, our recommendations and results can be readily applied to any system whose interaction network has been established (e.g., the many systems included in Jordi Bascompte’s group ‘Web of Life’ database). Additionally, for systems or geographical locations missing from such databases, the network structure of species interactions can be determined via systematic field surveys (e.g., using environmental DNA collection) and literature reviews. Multi-species abundance maintenance could be operationalized via

habitat restoration that supports multiple target species simultaneously (e.g., diversified floral plantings, hedgerow establishment, sequential blooming species mixes), coupled with long-term monitoring to maintain populations near baseline levels. While our focus here is on illustrating potential outcomes rather than prescribing detailed management protocols, these approaches demonstrate feasible pathways for translating our model framework into conservation planning.
Accordingly, we have incorporated these practical application points into the Methods section (Lines 466-470) to clarify how our simulated management strategies can be operationalized in real-world contexts.

Revised manuscript text lines 466-470: "In field-level conservation practices, the top 10% most connected species can be identified through field-based pollination network mapping and trait analysis, with their populations maintained via multi-species habitat restoration (e.g., diversified floral plantings, hedgerows, sequential bloom schemes) and long-term monitoring to sustain abundance near baseline levels."

4.Bonferroni-corrected *t*-tests lack variance/confidence interval disclosure, hindering robustness assessment. Visual analyses are inadequate (e.g., RAC curve differences across strategies require deeper interpretation) (Lines 442-484).

We have included the 95% confidence intervals in the supplementary table S3 as requested. Also, we have added the Bonferroni-corrected *t*-tests with robustness assessment in Fig. S10.
In addition, recognizing the need for deeper interpretation of RAC curve differences across strategies, we have expanded and reframed the Results section (L227-236, L244-250) to provide this clarification.

Revised manuscript text lines 227-236: "In tropical networks, implementing pollinator management strategies, both single and multi-pollinator, mirror the without-management RAC (Fig. 4a; Fig. S11a) with marginally improves evenness (Fig. 4d; Fig. S12a) at low mutualistic strength (γ_0 = 1; evenness index difference 0.01) whereas at high mutualistic strength (γ_0 = 3), pollinator management reduces evenness (evenness index difference -0.01 for single pollinator management and -0.03 for multi pollinator management). In contrast, plant management strategies (single and multi-plant) produce a more gradual slope, indicating improved evenness at both high (γ_0 = 3; Pielou evenness difference = 0.06, 0.13 under single and multi-plant management) and low (γ_0 = 1; Pielou evenness difference =

0.15, 0.36 under single and multi-plant management) mutualistic strength through broader support for pollinator populations connected to managed plants."

Revised manuscript text lines 244-250: "Under different management strategies, both mediterranean and temperate networks show high overlap, indicating that implementing either single- or multi-species management does not substantially alter species abundance distributions. Consistent with this, Pielou's evenness index reflects only marginal increases (evenness index difference of 0.03) under plant-focused strategies in mediterranean networks ($\gamma_0 = 1$), with no notable effects observed in temperate networks across different levels of mutualistic strength (γ_0)."

5.Networks M_PL_012 and M_PL_050 (Atlantic islands west of Gibraltar Strait) do not represent typical Köppen Csa ecosystems. Reanalysis with authentic Mediterranean networks is recommended (Lines 121-126).

We thank the reviewer for noting that M_PL_012 and M_PL_050 are located on Atlantic islands west of the Gibraltar Strait and may not fully reflect the thermal extremes of typical mainland Köppen Csa regions due to maritime influence. These networks were selected because they met our inclusion criteria of having 30-100 pollinators, and their climate classification was based on a high-resolution Köppen dataset[2,3], which itself incorporates detailed regional climatology from the Climate Atlas of the Archipelagos of the Canary Islands, Madeira, and the Azores[4]. According to this dataset, both networks are located in transitional zones influenced by both Csa and Csb climatic characteristics.

To address the reviewer's suggestion, we considered another Csa network in our dataset. M_PL_016 is a terrestrial network, which is located in Matalascañas, Spain. M_PL_016 (179 pollinators) was reanalysed for pollinator decline, management strategy effects, relative abundance change (RAC), and evenness. Results from M_PL_016 were consistent with the patterns reported for Csa networks, supporting the robustness of our conclusions.

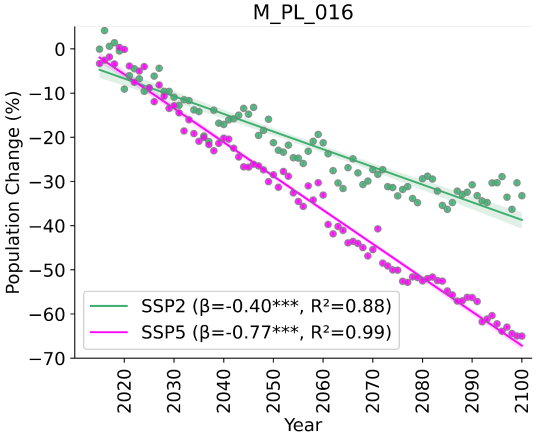

Fig.1: Annual mean pollinator population abundance percentage change of M_PL_016 under warming scenarios SSP2-4.5 and SSP5-8.5 (2015-2100) w.r.t. 2014 population level in moderate environmental health conditions (γ0 = 1.5)

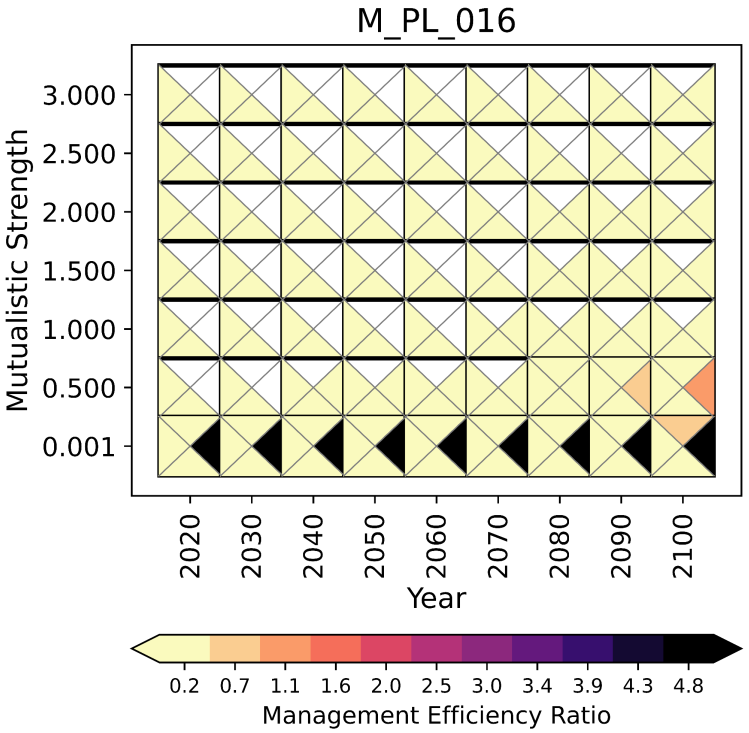

Fig. 2: Management Efficiency Ratio (MER) of mean pollinator abundance at 10-year intervals with respect to varying mutualistic strength for SSP5-8.5 scenario (M_PL_016)

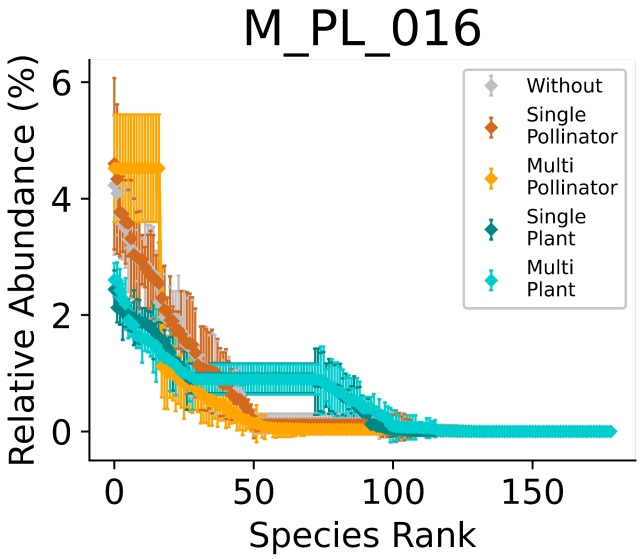

Fig. 3: Rank abundance curve for pollinators at mutualistic strength (γ0 = 1.5) under SSP5-8.5, comparing different management strategies (Single Plant, Multi Plant, Single Pollinator, Multi Pollinator) against without management baseline

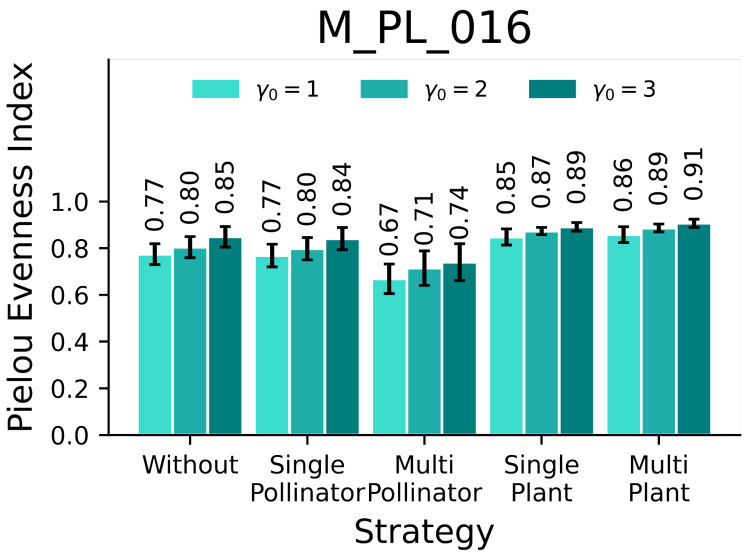

Fig. 4: Variation in the Pielou evenness index under SSP5-8.5 comparing different management strategies against the without-management scenario under different mutualistic strengths.

We have also included all results of M_PL_016 in S13.

We have also replaced M_PL_012 (an island network) from all the figures in the main text with M_PL_058, a terrestrial network, and moved M_PL_012 to the supplementary material.

6.M_PL_040 and M_PL_035 likely fall within the same 1° grid cell, potentially violating spatial independence assumptions (Lines 305-310).

We acknowledge the reviewer for noting the potential proximity of M_PL_040 and M_PL_035 within the same 1° climate grid cell. While it is possible that both sites share similar coarse-scale climate forcing from the ESM data, our results are not solely determined by temperature. The network-based modeling framework also incorporates site-specific plant-pollinator network topology, species identities, and mutualistic strength distributions, all of which differ between these two networks (Fig. 1a-c, Fig. S3).

Consequently, even under similar climatic inputs, differences in network structure produce distinct population dynamics and responses to management interventions.

7.M_PL_026 (island location) cannot typify tropical savanna climate (Aw). Justification for site selection is needed (Lines 301-304).

We understand the reviewer's concern regarding the inclusion of M_PL_026 in the tropical category, as it is situated on an island. The network M_PL_026[5] was recorded on Pinta Island and Santa Cruz Island of the Galápagos archipelago. According to high-resolution Köppen climate classification, both Pinta and Santa Cruz Islands are characterised by a Tropical Savanna (Aw) climate[6], and therefore fit our classification criteria. In addition, we applied a consistent selection threshold across all sites, including only those networks with more than 30 and fewer than 100 pollinator species. This range was chosen because, in networks with very few species (e.g., ~10), actively managing the top 10% most connected species produces negligible differences between single- and multi-species management scenarios, while managing more than 10 species becomes logistically challenging for on-ground implementation.

Response to Reviewer 3

In this manuscript entitled "Warming demands extensive tropical but minimal temperate management in plant-pollinator networks", Datta and colleagues address a highly relevant question in global change ecology: how warming impacts plant–pollinator networks differently across climatic regions, and whether management strategies should be adapted accordingly. By integrating temperature-dependent species parameters, Earth system climate projections (CMIP6), and mutualistic network dynamics, the study provides timely insights into the effectiveness of conservation strategies in tropical, temperate, and Mediterranean ecosystems.
In general, I think this study is well conducted and made good use of future/current global ecological and conservation impacts. While understanding plant-pollinator interactions under introducing the concept of Management Efficiency Ratio (MER) and emphasizing the differential benefits of multi-species management is important and timely, there are several key issues in the current manuscript.

We thank the reviewer for their positive assessment of our study and for recognising the relevance of our approach in addressing global change impacts on plant–pollinator networks. We appreciate their acknowledgment of the novelty of introducing the

Management Efficiency Ratio (MER) and highlighting the differential benefits of multi-species management across climatic regions. We address the key issues raised in the following responses and have revised the manuscript accordingly to improve clarity, methodological detail, and translational relevance.

Major comments:

1. I noticed that the authors selected a total of 11 plant–pollinator networks from the Web of Life database (Table S1), covering three ecoregions. As a non-specialist in Earth science but someone with a background in Ecology, I had a few questions regarding the representativeness of these networks. Could you please clarify the criteria used for selecting these networks? Specifically:

A. What factors were considered (e.g., data quality, structural completeness)?

B. Do the selected networks adequately represent the ecological and climatic diversity of each region? C. Some climate types, such as tropical rainforest and Mediterranean semi-arid systems, appear to be underrepresented—was there a specific reason for their exclusion?

Clarifying these points would help evaluate the representativeness of the data and the generalizability of the conclusions.

Our selection was based on a combination of data quality, network completeness, and suitability for testing our management scenarios across distinct climatic regions as detailed below.

A. Selection criteria:

We considered only empirical networks from the Web of Life database that:

i) Included both plants and pollinators with clearly documented interaction matrices.

ii) Had a network size between 30 and 100 pollinator species to ensure meaningful differences between single- and multi-species management scenarios (very small networks reduce management contrast, while extremely large networks can skew results and pose unrealistic management burdens).

iii) Comprised only insect-plant pollination networks; hummingbird–plant networks present in the Web of Life database were excluded because hummingbirds are endothermic and therefore less sensitive to temperature changes than ectothermic insect pollinators.

B. Representativeness of ecological and climatic diversity:

Within each target climate region (tropical, temperate, mediterranean), we aimed to include networks that covered the available range of ecological contexts while meeting the above

criteria. Although these networks do not capture the complete diversity of each biome, they represent a variety of geographic locations, plant-pollinator compositions, and interaction structures.

C. Underrepresentation of some climate types:
Tropical rainforest and mediterranean semi-arid systems were underrepresented in the Web of Life dataset after applying our inclusion thresholds. In addition, parameter values for these regions were not available in the literature sources we used[7]. Therefore, we restricted our analysis to networks from regions for which both suitable networks existed in the database and the corresponding parameter data were available.

We have added all of this information to the manuscript text in Lines 330-344.

2. The management scenarios in the study assume that the abundance of selected plant or pollinator species is fixed at 2014 levels. While this provides theoretical insight, I would appreciate clarification on the ecological realism of this approach, such as: what real-world actions (e.g., habitat restoration, species translocation) would be needed to sustain fixed abundances over decades? Moreover, while the study concludes that temperate systems may require minimal management could be less robust under longer-term scenarios or in more fragmented landscapes; therefore, this point may benefit from further discussion.

We have revised L456-457, L466-470 to clarify the ecological relevance of our fixed-abundance assumption. The updated text now outlines how such an approach can be grounded in real-world conservation practices, including habitat restoration techniques and long-term monitoring strategies that could help sustain populations of key species over time.

Revised manuscript text lines 456-457: This year was chosen as the baseline because it marks the start of future climate projections in our simulations.

*Revised manuscript text lines 466-470: "*In field-level conservation practices, the top 10% most connected species can be identified through field-based pollination network mapping and trait analysis, with their populations maintained via multi-species habitat restoration (e.g., diversified floral plantings, hedgerows, sequential bloom schemes) and long-term monitoring to sustain abundance near baseline levels."

We agree that temperate systems may behave differently under longer-term scenarios or in more fragmented landscapes. To assess this fully, dynamic network data at multiple time

steps would be required, enabling us to evaluate how temperate systems respond over extended periods. In our current framework, landscape fragmentation is indirectly represented through lower mutualistic strength (γ_0); however, we did not incorporate original land-use land-cover data into the model. Including such spatially explicit habitat information would be a valuable extension for future work.

To address this limitation, we have revised the conclusion (L308-310) to highlight that incorporating LULC data in future models offers an opportunity to further refine predictions and enhance the applicability of our framework.

Revised manuscript text lines 308-310: “Incorporating spatially explicit land-use and land-cover data could further refine predictions by capturing habitat fragmentation and landscape heterogeneity, which may influence network dynamics and management outcomes.”

3. The manuscript reports that Mediterranean networks show limited or insignificant responses to management, especially at moderate mutualistic strength. However, this outcome is under-discussed. Could this be due to greater functional redundancy, phenological flexibility, or network structural buffering? A more explicit ecological interpretation would enhance the explanatory power of the results.

Here, the observed patterns are the result of a complex interplay between multiple factors: the interaction dynamics between plants and pollinators, the distinct temperature time series for each site, and the parameter values used in the simulations. As our networks are binary (presence or absence of interaction only), these differences do not arise from initial abundance values, but from the way climatic forcing interacts with network architecture. To pinpoint the exact ecological mechanisms behind the limited response of Mediterranean networks—such as potential roles of functional redundancy, phenological flexibility, or network structural buffering—would require more extensive datasets, including a larger number of networks from the same climate region and dynamic time-series data. Such information would allow us to disentangle the relative contributions of climatic variability, network structure, and species traits to this phenomenon.

4. About statistical and ecological significance, I have some suggestions. Several results (e.g., ~5% decline in temperate pollinators) are statistically significant ($p < 0.05$), but may have limited ecological impact. The manuscript should distinguish statistical from biological

significance, and report effect sizes (e.g., R2 or MER values with confidence intervals) where appropriate.

We recognize the reviewer's point regarding the distinction between statistical and ecological significance. In response, we have clarified in the manuscript (L167-169) that although temperate pollinator declines are statistically significant, the pollinator decline is relatively small and may not reflect meaningful ecological disruption within the modeled timeframe.

Revised manuscript text lines 308-310: "While these declines are statistically significant (Fig. 2f), their relatively small magnitude (~5% pollinator decline) suggests limited ecological concern in the short term, especially given the stability of overall network structure."

We have reported the R2 value in Fig. 2d,e,f, and the supplementary Fig. S7, and also reported the confidence intervals in Table S3.

5. Table S2 shows that region-specific biophysical parameters (e.g., To , σ, βo) were used to simulate thermal responses of pollinators in tropical, Mediterranean, and temperate zones, which is appropriate. However, it remains unclear whether all species within a region share the same parameters, or whether intra-regional variation (e.g., among tropical specialists vs. generalists) was considered. Given known variability in thermal sensitivity within ecoregions, some discussion or justification of this simplification would strengthen the modeling framework.

We thank the reviewer for raising this important point. In our current framework, all species within a given Köppen climate region share the same set of biophysical parameters (e.g., T_{opt} , σ, β_o) as reported in Table S2. We agree that intra-regional variation in thermal sensitivity exists (e.g., between specialists and generalists) and have clarified in the Methods that capturing this variation would require species-specific parameterisation, which was beyond the scope of the current study but is an important avenue for future work.

We have added this statement to the Methods section (L424-425) for clarity and have also noted it as a limitation in the Conclusions section (L311-313).

Revised manuscript text lines 424-425: "All species within a given Köppen climate region were assigned the same parameter values, based on region-level averages from the compiled dataset."

Revised manuscript text lines 311-313: “Additionally, the use of region-averaged biophysical parameters does not account for potential intra-regional variation in species’ thermal sensitivities, which could influence local-scale projections.”

The manuscript addresses an important and timely topic using an innovative modeling framework. However, revisions are needed to clarify model assumptions, address representativeness issues, and strengthen ecological interpretation of the findings. Once these concerns are addressed, the manuscript will make a strong contribution to the field.

We thank the reviewer for recognising the importance and timeliness of our study and for noting the novelty of our modelling framework. We have carefully revised the manuscript to clarify model assumptions, address questions regarding network representativeness, and expand the ecological interpretation of our results.

Minor comments

1. I recommend ensuring that the description of the formula parameters matches the notation used in the equation itself. Maintaining this consistency would enhance clarity and help readers more easily follow the methodological details.

We appreciate the suggestion and have carefully revised the Methods section to ensure that the parameter descriptions match the notation used in the corresponding equations.

2. In line 155, the labels “Fig. 2b,c” are missing a space after the comma. For consistency and improved readability, I suggest checking the manuscript for similar issues and ensuring proper spacing after punctuation. Additionally, using a semicolon (e.g., “Fig. 2b, c; Fig S6b, c”) instead of a comma might improve clarity when referring to multiple distinct figures.

We have checked all figure citations in the text and added spaces where multiple figure numbers are mentioned. For improved clarity, we have also used semicolons instead of commas when referring to multiple distinct figures.

3. Lines 463–464: I suggest ensuring consistency in the notation of the Shannon index (H') throughout the manuscript. In particular, the expression in line 464 appears to differ slightly from that in line 472. Aligning these notations with the formula previously introduced would improve clarity and avoid potential confusion for readers.

We have aligned these notations to improve clarity and avoid potential confusion for readers.

References:

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2. Santana, J. *et al.* Climate, biogeography, and human resilience in the demographic history of the Canary Islands during the Amazigh period. *Sci. Rep.* **15**, 19485 (2025).
3. Luque Söllheim, Á. L., Máyer Suarez, P. & García Hernández, F. The digital climate atlas of the Canary Islands: A tool to improve knowledge of climate and temperature and precipitation trends in the Atlantic islands. *Clim. Serv.* **34**, 100487 (2024).
4. *Climate Atlas of the Archipelagos of the Canary Islands, Madeira and the Azores.* 80 https://www.ipma.pt/export/sites/ipma/bin/docs/publicacoes/atlas.clima.ilhas.iberico.2011.pdf (2012).
5. McMullen, C. K. Flower-visiting insects of the Galapagos Islands. *Pan-Pac. Entomol.* **69**, 95–106 (1993).
6. Steinitz-Kannan, M., López, C., Jacobsen, D. & Guerra, M. D. L. History of limnology in Ecuador: a foundation for a growing field in the country. *Hydrobiologia* **847**, 4191–4206 (2020).
7. Deutsch, C. A. *et al.* Impacts of climate warming on terrestrial ectotherms across latitude. *Proc. Natl. Acad. Sci.* **105**, 6668–6672 (2008).

Point-by-point response: Warming demands extensive tropical but minimal temperate management in plant-pollinator networks

Dear Reviewers,

We sincerely thank all reviewers for their thoughtful, constructive, and encouraging feedback on our manuscript. We are pleased to note that Reviewers 1 and 2 have accepted the revised version, and we have now carefully addressed the remaining comments from Reviewer 3. The revisions made in response to these suggestions have further improved the clarity, rigor, and overall quality of the manuscript.

Below, we provide a detailed, point-by-point response to each comment. For clarity, the reviewers' comments are shown in **blue**, followed by our responses in **black**. Where applicable, we have also indicated the corresponding line numbers where modifications have been incorporated in the revised manuscript.

We have also carefully reviewed the entire manuscript and made minor refinements to the text to improve clarity and readability. All such minor changes are documented in the Tracked Changes version of the document.

Reviewer #1 (Remarks to the Author):

Dear Editor,

I have carefully reviewed the modifications made to the manuscript: “Warming demands extensive tropical but minimal temperate management in plant-pollinator networks” and I consider that the authors have adequately addressed the observations raised in the first round of review.

I agree with the responses and changes incorporated into the text, and therefore I recommend that the manuscript be accepted for publication.

I remain available for any further comments.

We sincerely thank the reviewer for their careful reading of our revised manuscript and for recognizing the adequacy of our responses and modifications. We are grateful for their positive evaluation and recommendation for acceptance.

Reviewer #2 (Remarks to the Author):

The revised manuscript has addressed all my concerns, and I believe it is now acceptable for publication.

We are grateful to the reviewer for their thoughtful evaluation of our revision and for confirming that their concerns have been resolved. We appreciate their recommendation for acceptance and their constructive feedback throughout the review process.

Reviewer #3 (Remarks to the Author):

Comments to the Author
Review for Communications Earth & Environment
Manuscript COMMSENV-25-2335A

The ms is much improved from the earlier submission. With some additional clarification, it will be a useful contribution. I had the following review.

We thank the reviewer for acknowledging the improvements in the revised manuscript and for recognizing its potential contribution. We appreciate the constructive suggestions provided and carefully address each of the points raised in the detailed review below.

1. I remain concerned about the authors' selection of only 11 plant–pollinator networks from the Web of Life database, particularly regarding the sampling of Mediterranean regions, which may affect the result for the first research question. From a statistical perspective, a minimum of three or more replicates per region is generally required to ensure meaningful representation. The inclusion of only two Mediterranean networks raises the possibility of pseudoreplication and may introduce substantial variability into the overall analysis.

I would suggest two potential approaches to address this issue:

A. The authors could perform rarefaction analysis to demonstrate that sampling effort was sufficient;

B. Alternatively, adding at least one additional Mediterranean network would help substantiate the robustness of the sampling. The dataset provided in this manuscript may be useful in this regard. (Allen‐Perkins A, Magrach A, Dainese M, et al. CropPol: A dynamic, open and global database on crop pollination. Ecology. 2022. DOI: 10.1002/ecy.3614)

I consider this a matter that should be resolved to strengthen the study's conclusions.

We thank the reviewer for this thoughtful and constructive comment, which provides an opportunity to clarify our network selection approach. The study includes 11 plant-pollinator networks selected from the Web of Life database based on specific criteria outlined in the manuscript. These criteria ensured that each network aligned with the availability of

region-specific biological parameters required for the mutualistic network model and included 30-100 pollinator species to ensure representativeness across regions.

Regarding the Mediterranean region, our analysis incorporates three networks—M_PL_012, M_PL_050, and M_PL_058. Among these, M_PL_058 is shown in the main text, while M_PL_012 and M_PL_050 are provided in the Supplementary Information. Thus, the Mediterranean region is represented by three independent networks, not two, consistent with our defined selection framework (L343-348). Within these constraints, all suitable Mediterranean networks available in the Web of Life database were included.

We also appreciate the reviewer's suggestion to consider the CropPol dataset (Allen‐Perkins et al., 2022). While this dataset is indeed valuable, it primarily focuses on crop pollination systems rather than natural bipartite plant–pollinator interaction networks, which are required for our model simulations. Therefore, it could not be incorporated within the current analytical framework. Nonetheless, we fully recognize its importance and potential for future comparative extensions of this work.

2. I have noticed an apparent inconsistency between Figure S1 and Figure S2 regarding the representation of the pollinator networks. Although Figure S1 is intended as a schematic illustration, the number of networks depicted seems to differ from that shown in Figure S2, where more pollinator networks appear to be included. Ensuring consistency between these figures, even when one is schematic, would improve clarity and facilitate reader interpretation.

We thank the reviewer for this helpful observation. To clarify, the tropical (M_PL_035), Mediterranean (M_PL_058), and temperate (M_PL_007) networks are presented in the main text, while the remaining eight networks from these three regions are displayed in Figure S1. Figures S2 and S3 show the network-level characteristics (e.g., connectance, asymmetry, nestedness, and the number of plant and pollinator species) and therefore include information from all 11 networks. The apparent difference arises because Figure S1 illustrates only the eight supplementary networks, whereas Figures S2 and S3 summarize the characteristics of all networks combined. This clarification has now been added to the main text (L124-126).

Revised manuscript Lines 124-126: "The tropical (M_PL_035), Mediterranean (M_PL_058), and temperate (M_PL_007) networks are shown as representative examples, while the

remaining eight networks from these regions are provided in the Supplementary Information."

3.L128-130 I would suggest that the authors include a more detailed explanation of the key structural of the plant–pollinator networks in this study—specifically, connectance, web asymmetry, and nestedness. Clarifying the ecological implications of these metrics would significantly enhance the reader's understanding of how network structure influences plant–pollinator interactions under climate warming. If space constraints in the main text are a concern, this information could be presented in a supplementary table.

We appreciate this thoughtful suggestion. In response, we have expanded the text to briefly explain the key structural metrics—connectance, web asymmetry, and nestedness—and their ecological significance in shaping plant-pollinator interactions under climate warming (L128-132).

Revised manuscript Lines 128-132: "The plant-pollinator networks span a range of species richness levels (Fig. S2) and exhibit some variation in their topological properties (Fig. 1a-c) such as connectance (i.e., density of interactions), asymmetry (i.e., balance of plant and pollinator richness), nestedness (i.e., extent to which specialists interact with subsets of generalists; Fig. S3), all three of which have been linked to stability and persistence."

4. L149 I would appreciate some clarification regarding the term "network stability" as used in this study. Could the authors please specify how they define stability in these plant–pollinator networks? it would be valuable to explain which structures were used to quantify stability. A more explicit definition and description of the stability measures would greatly enhance the interpretability and reproducibility of the findings.

In this study, the term network stability is used in a structural-functional sense. While we calculated network topological parameters (connectance, asymmetry, and nestedness) to describe network structure, the assessment of stability here was based on pollinator abundance and species evenness. These parameters reflect how interaction balance and pollinator community composition respond to warming scenarios, serving as proxies for the network's ability to maintain functional integrity under environmental stress (L152-156). For instance, increased imbalance (i.e., reduced abundance evenness) could promote network instability due to the increased risk of extinction of rare(r) species.

Revised manuscript Lines 152-156: “While projected temperature increases in temperate and mediterranean regions are less extreme than in the tropics, the interaction of seasonal peaks and warming trends poses potential stress periods that could affect pollinator mean abundance and evenness, potentially influencing the overall functional stability of plant-pollinator networks by promoting extinction risk for rare species.”

5. Have the authors attempted to compare structural differences—such as connectance, web asymmetry, and nestedness—across the different regions examined? Such a comparative analysis could help clarify how pollinator networks vary geographically under climate warming and provide deeper mechanistic insights into their responses to projected temperature changes. Furthermore, it would strengthen the interpretation of network stability and its regional determinants.

We thank the reviewer for this insightful suggestion. We have presented the structural differences (connectance, web asymmetry, and nestedness) for all 11 networks in Figure S3. Briefly, we found no statistically significant differences in any of the structural metrics (connectance, web asymmetry, and nestedness) across regions, as mentioned in L132-136 and Table S2 in the revised manuscript. This suggests that the initial regional differences in network structure were not responsible for the disparate climate impacts observed in our simulations. While a region-wise comparative analysis of these metrics under projected warming would indeed provide deeper insights into geographical variation in network structure, such an analysis would require temporally resolved species interaction data across multiple timeframes for a single region. This would allow for tracking how network properties change dynamically with shifting climatic conditions. However, the Web of Life database provides static interaction matrices compiled from single field sampling events, without repeated measurements through time. As a result, the available data do not permit a direct comparison of temporal or climate-driven changes in structural attributes across regions. We have explicitly acknowledged this as a limitation of our study in the revised manuscript (L309-313). At the same time, we recognize that incorporating time-series data on plant-pollinator interactions in future work could be extremely valuable for understanding how network architecture evolves under ongoing climate change.

Revised manuscript Lines 132-136: “Despite this variation, we found no significant regional differences in mean connectance ($p = 0.077$), asymmetry ($p = 0.119$), or nestedness ($p = 0.119$; Fig. S2). Since these networks shared the same baseline topological properties, we sought to determine whether regional differences in projected warming would be sufficient to yield divergent outcomes with respect to both their stability and management.”

6. L284-286 In tropical ecosystems, the conclusion that climate warming leads to altered pollinator foraging behavior, thereby impairing plant reproductive fitness, requires support from the literature.

Citations were added to the manuscript text.

7. Since the authors have adopted the Vancouver style of reference citation, it would be preferable to list the cited references in numerical order throughout the text to ensure consistency with this convention.

The cited references have now been carefully reordered to follow numerical sequence throughout the text, ensuring consistency.

Minor comments：

1. L46-48 The actual ecological significance represented by the pollination network refers to plant-pollinator interactions. Therefore, I suggest modifying it as follows: “Understanding these responses is critical for plant-pollinator interactions, which form networks that not only influence species reproduction and survival but also underpin ecosystem health and food security—yet are increasingly vulnerable to rising temperatures.”

We have revised the sentence accordingly to better emphasize the ecological significance of plant-pollinator interactions (L46-48).
Revised manuscript Lines 46-48: “Understanding these responses is crucial for plant-pollinator interactions, which form networks that sustain species reproduction, ecosystem health, and food security, yet are increasingly threatened by rising temperatures”.

2. L164 I would recommend avoiding the use of two consecutive parentheses, as the information they contain could be effectively combined into a single set without compromising the intended meaning or clarity. By the way, could you please clarify whether the different colors used in “Fig. 2d…” represent distinct types of information or groupings?

We thank the reviewer for this helpful observation. The sentence at L164 has been revised to avoid consecutive parentheses and to improve readability. We also clarify that the different colors used in Fig. 2d represent the three ecoclimatic regions analyzed—tropical, Mediterranean, and temperate—as noted in the figure legend.

3. L 211 The labels “Fig. S9, Fig. S10” are missing a semicolon. For consistency and improved readability, I suggest checking the manuscript for similar issues.

We acknowledge the reviewer for noticing this detail. The missing semicolon between “Fig. S9” and “Fig. S10” has been added, and the manuscript has been thoroughly checked for similar formatting inconsistencies.

Point-by-point response: Warming demands extensive tropical but minimal temperate management in plant-pollinator networks

Dear Reviewers,

We sincerely thank all reviewers for their thoughtful, constructive, and encouraging feedback on our manuscript.

Below, we provide a detailed, point-by-point response to each comment. For clarity, the reviewers' comments are shown in **blue**, followed by our responses in **black**.

REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

Dear Editor,

I have carefully reviewed all the revisions made to the manuscript and believe that the authors have adequately addressed all the comments and concerns raised during the review process.
I agree with the responses and modifications provided in the manuscript and therefore recommend that it be accepted for publication.

I appreciate the authors' efforts and contributions—Congratulation !

We sincerely thank the reviewer for the kind words and for recognizing the improvements made in the revised manuscript. We are grateful for the constructive feedback provided during the earlier round, which helped improve the clarity and overall quality of the manuscript.