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Fluctuating selection and global change: a synthesis and review on disentangling the roles of climate amplitude, predictability and novelty

M. C. Bitter, J. M. Wong, H. G. Dam, S. C. Donelan, C. D. Kenkel, L. M. Komoroske, K. J. Nickols, E. B. Rivest, S. Salinas, S. C. Burgess and K. E. Lotterhos

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Review timeline

Original submission:	26 March 2021
Revised submission:	2 July 2021
Final acceptance:	23 July 2021

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

Review History

RSPB-2021-0727.R0 (Original submission)

Review form: Reviewer 1

Recommendation
Reject – article is not of sufficient interest (we will consider a transfer to another journal)

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

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

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

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?
No

Is it adequate?
Yes

Do you have any ethical concerns with this paper?
No

Comments to the Author
Bitter and colleagues argue that a current challenge in global change science is to connect research on climate novelty with that on phenotypic plasticity in changing environments, with a particular focus on marine systems. The strongest contribution of the manuscript is the push for others to study phenotypic plasticity in variable environments. That said, I found it difficult to interpret the verbal model, to find clear takeaways from the review portion of the paper, or to reconcile the future directions outlined with improving predictability. I detail these concerns and a few others below.

1. I struggled with the verbal model presented in Box 2 – in particular – how was predictability modeled? What are the assumptions and limitations of this approach? Some framing would help ground this example and make it more useful. Connecting the model more tightly to the discussion of time scales would help. Finally, how can it be applied in a climate change context? Is it that we can assign different species or systems to different types (say A – D), and use what we learn to make predictions in the climate change context, or will climate change flip a system from one type to another?

Related to these points, I found the figure difficult to understand, and more text in Box 2 would help. (including, what does the x-axis mean in panels F – H?)

2. While I agree the call to better understand phenotypic plasticity in the context of variable environments is important, the argument that doing so will lead to increased predictability for species responses to climate change seems like a stretch. To me, there is a leap from testing the theoretical prediction that predictability of fluctuations will favor plasticity to making predictions about how a species will respond to fluctuating environments in novel climates. Most importantly, don't many of these predictions then depend on knowing something about future (and/or novel) climates?

Is there any role to incorporate mean changes in the framework presented here – when can we ignore the mean changes? When will fluctuations be more important than changes in mean climate? This is perhaps beyond the scope of the paper as defined, but even without it, a more nuanced treatment of predictability, and a stronger link between the approach advocated for and climate change would make the paper a more useful contribution.

3. Figure 2 is pretty, but I did not find it very helpful. What else can we learn from the collection of all of these studies? I was surprised that all 60 studies described as 'a dearth' – although none met all 4 criteria, many meant several. Can more be reported on what the studies found? Maybe I missed something, but I wasn't sure what we were supposed to take away from this figure.
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5. Instead of arguing marine systems are ideal for these questions, could the argument be made instead that we really need to understand this topic in marine systems? And then add a short discussion of how results would apply/not apply to terrestrial systems. Or could just leave as an important point for marine systems, but the argument about marine systems being ideal isn't very helpful unless there is a translation to what we can learn in other systems.

6. I was confused about the point that environments will vary orthogonally between amplitude & predictability. How often will the same species encounter such environments? An example here would help.

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?
Good

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

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.
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Comments to the Author
In this manuscript, the authors propose an approach for investigating population responses to fluctuating environment, including extinction risk due to anthropogenic global change. They base their argument on the theoretical literature in evolutionary ecology, which indicates that the predictability of environmental fluctuations (quantified for instance by their autocorrelation on biologically relevant time scales) can be at least as important as their magnitude, with respect to its influence on the evolution and demography of populations. The reason is that the tracking of a fluctuating optimum phenotype across environments through phenotypic plasticity is strongly dependent on this environmental predictability. In addition, current levels of plasticity evolved in response to past levels of environmental predictability. The authors therefore argue that experiments that investigate the magnitude and predictability of environmental fluctuations in factorial designs (or "orthogonally") should prove more useful than those that focus only on the degree of novelty in the environment (as currently done my many studies). After briefly reviewing the theoretical literature, the authors propose general experimental designs along these lines, and then review existing empirical work, focusing on marine systems. They find that, although a few studies satisfy some of their criteria, none includes them all (knowledge of historical fluctuation patterns, factorial manipulation of magnitude and predictability, inclusion of novel environments, measurements of reaction norms and fitness). They also argue that these types of experiments should be performed more often with non-microbial organisms, and highlight that marine systems are particularly well suited to investigate these questions.

I found this manuscript interesting, well-written, and overall convincing. The review of theory is synthetic and clear, and mostly accurate (but see comments below). The approach advocated by the authors seems useful and productive, and I agree that the importance of environmental predictability is certainly misunderstood and too much overlooked.

One criticism I would have is that the authors seem to dismiss (or at least ignore) the difficulties with the type of experiments they propose. The most trivial reason why such experiments haven't been performed more than they have is because they are very challenging to run. So I think it would be useful to include some reflection about whether, and to what extent, the gaps they identify in current empirical literature is explained by the complexity of studying environmental predictability x magnitude experimentally, and how this could be improved. In fact, the type of experiment sketched in figure may be used to illustrate how the required design may be simpler that one could think. But it would also be worth considering more seriously what are the limits and constraints associated with manipulating environments that fluctuate at timescales close to (or below) generation times over somewhat long time periods, and what can be done about it, depending on the type of organism and environment. I think giving more thoughts to these issues would strengthen the manuscript.

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Another general comment is that I find it a bit too restrictive to limit the empirical discussion to marine systems so much. I understand that the authors want to make a point about the importance of the approach they advocate for these systems, and it's fine that they perform their quantitative analysis of the literature on them. Still, results from organisms with other ecologies could also be mentioned briefly, just like the theory they cite is not specific to marine organisms. I'm thinking of studies such as:
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Pike N, Tully T, Haccou P, Ferrière R. The effect of autocorrelation in environmental variability on the persistence of populations: An experimental test. Proc R Soc B Biol Sci. 2004;271: 2143–2148.
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Surely some of this literature is worth mentioning here, if only to say that there are few studies that manipulated environmental predictability *overall*, not just in marine systems.

Minor points:
131-132: "the reaction norm approach described here is mathematically similar to the character state approach". It is not mathematically similar, since it is a different mathematical formulation. I would rather write that the two approaches are mathematically equivalent, one can be translated into the other.
Box1: "Correlations across the period of time between development of the phenotype and selection can then be used as a proxy for environmental predictability for a given study system (Leung et al. 2020)." In this experiment, the environment actually remained constant for a few generations in between each change. Note also that Rescan et al (2020 Nature Ecology and Evolution) investigated population dynamics and extinction risk in this same experiment, and found an important role of plasticity.
Box 1: "even when the environment fluctuates randomly across generations". You mean, "fluctuates unpredictably", or "undergoes white noise", or something like this? Random environments can still be autocorrelated: this includes whole categories of stochastic processes (autoregressive, etc).
173-174: "the degree of phenotypic plasticity (sometimes termed "niche breadth", e.g. Lynch and Gabriel 1987; Kassen 2002)" Niche breadth, or environmental tolerance breadth, are not identical to plasticity. Rather, they result from the interplay of plasticity with changes in the optimum (as discussed in Chevin et al 2010 PLoS Biol, and investigated theoretically by Lande 2014)
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241: times-scales -> timescales

Decision letter (RSPB-2021-0727.R0)

@@date to be populated upon sending@@

Dear Mr Bitter:

Your manuscript has now been peer reviewed and the reviews have been assessed by an Associate Editor. The reviewers' comments (not including confidential comments to the Editor) and the comments from the Associate Editor are included at the end of this email for your reference. As you will see, the reviewers and the Editors have raised some concerns with your manuscript and we would like to invite you to revise your manuscript to address them.

We do not allow multiple rounds of revision so we urge you to make every effort to fully address all of the comments at this stage. If deemed necessary by the Associate Editor, your manuscript will be sent back to one or more of the original reviewers for assessment. If the original reviewers are not available we may invite new reviewers. Please note that we cannot guarantee eventual acceptance of your manuscript at this stage.

To submit your revision please log into http://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.

When submitting your revision please upload a file under "Response to Referees" - in the "File Upload" section. This should document, point by point, how you have responded to the reviewers' and Editors' comments, and the adjustments you have made to the 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.

Your main manuscript should be submitted as a text file (doc, txt, rtf or tex), not a PDF. Your figures should be submitted as separate files and not included within the main manuscript file.

When revising your manuscript you should also ensure that it adheres to our editorial policies (https://royalsociety.org/journals/ethics-policies/). You should pay particular attention to the following:

Research ethics:
If your study contains research on humans please ensure that you detail in the methods section whether you obtained ethical approval from your local research ethics committee and gained informed consent to participate from each of the participants.

Use of animals and field studies:

If your study uses animals please include details in the methods section of any approval and licences given to carry out the study and include full details of how animal welfare standards were ensured. Field studies should be conducted in accordance with local legislation; please include details of the appropriate permission and licences that you obtained to carry out the field work.

Data accessibility and data citation:
It is a condition of publication that you make available the data and research materials supporting the results in the article. Please see our Data Sharing Policies (https://royalsociety.org/journals/authors/author-guidelines/#data). Datasets should be deposited in an appropriate publicly available repository and details of the associated accession number, link or DOI to the datasets must be included in the Data Accessibility section of the article (https://royalsociety.org/journals/ethics-policies/data-sharing-mining/). Reference(s) to datasets should also be included in the reference list of the article with DOIs (where available).

In order to ensure effective and robust dissemination and appropriate credit to authors the dataset(s) used should also be fully cited and listed in the references.

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.

For more information please see our open data policy http://royalsocietypublishing.org/data-sharing.

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. Please try to submit all supplementary material as a single file.

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].

Please submit a copy of your revised paper within three weeks. If we do not hear from you within this time your manuscript will be rejected. If you are unable to meet this deadline please let us know as soon as possible, as we may be able to grant a short extension.

Thank you for submitting your manuscript to Proceedings B; we look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Best wishes,
Dr Sasha Dall
mailto: proceedingsb@royalsociety.org

Associate Editor
Comments to Author:
Dear Dr. Bitter,

I have now received two reviews of your manuscript "Fluctuating selection and global change: a synthesis and review on disentangling the roles of climate amplitude, predictability, and novelty". As you will see, the reviewers were quite divergent in their opinion of the manuscript with one recommending Rejection and the other Major Revision.

Based on my own reading of the manuscript, I found myself more aligned with Reviewer #2 and therefore recommend Major Revision. I think a key issue, raised by R #2, is that undertaking the proposed experimental approach is not a trivial matter. I was left wondering whether readers will feel, after reading this manuscript, that they now have a clear, feasible path forward. I suggest adding some narrative that helps readers better appreciate the opportunities that lay ahead. I also agree (particularly with R #1) that the case for marine systems being "ideal" for exploring these questions was not particularly strong. I think one could easily argue that these issues are just as important for terrestrial systems. Ultimately, I do not think this is a fatal issue but I was left wondering how you could be less parochial and instead emphasize how work in marine systems could open new doors of opportunity in other systems (a good, albeit much less complex, example being the strong influence of early experimental work in marine systems that helped inspire manipulative experimental approaches in other systems).

I should note that the summary of the literature review was somewhat unsatisfying in its recitation of how past studies missed the mark. This is somewhat mitigated by the very brief discussion of more recent studies that seem to get closer to the "idealized scenario" and I felt that more attention to these recent studies would strengthen the manuscript.

Regarding Figure 2, I think the "A's" and "P's" with bars are more of a distraction than a help. The color coding makes it clear how each amplitude/predictability scenario unfolds, particularly in F, G, and H.

In Table 1, replace "Criteria" with "Criterion".

In closing, I appreciate the work you invested in developing this manuscript for our Special Feature and I look forward to seeing your revised manuscript!

Reviewer #1

While this reviewer appreciated the importance of studying phenotypic plasticity in variable environments, they had difficulty following the verbal model (I agree) and identifying clear conclusions from the review and robust future research directions that could deliver improved predictability. The reviewer also offers some general comments that may help improve the manuscript. Although the reviewer's comments are concise, some (e.g., point #2) resonated with me.

Reviewer #2

This reviewer was more enthusiastic about the paper, concluding that it was interesting, well-written, and convincing overall. An important point raised by the reviewer, and I agree, is that a main reason why progress has not been as rapid as one might hope is because conducting the experiments necessary to address the issue of predictability and magnitude are challenging to say the least. Hence, some reflection on this, as suggested by the reviewer, would help to strengthen the MS.

The reviewer also felt that the manuscript may be unnecessarily restrictive with its focused empirical discussion on marine systems. I understand the emphasis on "marine" given the goals of the Special Feature but the manuscript would nevertheless benefit from some effort to connect to work in other systems. I do think the paper would resonate more broadly by highlight how the leveraging of insights from marine systems can be important in establishing generality.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)
Bitter and colleagues argue that a current challenge in global change science is to connect research on climate novelty with that on phenotypic plasticity in changing environments, with a particular focus on marine systems. The strongest contribution of the manuscript is the push for others to study phenotypic plasticity in variable environments. That said, I found it difficult to interpret the verbal model, to find clear takeaways from the review portion of the paper, or to reconcile the future directions outlined with improving predictability. I detail these concerns and a few others below.

1. I struggled with the verbal model presented in Box 2 – in particular – how was predictability modeled? What are the assumptions and limitations of this approach? Some framing would help ground this example and make it more useful. Connecting the model more tightly to the discussion of time scales would help. Finally, how can it be applied in a climate change context? Is it that we can assign different species or systems to different types (say A – D), and use what we learn to make predictions in the climate change context, or will climate change flip a system from one type to another?

Related to these points, I found the figure difficult to understand, and more text in Box 2 would help. (including, what does the x-axis mean in panels F – H?)

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Author's Response to Decision Letter for (RSPB-2021-0727.R0)

See Appendix A.

Decision letter (RSPB-2021-0727.R1)

23-Jul-2021

Dear Mr Bitter

I am pleased to inform you that your manuscript entitled "Fluctuating selection and global change: a synthesis and review on disentangling the roles of climate amplitude, predictability, and novelty" 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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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,
Dr Sasha Dall
Editor, Proceedings B
mailto: proceedingsb@royalsociety.org

Associate Editor:
Comments to Author:
Dear Dr. Bitter and Colleagues,

I think you have done a very nice job in responding to the feedback on your original submission. It is clear to me that you put a lot of thought and effort into clarifying and addressing the issues that were raised during peer review. I therefore recommend acceptance of this paper. My only request is that you go over the manuscript carefully with one last proofread to fix any minor errors that emerged during the revision process (e.g., there appear to be some missing words in some sentences).

Congratulations on your strong effort to address this complex and important topic!
Best Regards,
Geoff

Appendix A

We are grateful for the opportunity to revise our manuscript. The editorial and referee comments were extremely valuable in the major revision and reorganization of the manuscript. Largely, we aimed for our revisions to make our proposed experimental design more approachable to readers, better highlight the key takeaways from our literature search, and highlight that the issues discussed transcend both marine and terrestrial habitats. Thank you for the opportunity to have our work reviewed by *Proc. B.*

On behalf of all authors,

Mark C. Bitter

Associate Editor
Board Member: 1
Comments to Author:
Dear Dr. Bitter,

I have now received two reviews of your manuscript “Fluctuating selection and global change: a synthesis and review on disentangling the roles of climate amplitude, predictability, and novelty”. As you will see, the reviewers were quite divergent in their opinion of the manuscript with one recommending Rejection and the other Major Revision.

Based on my own reading of the manuscript, I found myself more aligned with Reviewer #2 and therefore recommend Major Revision. I think a key issue, raised by R #2, is that undertaking the proposed experimental approach is not a trivial matter. I was left wondering whether readers will feel, after reading this manuscript, that they now have a clear, feasible path forward. I suggest adding some narrative that helps readers better appreciate the opportunities that lay ahead.

We have conducted major revisions and a reorganization of the manuscript to make the experimental recommendations more straightforward and the key takeaways clearer. We agree that undertaking the proposed experimental design may be prohibitively challenging. In the revised manuscript, we have thus included a reflection on the feasibility and challenges in implementing such a design. We highlight that our proposed experimental design is of similar methodological complexity to many multi-stressor experiments that have already been completed (e.g. lines 275-278). Furthermore, we provide a more expanded discussion of studies that have effectively tested theory in terrestrial systems (lines 171-188), demonstrating to readers that because many of the proposed experimental approaches are underway in other systems, they are likely feasible in marine systems.

I also agree (particularly with R #1) that the case for marine systems being “ideal” for exploring these questions was not particularly strong. I think one could easily argue that these issues are just as important for terrestrial systems. Ultimately, I do not think this is a fatal issue but I was left wondering how you could be less parochial and instead emphasize how work in marine systems could open new doors of opportunity in other systems (a good, albeit much less complex, example being the strong influence of early experimental work in marine systems that helped inspire manipulative experimental approaches in other systems).

In the revised manuscript we have discussed that the issues are indeed pertinent to both marine and terrestrial habitats. We have made more explicit that our focus on marine habitats is largely due to the systems' underrepresentation in tests of evolutionary theory. We suggest that to infer generality from theoretical predictions, predictions must be tested in marine systems at a comparable rate to terrestrial systems (e.g. lines 184-188). Additionally, we now provide a brief description of studies in terrestrial systems that have effectively tested theoretical predictions on lines 171-188.

I should note that the summary of the literature review was somewhat unsatisfying in its recitation of how past studies missed the mark. This is somewhat mitigated by the very brief discussion of more recent studies that seem to get closer to the "idealized scenario" and I felt that more attention to these recent studies would strengthen the manuscript.

In the revised manuscript, we have expanded the section discussing studies that have effectively tested evolutionary theory (lines 171-188; lines 278-286). Notably, we now include several examples of experimental tests in terrestrial systems as a means to both highlight both the importance, and feasibility, of such empirical work.

Regarding Figure 2, I think the "A's" and "P's" with bars are more of a distraction than a help. The color coding makes it clear how each amplitude/predictability scenario unfolds, particularly in F, G, and H.

We agree and have removed "A's" and "P's" in F-H of the figure. We now label each reaction norm/fitness function to its corresponding environmental time series in A-D.

In Table 1, replace "Criteria" with "Criterion".

This has been resolved in the text of the revised manuscript.

In closing, I appreciate the work you invested in developing this manuscript for our Special Feature and I look forward to seeing your revised manuscript!

Thank you for your insightful comments and thorough assessment of the referee reviews!

Reviewer #1

While this reviewer appreciated the importance of studying phenotypic plasticity in variable environments, they had difficulty following the verbal model (I agree) and identifying clear conclusions from the review and robust future research directions that could deliver improved predictability. The reviewer also offers some general comments that may help improve the manuscript. Although the reviewer's comments are concise, some (e.g., point #2) resonated with me.

Reviewer #2

This reviewer was more enthusiastic about the paper, concluding that it was interesting, well-written, and convincing overall. An important point raised by the reviewer, and I agree, is that a main reason why progress has not been as rapid as one might hope is because conducting the experiments necessary to address the issue of predictability and magnitude are challenging to say the least. Hence, some reflection on this, as suggested by the reviewer, would help to strengthen the MS.

The reviewer also felt that the manuscript may be unnecessarily restrictive with its focused empirical discussion on marine systems. I understand the emphasis on “marine” given the goals of the Special Feature but the manuscript would nevertheless benefit from some effort to connect to work in other systems. I do think the paper would resonate more broadly by highlight how the leveraging of insights from marine systems can be important in establishing generality.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)
Bitter and colleagues argue that a current challenge in global change science is to connect research on climate novelty with that on phenotypic plasticity in changing environments, with a particular focus on marine systems. The strongest contribution of the manuscript is the push for others to study phenotypic plasticity in variable environments. That said, I found it difficult to interpret the verbal model, to find clear takeaways from the review portion of the paper, or to reconcile the future directions outlined with improving predictability. I detail these concerns and a few others below.

1. I struggled with the verbal model presented in Box 2 – in particular – how was predictability modeled? What are the assumptions and limitations of this approach? Some framing would help ground this example and make it more useful. Connecting the model more tightly to the discussion of time scales would help.

It is not completely clear which “verbal model” the referee is referring to, but we assume it to be the model underlying the time-series presented in Box 2 figure 1. We added a clarifying statement that these data were generated via simulation following the model presented in Ruokolainen et al 2009. In Box 2 we state: “The environment is perfectly correlated with the cue, which affects the development of phenotypes. Therefore, the autocorrelation of the environmental time series provides a measure of predictability.” This is in accordance with how predictability is oftentimes modelled in theoretical studies (see Box 1; (Gavrilets and Scheiner 1993; Tufto 2000; Chevin et al. 2013)). We also point the referee to the R code we have deposited alongside this manuscript, which was used to generate the simulated data presented in Box 2 figure 1.

In the revised manuscript, we reframed the Box 2 environment to be phytoplankton evolving in response to temperature fluctuations as a means to ground the example. However, please note that the model would be relevant for be any environmental variable that causes selection. We also directly note in Box 2 that: “the specific timescale over which the environment fluctuates is only relevant within the context of the biology of the system, specifically the period between when the phenotype under selection develops and selection on that phenotype occurs.” Finally, Box 1

contains a more thorough discussion of biologically-relevant timescales in light of environmental predictability.

Finally, how can it be applied in a climate change context? Is it that we can assign different species or systems to different types (say A – D), and use what we learn to make predictions in the climate change context, or will climate change flip a system from one type to another?

The fundamental goal of the manuscript is to highlight the need to effectively test the theory of how different regimes of fluctuating selection at different populations *within a species* shape patterns of phenotypic plasticity in those populations, and their response to climate change. Therefore, extension of the suggested experimental design in Box 2 to species or ecosystems is beyond the scope of the manuscript. Our suggestion is that in the comparison of different populations within a species that have different patterns of historical environmental fluctuations (A-D), the response of each population to climate change can be inferred from the experimental assays in (F-H). Once there are more experimental tests of theory, we agree that extending these concepts to a species/ecosystem level and incorporating the ways in which climate change can alter patterns of environmental variability are important directions for future research. To clarify these points, we have revised text in Box 2 (e.g. "In the comparison of different populations within a species that have different patterns of historical environmental fluctuations (A-D), the response of each population to climate change can be inferred from the experimental assays in (F-H)).

Related to these points, I found the figure difficult to understand, and more text in Box 2 would help. (including, what does the x-axis mean in panels F – H?)

The x-axis in F and G ("environment") is the same as the y-axis in A-D (previously labeled at *E(t)*). To clarify the axes and ground the example, we present them as "temperature" in the revised manuscript.

2. While I agree the call to better understand phenotypic plasticity in the context of variable environments is important, the argument that doing so will lead to increased predictability for species responses to climate change seems like a stretch. To me, there is a leap from testing the theoretical prediction that predictability of fluctuations will favor plasticity to making predictions about how a species will respond to fluctuating environments in novel climates.

In the revised manuscript, on lines 369-378, we highlight that various theoretical models (e.g. Ashander et al. 2016, Scheiner et al. 2020) have shown that plasticity will augment population persistence under novel climates, as long as certain conditions/assumptions hold. Indeed, these are simply theoretical predictions that need to be robustly tested in practice. To that end, we provide criteria that studies must meet in order to test both predictions of whether plasticity is favored in predictably fluctuating environments, and whether it facilitates persistence in novel conditions (Box 2). We note that one limitation to the proposed study design is that we suggest focusing on "one or a few environmental variables", despite the highly multivariate nature of environmental change (line 274-275). We suggest this simplification to aid in logistical simplicity, as there is still pressing need for empirical validation of the basic principles explored in evolutionary theory.

In sum, theory predicts that a better understanding of the evolution of phenotypic plasticity can be used to infer how populations within a species will respond to climate change, but as we review in the section "Persistence in novel climates," outcomes can be context-dependent. Our goal with the manuscript is not to make the "leap" that the reviewer is referring to, but to point out that highly cited methods that are used to identify species vulnerabilities to climate change are based on the historical amplitude of fluctuations and ignore other aspects of fluctuating environments that select for phenotypic plasticity.

Most importantly, don't many of these predictions then depend on knowing something about future (and/or novel) climates?

Absolutely. We introduce and describe the literature forecasting the emergence of novel climates on lines 44-66. We also note a forthcoming manuscript that focuses on the emergence of novel climates in marine systems specifically (Lotterhos KE, Láruson AJ*, Jiang LQ. Novel and disappearing climates in the global sea surface from 1800 to 2100. In revision at *Nature Scientific Reports.*)

Is there any role to incorporate mean changes in the framework presented here – when can we ignore the mean changes? When will fluctuations be more important than changes in mean climate? This is perhaps beyond the scope of the paper as defined, but even without it, a more nuanced treatment of predictability, and a stronger link between the approach advocated for and climate change would make the paper a more useful contribution.

The experimental design presented in Box 2 can be used to determine the interplay between historical fluctuations that a population has experienced and different degrees of change in the mean climate. Specifically, expected changes in mean conditions are incorporated in the proposed experimental design provided in Box 2, wherein we state: "experiments seeking to disentangle the effects of amplitude and predictability, while controlling for the degree of novelty, should include five experimental levels as indicated in Fig. 1E". Furthermore, a nuanced definition of predictability is provided in Box 1, and the Supplementary Text provides even more detailed information and a quantitative description of this concept.

3. Figure 2 is pretty, but I did not find it very helpful. What else can we learn from the collection of all of these studies? I was surprised that all 60 studies described as 'a dearth' – although none met all 4 criteria, many meant several. Can more be reported on what the studies found? Maybe I missed something, but I wasn't sure what we were supposed to take away from this figure.

The main takeaway from the collection of these studies is that most experimental designs do not disentangle the role of historical predictability and amplitude in a fluctuating environment for assessing the response to climate change (not that there is a dearth of studies simply looking at responses to variable environments). In the revised manuscript, we have removed the term "dearth", and simply highlight the relatively few studies that have conducted targeted and effective tests of theoretical predictions (lines 260-286).

4. A discussion of time scales is missing from the beginning of the paper – doesn't appear until Fig 1 on page 14. In terms of understanding fluctuating environments, it seems key to bring time scales in much earlier. Fig. 1 seems more useful for describing current environmental

fluctuations – rather than how climate change might influence them. A connection here would help.

We maintain that the goal is not to ask how climate change will influence environmental fluctuations, but how a population's evolutionary history has shaped contemporary patterns of phenotypic variation in natural populations. Theory additionally predicts that it will be this variation that will influence how populations respond to the progression of global change. This is stated in the manuscript on lines 67-70: "However, climate novelty-based estimates may be inaccurate because they do not take into account other aspects of fluctuating environments, particularly the predictability of fluctuations, which evolutionary theory predicts will largely shape the phenotypic variation present within natural populations. This variation will ultimately dictate if, and how, populations persist as novel conditions emerge (Botero et al. 2015; Nadeau, Urban, and Bridle 2017; Scheiner, Barfield, and Holt 2020)."

In terms of the "timescales" that are pertinent to this conversation, these are species-specific periods over which shifts in the environment will select for, or against, phenotypic plasticity. A thorough discussion of these appropriate timescales is provided in Box 1 (now titled "Defining vs. measuring environmental predictability and the relevant timescale").

5. Instead of arguing marine systems are ideal for these questions, could the argument be made instead that we really need to understand this topic in marine systems? And then add a short discussion of how results would apply/not apply to terrestrial systems. Or could just leave as an important point for marine systems, but the argument about marine systems being ideal isn't very helpful unless there is a translation to what we can learn in other systems.

We agreed that these suggestions were good revisions to bring to the manuscript. In the revised manuscript, we have highlighted that these issues are indeed pertinent to both marine and terrestrial habitats. We further clarified that our focus on marine systems is based on their underrepresentation in tests of evolutionary theory. We suggest that to infer generality from theoretical predictions, marine systems must be used at a comparable rate to terrestrial systems. See lines 171-188.

6. I was confused about the point that environments will vary orthogonally between amplitude & predictability. How often will the same species encounter such environments? An example here would help.

Indeed, we do not know how often populations of marine species are exposed to orthogonal differences in the amplitude and predictability of selective pressures, because both of these aspects of fluctuating environments are rarely measured in marine systems. This motivates our call for increased high-frequency monitoring of oceanographic conditions in coastal habitats: "Such an approach relies on measures of the 'historical environment' in which focal populations likely evolved, and thus the existence of continuous environmental time-series data (which can be sparse in marine systems)" (lines 323-325). The revised manuscript discusses an empirical study that has effectively taken this approach (lines 320-322).

Ultimately, to disentangle the effect of fluctuation amplitude and predictability on patterns of phenotypic variation, these two variables must be examined in an orthogonal manner. Therefore we suggest either lab-based manipulation of these conditions or identification of populations locally adapted to such conditions in nature (Box 2).

Referee: 2

Comments to the Author(s)
In this manuscript, the authors propose an approach for investigating population responses to fluctuating environment, including extinction risk due to anthropogenic global change. They base their argument on the theoretical literature in evolutionary ecology, which indicates that the predictability of environmental fluctuations (quantified for instance by their autocorrelation on biologically relevant time scales) can be at least as important as their magnitude, with respect to its influence on the evolution and demography of populations. The reason is that the tracking of a fluctuating optimum phenotype across environments through phenotypic plasticity is strongly dependent on this environmental predictability. In addition, current levels of plasticity evolved in response to past levels of environmental predictability. The authors therefore argue that experiments that investigate the magnitude and predictability of environmental fluctuations in factorial designs (or "orthogonally") should prove more useful than those that focus only on the degree of novelty in the environment (as currently done my many studies). After briefly reviewing the theoretical literature, the authors propose general experimental designs along these lines, and then review existing empirical work, focusing on marine systems. They find that, although a few studies satisfy some of their criteria, none includes them all (knowledge of historical fluctuation patterns, factorial manipulation of magnitude and predictability, inclusion of novel environments, measurements of reaction norms and fitness). They also argue that these types of experiments should be performed more often with non-microbial organisms, and highlight that marine systems are particularly well suited to investigate these questions.

I found this manuscript interesting, well-written, and overall convincing. The review of theory is synthetic and clear, and mostly accurate (but see comments below). The approach advocated by the authors seems useful and productive, and I agree that the importance of environmental predictability is certainly misunderstood and too much overlooked.

Thank you very much for these insightful comments!

One criticism I would have is that the authors seem to dismiss (or at least ignore) the difficulties with the type of experiments they propose. The most trivial reason why such experiments haven't been performed more than they have is because they are very challenging to run. So I think it would be useful to include some reflection about whether, and to what extent, the gaps they identify in current empirical literature is explained by the complexity of studying environmental predictability x magnitude experimentally, and how this could be improved. In fact, the type of experiment sketched in figure may be used to illustrate how the required design may be simpler that one could think. But it would also be worth considering more seriously what are the limits and constraints associated with manipulating environments that fluctuate at timescales close to (or below) generation times over somewhat long time periods, and what can be done about it, depending on the type of organism and environment. I think giving more thoughts to these issues would strengthen the manuscript.

We agree that considerations regarding the nontrivial nature of the proposed experimental design are important for the manuscript. These were largely disregarded from the original submission of the manuscript due to space limitations. However, in the revised manuscript, we devote an entire paragraph to this reflection, highlighting the points raised by the referee (lines 259-286).

Regarding the general argument, although the description of theory is overall correct, I think it could be made more precise. This is particularly true for statements such as: “the extent to which fluctuations in the selective environment are predictable is the dominant factor influencing the adaptive advantage of phenotypic plasticity” (77); “the evolved reaction norm slope (e.g., the amount of plasticity) is to a great extent proportional to the strength of the correlation between the environmental variable acting as the cue and the optimal phenotype during selection […], but is to a much lesser extent proportional to the amplitude of fluctuations” (166-167); “the effects of amplitude are secondary to those of predictability in influencing the evolution of plastic traits” (179-180). To be more exact in such models, environmental predictability determines the *level of plasticity* that is expected to evolve at equilibrium, while the magnitude of fluctuations (together with the strength of stabilizing selection, inversely proportional to the width of the fitness peak) determines the (expected) *strength of selection on plasticity* (the reason why it is “expected” is because plasticity will itself fluctuate to some extent, although this is probably inconsequential for the present ms). In others words, conditional on the genetic (co)variance of reaction norms, the magnitude of environmental fluctuations determines how fast the population evolves to its expected equilibrium set by the environmental predictability (this can be seen very well in eq. 3c in Lande 2009, for instance). That’s why the statements quoted above are somewhat misleading: predictability and magnitude simply play a different role, the former determines the level of plasticity that’s favored, while the latter determines how much it’s favored, and also how badly phenotypic mismatches due to wrong plasticity are going to impact demography.

We have revised the manuscript in multiple locations to make the theoretical summary more precise. Specifically, we have stated that the amplitude of environmental fluctuations determines the strength of selection acting on the optimal degree of plasticity, and this optimal degree of plasticity is determined by the predictability of the fluctuations. For example, lines 162-164 now read: “The amplitude of environmental fluctuations then determines the strength of selection on plasticity, dictating the rate at which the population evolves to its expected equilibrium-level of plasticity (which is set by the environmental predictability, Lande 2009).”

Another general comment is that I find it a bit too restrictive to limit the empirical discussion to marine systems so much. I understand that the authors want to make a point about the importance of the approach they advocate for these systems, and it’s fine that they perform their quantitative analysis of the literature on them. Still, results from organisms with other ecologies could also be mentioned briefly, just like the theory they cite is not specific to marine organisms. I’m thinking of studies such as:
Gonzalez A, Holt RD. The inflationary effects of environmental fluctuations in source–sink systems. Proc Natl Acad Sci. 2002;99: 14872–14877.
Pike N, Tully T, Haccou P, Ferrière R. The effect of autocorrelation in environmental variability on the persistence of populations: An experimental test. Proc R Soc B Biol Sci. 2004;271: 2143–2148.
Dey S, Proulx SR, Teotónio H. Adaptation to Temporally Fluctuating Environments by the Evolution of Maternal Effects. PLoS Biol. 2016;14: 1–29.
Wieczynski DJ, Turner PE, Vasseur DA. Temporally Autocorrelated Environmental Fluctuations Inhibit the Evolution of Stress Tolerance. Am Nat. 2018;191: E000–E000.
Karve SM, Daniel S, Chavhan YD, Anand A, Kharola SS, Dey S. Escherichia coli populations in unpredictably fluctuating environments evolve to face novel stresses through enhanced efflux activity. J Evol Biol. 2015;28: 1131–1143.
Surely some of this literature is worth mentioning here, if only to say that there are few studies

that manipulated environmental predictability *overall*, not just in marine systems.

This is in alignment with the comment from the editor and Referee 1. Accordingly, the revised manuscript highlights that the principles discussed here hold relevance to both marine and terrestrial systems. We have highlighted the progress made in terrestrial systems (including the references provided here, as well as several others), to suggest that marine systems are underrepresented in tests of theoretical predictions, warranting the special focus we provide them in this manuscript (lines 184-195).

Minor points:
131-132: “the reaction norm approach described here is mathematically similar to the character state approach”. It is not mathematically similar, since it is a different mathematical formulation. I would rather write that the two approaches are mathematically equivalent, one can be translated into the other.

We agree and have changed the text accordingly.

Box1: “Correlations across the period of time between development of the phenotype and selection can then be used as a proxy for environmental predictability for a given study system (Leung et al. 2020).” In this experiment, the environment actually remained constant for a few generations in between each change. Note also that Rescan et al (2020 Nature Ecology and Evolution) investigated population dynamics and extinction risk in this same experiment, and found an important role of plasticity.

Indeed, Leung et al. 2020 was incorrectly cited in this instance. We have resolved this in the revised manuscript (though a description of that study remains on lines 280-285) and also added the Rescan et al. citation in an appropriate location on line 280.

Box 1: “even when the environment fluctuates randomly across generations”. You mean, “fluctuates unpredictably”, or “undergoes white noise”, or something like this? Random environments can still be autocorrelated: this includes whole categories of stochastic processes (autoregressive, etc).

This has been clarified in Box 1 which now reads: “plasticity in developmental traits can still evolve even when the environment undergoes white noise (unpredictably fluctuates) across generations”

173-174: “the degree of phenotypic plasticity (sometimes termed "niche breadth", e.g. Lynch and Gabriel 1987; Kassen 2002)” Niche breadth, or environmental tolerance breadth, are not identical to plasticity. Rather, they result from the interplay of plasticity with changes in the optimum (as discussed in Chevin et al 2010 PLoS Biol, and investigated theoretically by Lande 2014)

The term “niche breadth” has been removed from the revised manuscript.

Box 2: “or in a field-based approach where populations locally adapted to distinct regimes of fluctuating selection pressures are collected and compared in a lab-based experiment”. What about reciprocal field transplants across sites with different fluctuating regimes? Wouldn’t that be worth encouraging too?

The reason we do not mention a reciprocal transplant approach is that it would preclude effectively assessing responses to novel conditions for each population (such conditions must be manipulated in the lab, as they are not observed in contemporary environments). A main goal of the manuscript is to assess how evolved plasticity mitigates the population's response to environmental change, while controlling for the degree of novelty.

Box 2 Figure: shouldn't there be a symbol like sigma instead of a square here?

Yes, this must have been an issue during .pdf conversion. We have uploaded a separate .pdf containing figures as a supplementary file to circumvent this issue during final submission.

214-216: "Plasticity increases population persistence in novel climates when reaction norms are considered to be physiologically continuous, meaning that the same loci contribute to plasticity in both the novel and historical environment (de Jong 2005)." An additional condition discussed in Chevin & Hoffmann (2017, Phil Trans) is that the optimum phenotype changes smoothly across environments.

We have added this additional condition, and associated citation, to the revised manuscript (lines 370-374).

231-233: "Such cryptic variation can change the slope of the reaction norm across the environment". Not exactly: variance in reaction norm slope implies there has to be cryptic genetic variance (with linear reactions norms), as illustrated in Lande 2009. If this variation is heritable, then the mean plasticity can evolve.

This issue has been resolved in the revise manuscript on lines 413-415.

241: times-scales -> timescales

Resolved in text.