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Biotic stability mechanisms in Inner Mongolian grassland

Yonghui Wang, Xiaxia Niu, Liqing Zhao, Cunzhu Liang, Bailing Miao, Qing Zhang, Jinghui Zhang, Bernhard Schmid and Wenhong Ma

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Review timeline

Original submission:	22 December 2019
1st revised submission:	25 March 2020
2nd revised submission:	28 April 2020
Final acceptance:	28 April 2020

Note: Reports are unedited and appear as submitted by the referee. The review history appears in chronological order.

Review History

RSPB-2019-2977.R0 (Original submission)

Review form: Reviewer 1

Recommendation
Major revision is needed (please make suggestions in comments)

Scientific importance: Is the manuscript an original and important contribution to its field?
Good

General interest: Is the paper of sufficient general interest?
Good

Quality of the paper: Is the overall quality of the paper suitable?
Excellent

Is the length of the paper justified?
Yes

Should the paper be seen by a specialist statistical reviewer?
No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.
No

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?
Yes

Is it clear?
Yes

Is it adequate?
Yes

Do you have any ethical concerns with this paper?
No

Comments to the Author
This study used a five-year dataset and explored how biotic and abiotic factors regulated the temporal variability of plant communities across a large spatial extent. They found that the mean and variability of precipitation had contrast effect on community stability, via affecting species variability and synchrony, respectively. While the overall species diversity was weakly related to community stability, the diversity of dominant species was negatively associated to species synchrony and community variability. Moreover, the mean-variance exponent was positively related to variability at both species and community levels. The paper was very well written and easy to follow. The results were clear and interesting.

I have a few suggestions to improve the manuscript.

The Method section can be significantly reduced. The part c (and the associated appendices) presented in detail the theoretical results by Thibaut & Connolly (2013), which in my opinion was unnecessary. I think that this section need simply provide the definition of different properties, e.g. synchrony, species stability, mean-variance exponent, etc, and explain their underlying links by citing Thibaut & Connolly (2013). The part d developed an additive partitioning, which however was not used in their later analysis (though in the appendix). I think that this partitioning was also unnecessary and could be removed. That said, the author can still define the variability and synchrony among dominant species (without such a partitioning), although the finding that the dominant species affect community-wide properties was rather intuitive.

The discussion can be expanded. First, the mean-variance exponent was used to explain community stability, but this exponent itself is a statistical property resulting from lower-level processes. It would be useful to explain why this exponent differs among sites. Second, it was less explained why species variability (at least for the dominant species) decreased with the mean precipitation. Is it because precipitation increased species and community biomass and thereby decreased species variability? This implies the role of overyielding effect, which was mentioned several times in the Introduction and Methods but ignored later.

Minor comments:
L152-155: I suggest to weaken this argument. It is far from trivial to extend results from 3-year data to longer ones. Maybe change it to: "Although two sites had available data for only 3 years, a recent study showed that investigating this relationship using a 3-year dataset can produce similar patterns with that of using longer time series". Moreover, I am not sure that Hector et al. (2010) is the right reference. Did they compare their 3-year results to longer ones? Otherwise, the

recent paper by Craven et al. (2018 Nature Ecology & Evolution) could fit here.
L321-326: Not surprising given their definitions.
L411: This argument should be justified, if the authors prefer to keep it. An appropriate null model is needed to test whether population dynamics are more compensatory or synchronous.
L425-427: Again, the first half of this sentence needs a statistical test. See above.
L431: Another argument that was not supported by the data. I suggest removing it.

Review form: Reviewer 2

Recommendation
Major revision is needed (please make suggestions in comments)

Scientific importance: Is the manuscript an original and important contribution to its field?
Acceptable

General interest: Is the paper of sufficient general interest?
Good

Quality of the paper: Is the overall quality of the paper suitable?
Acceptable

Is the length of the paper justified?
Yes

Should the paper be seen by a specialist statistical reviewer?
No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.
No

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Is it accessible?
Yes

Is it clear?
Yes

Is it adequate?
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Do you have any ethical concerns with this paper?
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within communities, reducing their stability. This is an interesting topic in community ecology, however there are still many points unclear and contradiction. I would make commendation if the authors well deal with my comments below.

Comments to authors:
1. The main conclusion is that community stability was regulated by species synchrony and population dynamics. Hence, the data of time series on species' abundance should be a key information and evidence to support this argument. Why authors omitted this data in the figures. I would see that the authors present the analysis of the time series. What's about the phase difference to explain your argument.
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3. The authors have many contexts referred as man-variance scaling knowledge, but I didn't read any data and figure to relate this Talyor law. How did your data links and advance our present understood of Talyor law?

Decision letter (RSPB-2019-2977.R0)

13-Feb-2020

Dear Dr Wang:

I am writing to inform you that your manuscript RSPB-2019-2977 entitled "Biotic stability mechanisms in Inner Mongolian grassland" has, in its current form, been rejected for publication in Proceedings B.

This action has been taken on the advice of referees, who have recommended that substantial revisions are necessary. With this in mind we would be happy to consider a resubmission, provided the comments of the referees are fully addressed. However please note that this is not a provisional acceptance.

The resubmission will be treated as a new manuscript. However, we will approach the same reviewers if they are available and it is deemed appropriate to do so by the Editor. Please note that resubmissions must be submitted within six months of the date of this email. In exceptional circumstances, extensions may be possible if agreed with the Editorial Office. Manuscripts submitted after this date will be automatically rejected.

Please find below the comments made by the referees, not including confidential reports to the Editor, which I hope you will find useful. If you do choose to resubmit your manuscript, please upload the following:

1) A 'response to referees' document including details of how you have responded to the comments, and the adjustments you have made.
2) A clean copy of the manuscript and one with 'tracked changes' indicating your 'response to referees' comments document.
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Please note that this decision may (or may not) have taken into account confidential comments.

In your revision process, please take a second look at how open your science is; our policy is that *ALL* (maximally inclusive) data involved with the study should be made openly accessible, fully enabling re-use, replication and transparency-- see:
https://royalsociety.org/journals/ethics-policies/data-sharing-mining/
Insufficient sharing of data can delay or even cause rejection of a paper.

Sincerely,
Professor John Hutchinson, Editor
mailto: proceedingsb@royalsociety.org

Associate Editor
Board Member: 1
Comments to Author:
In this manuscript, the authors established a monitoring network consisting of 23 sites across temperate grasslands on the Mongolian plateau, and conducted field surveys and resurveys at each site during 2012 – 2016. Temperate grasslands in central Eurasia are one of the largest grassland ecosystems and have been shown to be vulnerable to climate change. Using these monitoring data, together with a new additive portioning method, the authors explored how species diversity, species synchrony and population dynamics influenced the stability of the community biomass production. The authors found that community stability was independent of species diversity, but was mainly influenced by species synchrony and population dynamics of dominant species. The question addressed here is one of the central issues in community ecology, yet remains controversial since 1970s. The data that the authors sampled and used in the analyses are valuable to address this question. I therefore think that this manuscript has some merits. Two reviewers have reviewed this manuscript, and provided useful comments and suggestions. I would like to reinforce a few points. 1) Both reviewers #1 and #2 pointed out that the biological mechanisms underlying the relationships between species diversity, species synchrony and population dynamics and the community biomass production were not very clearly discussed. 2) As review #1 pointed out, some part of the methods could be shortened. I agree. Moreover, I feel that manuscript is written in a relatively more technical (or mathematical) way. For example, there are so many abbreviations in the text, which makes the test is less readable. I suggest that authors could improve the writing throughout the manuscript so that the text is less technical and easier to read.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)
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The discussion can be expanded. First, the mean-variance exponent was used to explain community stability, but this exponent itself is a statistical property resulting from lower-level processes. It would be useful to explain why this exponent differs among sites. Second, it was less explained why species variability (at least for the dominant species) decreased with the mean precipitation. Is it because precipitation increased species and community biomass and thereby decreased species variability? This implies the role of overyielding effect, which was mentioned several times in the Introduction and Methods but ignored later.

Minor comments:
L152-155: I suggest to weaken this argument. It is far from trivial to extend results from 3-year data to longer ones. Maybe change it to: "Although two sites had available data for only 3 years, a recent study showed that investigating this relationship using a 3-year dataset can produce similar patterns with that of using longer time series". Moreover, I am not sure that Hector et al. (2010) is the right reference. Did they compare their 3-year results to longer ones? Otherwise, the recent paper by Craven et al. (2018 Nature Ecology & Evolution) could fit here.
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L431: Another argument that was not supported by the data. I suggest removing it.

Referee: 2

Comments to the Author(s)
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Comments to authors:
1. The main conclusion is that community stability was regulated by species synchrony and population dynamics. Hence, the data of time series on species' abundance should be a key information and evidence to support this argument. Why authors omitted this data in the figures. I would see that the authors present the analysis of the time series. What's about the phase difference to explain your argument.
2. The whole story is biotic stability mechanisms here, but I still unknow what potential biotic mechanisms work here. Contrarily, authors have written "Precipitation fluctuations synchronized population dynamics within communities, reducing their stability." in abstract. So, I get the message that the community stability was controlled by precipitation fluctuations, isn't it? If so, it is still an abiotic mechanism regulating the community stability.

3. The authors have many contexts referred as man-variance scaling knowledge, but I didn't read any data and figure to relate this Talyor law. How did your data links and advance our present understood of Talyor law?

Author's Response to Decision Letter for (RSPB-2019-2977.R0)

See Appendix A.

RSPB-2020-0675.R0

Review form: Reviewer 1

Recommendation
Accept with minor revision (please list in comments)

Scientific importance: Is the manuscript an original and important contribution to its field?
Good

General interest: Is the paper of sufficient general interest?
Good

Quality of the paper: Is the overall quality of the paper suitable?
Good

Is the length of the paper justified?
Yes

Should the paper be seen by a specialist statistical reviewer?
No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.
No

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?
Yes

Is it clear?
Yes

Is it adequate?
Yes

Do you have any ethical concerns with this paper?
No

Comments to the Author
This revised manuscript presented an interesting study investigating the biotic and abiotic drivers of grassland ecosystem stability along a broad environmental gradient. The authors have addressed all my comments and have carefully revised the manuscript, and I have only two minor comments.

The recent theoretical work by Arnoldi et al. (2019) explored how species abundance may predict their contribution to community stability, which fits well into the current context. So I suggest the authors to incorporate it.

In lines 72-78, I would suggest an inverse order of these three mechanisms to match their popularity in the literature on stability.

Reference:
Arnoldi, J.F., Loreau, M. and Haegeman, B., 2019. The inherent multidimensionality of temporal variability: How common and rare species shape stability patterns. Ecology letters, 22(10), pp.1557-1567.

Review form: Reviewer 2

Recommendation
Accept as is

Scientific importance: Is the manuscript an original and important contribution to its field?
Good

General interest: Is the paper of sufficient general interest?
Good

Quality of the paper: Is the overall quality of the paper suitable?
Good

Is the length of the paper justified?
Yes

Should the paper be seen by a specialist statistical reviewer?
No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.
No

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?
Yes

Is it clear?
Yes

Is it adequate?
Yes

Do you have any ethical concerns with this paper?
No

Comments to the Author
I have no further comments now.

Decision letter (RSPB-2020-0675.R0)

20-Apr-2020

Dear Dr Wang

I am pleased to inform you that your manuscript RSPB-2020-0675 entitled "Biotic stability mechanisms in Inner Mongolian grassland" has been accepted for publication in Proceedings B. Congratulations!

The referee(s) have recommended publication, but also suggest some minor revisions to your manuscript. Therefore, I invite you to respond to the referee(s)' comments and revise your manuscript. Because the schedule for publication is very tight, it is a condition of publication that you submit the revised version of your manuscript within 7 days. If you do not think you will be able to meet this date please let us know.

To revise your manuscript, log into https://mc.manuscriptcentral.com/prsb and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision. You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript and upload a new version through your Author Centre.

When submitting your revised manuscript, you will be able to respond to the comments made by the referee(s) and upload a file "Response to Referees". You can use this to document any changes you make to the original manuscript. We require a copy of the manuscript with revisions made since the previous version marked as ‘tracked changes’ to be included in the ‘response to referees’ document.

Before uploading your revised files please make sure that you have:

1) A text file of the manuscript (doc, txt, rtf or tex), including the references, tables (including captions) and figure captions. Please remove any tracked changes from the text before submission. PDF files are not an accepted format for the "Main Document".

2) A separate electronic file of each figure (tiff, EPS or print-quality PDF preferred). The format should be produced directly from original creation package, or original software format. PowerPoint files are not accepted.

3) Electronic supplementary material: this should be contained in a separate file and where possible, all ESM should be combined into a single file. All supplementary materials accompanying an accepted article will be treated as in their final form. They will be published alongside the paper on the journal website and posted on the online figshare repository. Files on

figshare will be made available approximately one week before the accompanying article so that the supplementary material can be attributed a unique DOI.

Online supplementary material will also carry the title and description provided during submission, so please ensure these are accurate and informative. Note that the Royal Society will not edit or typeset supplementary material and it will be hosted as provided. Please ensure that the supplementary material includes the paper details (authors, title, journal name, article DOI). Your article DOI will be 10.1098/rspb.[paper ID in form xxxx.xxxx e.g. 10.1098/rspb.2016.0049].

4) A media summary: a short non-technical summary (up to 100 words) of the key findings/importance of your manuscript.

5) Data accessibility section and data citation
It is a condition of publication that data supporting your paper are made available either in the electronic supplementary material or through an appropriate repository.

In order to ensure effective and robust dissemination and appropriate credit to authors the dataset(s) used should be fully cited. To ensure archived data are available to readers, authors should include a 'data accessibility' section immediately after the acknowledgements section. This should list the database and accession number for all data from the article that has been made publicly available, for instance:
• DNA sequences: Genbank accessions F234391-F234402
• Phylogenetic data: TreeBASE accession number S9123
• Final DNA sequence assembly uploaded as online supplemental material
• Climate data and MaxEnt input files: Dryad doi:10.5521/dryad.12311
NB. From April 1 2013, peer reviewed articles based on research funded wholly or partly by RCUK must include, if applicable, a statement on how the underlying research materials – such as data, samples or models – can be accessed. This statement should be included in the data accessibility section.

If you wish to submit your data to Dryad (http://datadryad.org/) and have not already done so you can submit your data via this link
http://datadryad.org/submit?journalID=RSPB&manu=(Document not available) which will take you to your unique entry in the Dryad repository. If you have already submitted your data to dryad you can make any necessary revisions to your dataset by following the above link. Please see https://royalsociety.org/journals/ethics-policies/data-sharing-mining/ for more details.

6) For more information on our Licence to Publish, Open Access, Cover images and Media summaries, please visit https://royalsociety.org/journals/authors/author-guidelines/.

Once again, thank you for submitting your manuscript to Proceedings B and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Sincerely,
Dr John Hutchinson, Editor
mailto: proceedingsb@royalsociety.org

Associate Editor
Board Member
Comments to Author:
Thank for submitting the revised manuscript, which was reviewed by the same two reviewers. Both reviewers thought that the authors have addressed their concerns in the revision. I agree with them. However, I think the authors should may much attention to their writing of the manuscript. I think there are many grammar mistakes and unclear sentences in the writing throughout the manuscript. One of the reviewers also point this out. I therefore suggest that the

authors should check and improve their writing very carefully throughout the manuscript. It can't be acceptable if the unclear writing remains.

Line 51, change variation to variations
Line 71, remove "more"
Line 82, change scale to scales
Line 97, conditions
Lines 104 – 106, consider to revise this sentence. Do you mean "the temperature grassland in Inner Mongolia is a typical part of the Eurasian grassland biome"?
Lines 106 – 109. Consider to rephrase this sentence and separate it into two. In this region, precipitation is the dominant driver of biomass, species richness and species composition in communities. Observational studies have shown that precipitation in Inner Mongolian grassland has dramatically changed during the past decades, while the ecological consequences of precipitation changes are still not fully understood. "
Line 117, consider to remove "biotic mechanisms assigned to the"
Lines 150 – 154. I think this should be separated into two sentences.
Line 161, material should be materials
Lines 192 – 193, do you mean Shannon-Wiener diversity?
Line 193, remove index
Line 199, change "trend of year" to "temporal trend"
Lines 201 – 202, consider to rephrase the sentence to: "The Taylor's power law [17] was employed to estimate the mean–variance scaling coefficient"
Line 259, change overyielding to overyielding effect
Lines 271 – 276, these two sentences read odd. Maybe consider to change them into the following: To test the hypotheses that the weighted average species temporal CV and species synchrony directly influenced the community temporal CV, we conducted path analysis to relate the community temporal CV to climatic factors, species diversity indices and biotic stability mechanisms using SEMs. Our model of path analysis had a total explanatory power of 0.87.
Line 362, lack of
Lin 369, root systems
Line 390, change are affecting to affect
Line 397, dynamics

Reviewer(s)' Comments to Author:

Referee: 2

Comments to the Author(s).
I have no further comments now.

Referee: 1

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Author's Response to Decision Letter for (RSPB-2020-0675.R0)

See Appendix B.

Decision letter (RSPB-2020-0675.R1)

28-Apr-2020

Dear Dr Wang

I am pleased to inform you that your manuscript entitled "Biotic stability mechanisms in Inner Mongolian grassland" has been accepted for publication in Proceedings B.

You can expect to receive a proof of your article from our Production office in due course, please check your spam filter if you do not receive it. PLEASE NOTE: you will be given the exact page length of your paper which may be different from the estimation from Editorial and you may be asked to reduce your paper if it goes over the 10 page limit.

If you are likely to be away from e-mail contact please let us know. Due to rapid publication and an extremely tight schedule, if comments are not received, we may publish the paper as it stands.

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Electronic supplementary material:
All supplementary materials accompanying an accepted article will be treated as in their final form. They will be published alongside the paper on the journal website and posted on the online figshare repository. Files on figshare will be made available approximately one week before the accompanying article so that the supplementary material can be attributed a unique DOI.

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with the press until the date of publication. Please visit https://royalsociety.org/journals/ethics-policies/media-embargo for more information.

Thank you for your fine contribution. On behalf of the Editors of the Proceedings B, we look forward to your continued contributions to the Journal.

Sincerely,
Proceedings B
mailto: proceedingsb@royalsociety.org

Appendix A

Responses to Comments

##

Associate Editor
Board Member: 1
Comments to Author:
In this manuscript, the authors established a monitoring network consisting of 23 sites across temperate grasslands on the Mongolian plateau, and conducted field surveys and resurveys at each site during 2012 – 2016. Temperate grasslands in central Eurasia are one of the largest grassland ecosystems and have been shown to be vulnerable to climate change. Using these monitoring data, together with a new additive portioning method, the authors explored how species diversity, species synchrony and population dynamics influenced the stability of the community biomass production. The authors found that community stability was independent of species diversity, but was mainly influenced by species synchrony and population dynamics of dominant species. The question addressed here is one of the central issues in community ecology, yet remains controversial since 1970s. The data that the authors sampled and used in the analyses are valuable to address this question. I therefore think that this manuscript has some merits. Two reviewers have reviewed this manuscript, and provided useful comments and suggestions. I would like to reinforce a few points. 1) Both reviewers #1 and #2 pointed out that the biological mechanisms underlying the relationships between species diversity, species synchrony and population dynamics and the community biomass production were not very clearly discussed. 2) As review #1 pointed out, some part of the methods could be shortened. I agree. Moreover, I feel that manuscript is written in a relatively more technical (or mathematical) way. For example, there are so many abbreviations in the text, which makes the test is less readable. I suggest that authors could improve the writing throughout the manuscript so that the text is less technical and easier to read.

Response: Thanks for your valuable comments and suggestions. Now, this manuscript has been totally revised accordingly. Briefly:
1) We revised the Discussion and focused on the crucial effect of dominant species on community stability. These discussions are supported by newly added results in Supporting Information. These results showed that the biomass production of dominant species is more stable than that of other species in a fluctuating environment and can be further stabilized by high precipitation because the stimulation of precipitation on the mean biomass of dominant species is stronger than that on the standard deviation, thus reducing the standard deviation-to-mean ratio (Lines 360–371). In addition, we also discuss that our results add evidence for the view that few dominant species can drive species synchrony in ecosystems with high unevenness and that our new partitioning method provides a tool to quantify the effects of species groups such as our dominant one on synchrony in other ecosystems (Lines 389–402).
2) We shortened the Method section by removing mathematical derivations (Lines 174–195). The detailed mathematical derivations can still be found in Supporting Information, which should be helpful for readers to understand how the community temporal coefficient of variation (CV, inverse of stability) can be related to biotic stability

mechanisms and how these biotic mechanisms can be partitioned among different species groups. In addition, in this revised version, we did not use abbreviations, except for the CV and SEMs, to make an easier read.

##########

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)
This study used a five-year dataset and explored how biotic and abiotic factors regulated the temporal variability of plant communities across a large spatial extent. They found that the mean and variability of precipitation had contrast effect on community stability, via affecting species variability and synchrony, respectively. While the overall species diversity was weakly related to community stability, the diversity of dominant species was negatively associated to species synchrony and community variability. Moreover, the mean-variance exponent was positively related to variability at both species and community levels. The paper was very well written and easy to follow. The results were clear and interesting.

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Response: In this revised version, we shortened the Method section by removing mathematical derivations in part c and d and only kept the definitions of biotic stability mechanisms (estimated with all species all only dominant species). Please see Lines 174–195 for details. The detailed derivations can still be found in Supporting Information.

The discussion can be expanded. First, the mean-variance exponent was used to explain community stability, but this exponent itself is a statistical property resulting from lower-level processes. It would be useful to explain why this exponent differs among sites. Second, it was less explained why species variability (at least for the dominant species) decreased with the mean precipitation. Is it because precipitation increased species and community biomass and thereby decreased species variability? This implies the role of

overyielding effect, which was mentioned several times in the Introduction and Methods but ignored later.

Response: We examined effects of climatic factors and species diversity indexes in affecting the mean–variance scaling exponent, and found no effects. We added these results in the revised Results section (Lines 256–259). Because the mean–variance scaling exponent was independent of these climatic factors and species diversity, we did not further discuss it.

In addition, we also further explored the reason for the negative relationship between the weighted average dominant species temporal CV and precipitation. We found a stronger stimulation of precipitation on biomass production of dominant species (steeper slope) than that on the standard deviation, suggesting a low standard deviation-to-mean ratio at high precipitation as anticipated by the reviewer's comment — thank you very much. See Lines 360–371 and Figures S9–S10.

Minor comments:
L152-155: I suggest to weaken this argument. It is far from trivial to extend results from 3-year data to longer ones. Maybe change it to: "Although two sites had available data for only 3 years, a recent study showed that investigating this relationship using a 3-year dataset can produce similar patterns with that of using longer time series". Moreover, I am not sure that Hector et al. (2010) is the right reference. Did they compare their 3-year results to longer ones? Otherwise, the recent paper by Craven et al. (2018 Nature Ecology & Evolution) could fit here.

Response: This part has been revised accordingly, including the change of reference (Lines 150–154).

L321-326: Not surprising given their definitions.

Response: We removed these results in the revised version.

L411: This argument should be justified, if the authors prefer to keep it. An appropriate null model is needed to test whether population dynamics are more compensatory or synchronous.
L425-427: Again, the first half of this sentence needs a statistical test. See above.

Response: In this revised version, we focused on discussing the essential effect of dominant species on the species synchrony in ecosystems with highly uneven distributions of species abundances. This pattern has been proposed by theoretical studies and supported by some smaller-scale observational studies, but the strength so far had not been quantified. Our study adds new region-scale evidence for this view, but also provides a method to examine and quantify it (partitioning it into different species abundance groups). This partitioning method should be helpful to better understand species synchrony in natural ecosystems. Please find these text changes in Lines 389–402.

L431: Another argument that was not supported by the data. I suggest removing it.

Response: This statement has been removed from the revised version. Please see Line 405.

##

Referee: 2

Comments to the Author(s)
In this manuscript, the authors try to revisit the diversity–community stability debate with field data in Inner Mongolian grassland China. The mainly conclusion is draw based on their five years consecutive data. Here they claim that community stability was independent of species richness but was regulated by species synchrony and population dynamics, especially of abundant species. However, the precipitation fluctuations synchronized population dynamics within communities, reducing their stability. This is an interesting topic in community ecology, however there are still many points unclear and contradiction. I would make commendation if the authors well deal with my comments below.

Comments to authors:
1. The main conclusion is that community stability was regulated by species synchrony and population dynamics. Hence, the data of time series on species' abundance should be a key information and evidence to support this argument. Why authors omitted this data in the figures. I would see that the authors present the analysis of the time series. What's about the phase difference to explain your argument.

Response: In this revised version, we provide a time-series figure of species biomass, which shows that the dominant species have different direction and extent regarding their response to environmental fluctuations. You can find these additions on Lines 391–392 and in the new Figure S3.

2. The whole story is biotic stability mechanisms here, but I still unknow what potential biotic mechanisms work here. Contrarily, authors have written "Precipitation fluctuations synchronized population dynamics within communities, reducing their stability." in abstract. So, I get the message that the community stability was controlled by precipitation fluctuations, isn't it? If so, it is still an abiotic mechanism regulating the community stability.

Response: Thank you very much for this point! Indeed, our results did not deny the effects of climatic factors, e.g. precipitation and its interannual variation, in affecting the community stability, but showed that these abiotic factors affected stability via the intermediate variable species synchrony. We now point out that the interannual variation in precipitation affects community stability mechanistically via species synchrony (Lines 241–243 and 276–279) and add further results at the end of the new Appendix 2. In

addition, we now discuss in more depth the essential role of dominant species in affecting the biotic stability mechanisms and community stability. These revisions can be found on Lines 360–371 and Line 389–402.

3. The authors have many contexts referred as mean-variance scaling knowledge, but I didn't read any data and figure to relate this Talyor law. How did your data links and advance our present understood of Talyor law?

Response: We added a new figure to show the mean–variance scaling relationship, which should be helpful to provide a more straightforward insight into this relationship. In addition, we also added text about the non-significant effects of climatic factors (precipitation and its interannual variation) and species diversity indices on the mean–variance scaling. Please find these revisions on Lines 181–183 and 256–259 and in the new Figure S5.

Appendix B

Responses to Comments

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Associate Editor
Board Member
Comments to Author:
Thank for submitting the revised manuscript, which was reviewed by the same two reviewers. Both reviewers thought that the authors have addressed their concerns in the revision. I agree with them. However, I think the authors should may much attention to their writing of the manuscript. I think there are many grammar mistakes and unclear sentences in the writing throughout the manuscript. One of the reviewers also point this out. I therefore suggest that the authors should check and improve their writing very carefully throughout the manuscript. It can't be acceptable if the unclear writing remains.
Response: Thank you very much for these points. We carefully checked and corrected the whole manuscript again to make sure there are no grammatical errors anymore and sentences are logically clear and correct.

Line 51, change variation to variations
Response: "variation" has been changed to "variations". Please see line 51 for the detail.

Line 71, remove "more"
Response: "more" has been removed. Please see line 71 for the detail.

Line 82, change scale to scales
Response: "scale" has been changed to "scales". Please see line 82 for the detail.

Line 97, conditions
Response: "condition" has been changed to "conditions". Please see line 97 for the detail.

Lines 104 – 106, consider to revise this sentence. Do you mean "the temperature grassland in Inner Mongolia is a typical part of the Eurasian grassland biome"?
Response: This sentence has been revised as "The Inner Mongolian grassland is a typical part of the Eurasian grassland biome…". Please see lines 104–106 for details.

Lines 106 – 109. Consider to rephrase this sentence and separate it into two. "In this region, precipitation is the dominant driver of biomass, species richness and species composition in communities. Observational studies have shown that precipitation in Inner Mongolian grassland has dramatically changed during the past decades, while the ecological consequences of precipitation changes are still not fully understood."
Response: This sentence has been revised accordingly. Please see lines 106–110 for details.

Line 117, consider to remove "biotic mechanisms assigned to the"

Response: It has been removed. Now, this sentence is revised as "community stability was more strongly affected by the most abundant, i.e. dominant species…". Please see lines 117–118 for details.

Lines 150 – 154. I think this should be separated into two sentences.
Response: This sentence has been revised as "A recent 39-site meta-analysis showed that investigating biodiversity–stability relationship with time series of ≤ 4 years can produce similar results to those using longer time series. In addition, multiple short time series to some extent may compensate few long time series. We therefore expected that our dataset could provide reliable insights into the biodiversity–stability relationship of the studied region."

Line 161, material should be materials
Response: We believe it should remain "material" as we checked in the internet. However, if the journal style requires "materials" for this phrase, we are of course happy to add the "s". At present we left it as "material" (line 161).

Lines 192 – 193, do you mean Shannon-Wiener diversity?
Line 193, remove index
Response: "Shannon diversity index" has been corrected to "Shannon-Wiener diversity". Please see lines 191–192 for details.

Line 199, change "trend of year" to "temporal trend"
Response: "trend of year" has been changed to "temporal trend". Please see lines 198.

Lines 201 – 202, consider to rephrase the sentence to: "The Taylor's power law [17] was employed to estimate the mean–variance scaling coefficient"
Response: This sentence has been revised to "Taylor's power law [21] was used to estimate …". Please see lines 200–201.

Line 259, change overyielding to overyielding effect
Response: "overyielding" has been changed to "overyielding effect". Please see line 258.

Lines 271 – 276, these two sentences read odd. Maybe consider to change them into the following: To test the hypotheses that the weighted average species temporal CV and species synchrony directly influenced the community temporal CV, we conducted path analysis to relate the community temporal CV to climatic factors, species diversity indices and biotic stability mechanisms using SEMs. Our model of path analysis had a total explanatory power of 0.87.
Response: This sentence has been rewritten accordingly, but with the last sentence reading "This path analysis did confirm the hypotheses and had a total explanatory power of 0.87 (Appendix 2, Figure 5a)." Please see lines 271–275.

Line 362, lack of
Response: "lack" has been changed to "lack of". Please see line 360.

Lin 369, root systems
Response: “root system” has been changed to “root systems”. Please see line 367.

Line 390, change are affecting to affect
Response: “are affecting” has been changed to “affect”. Please see line 388.

Line 397, dynamics
Response: “dynamic” has been changed to “dynamics”. Please see line 395.

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Reviewer(s)' Comments to Author:

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Referee: 2

Comments to the Author(s).
I have no further comments now.

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Referee: 1

Comments to the Author(s).
This revised manuscript presented an interesting study investigating the biotic and abiotic drivers of grassland ecosystem stability along a broad environmental gradient. The authors have addressed all my comments and have carefully revised the manuscript, and I have only two minor comments.

The recent theoretical work by Arnoldi et al. (2019) explored how species abundance may predict their contribution to community stability, which fits well into the current context. So I suggest the authors to incorporate it.
Response: Thank you very much for this recommendation. We are now citing this paper. Please see reference [44].

In lines 72-78, I would suggest an inverse order of these three mechanisms to match their popularity in the literature on stability.
Response: This order has been changed to species synchrony, mean–variance scaling relationship and overyielding effect. Please see lines 72–78.

Reference:
Arnoldi, J.F., Loreau, M. and Haegeman, B., 2019. The inherent multidimensionality of temporal variability: How common and rare species shape stability patterns. Ecology letters, 22(10), pp.1557-1567.