Comments from the Editors and Reviewers:

Subject Editor:  
Three reviewers found that this manuscript addresses an interesting topic, with the potential to contribute importantly to the literature on microclimatic effects on plant herbivore-interactions, and that the ideas are mostly clearly and logically presented. However, all three also had significant concerns with various aspects of the study methods, novelty, and/or interpretation of results, which would need to be convincingly addressed in a major revision of the manuscript.  
  
Reviewers 1 and 2 both highlight the single measure of soil moisture as a serious limitation of the data set, which requires additional context and justification. Reviewer 1 suggests that additional data or analyses may be required to adequately address this concern. Reviewer 3, notes that soil moisture is the one novel variable examined in this study (relative to other papers already published on this data set), such that convincing your reader of the rigor and relevance of the variable as measured is particularly important. Reviewers 2 and 3 also suggests a more explicit explanation and justification for the microclimatic variables and scale selected for study, in the context of what is known about this system, is required.. Both reviewers also note issues of potential autocorrelation in the data set, and recommend a more detailed description of how this was addressed statistically.  
  
Reviewers 1 and 2 found the complexity of the data set makes the manuscript hard to follow at times. They suggest the logical flow may benefit form a figure to explain the predictions of possible relationships among variables (R1), and/or using the predictions to help the reader follow the complex set-up and network of connections in the analysis (R2).  
  
Finally, reviewer three notes a high degree of overlap between this manuscript and previously published work by the Authors (Valdes & Ehrlen 2018 (Oikos) and Valdes & Ehrlen 2019 (J Animal Ecology). A revised manuscript should thus clearly outline what was found in previous work, and explicitly state how this study builds on and adds to these earlier results. As the reviewer notes, it should be clear what distinguishes this study from their earlier work. This may also add significant context for the present study (addressing some of the concerns outlined above).  
  
Reviewer #1: The manuscript "Microclimate influences plant reproductive performance via an antagonistic interaction" presents a study investigating how two microclimatic variables (temperature and soil moisture) can mediate the relationship between a plant, Gentiana pneumonanthe, and its seed predator, the myrmecophagous butterfly Phengaris alcon. Results indicate that the effects of microclimate on plant reproduction are indirect and mediated by focal plant phenology, ant abundance, and the density and phenology of neighboring plants. The topic is timely and interesting, the ideas in the manuscript are presented in a logical way and the overall statistical analyses are appropriate (but see comment to line 218). My main concern is that soil moisture was measured only once at each sampling location throughout the study, while temperature was measured multiple times over ~2 months. SEM require large sample sizes; thus, the small sample size to estimate moisture levels is a potentially serious issue. Soil moisture varied greatly from ~40 to 80% and I wonder if that reflects environmental variation in the study site or if it's an artifact due to the lack of replication. Authors do not justify the limited sampling in the methods nor address how this could be affecting their results in the discussion. I understand re-sampling is probably not an option, but I wonder if there may be some additional information or analyses that the authors can add to make up for this limitation.  
There are some findings that I consider should be discussed in more depth:  
1. There is a pattern in which plant performance decreased with increasing soil moisture. Are the optimum soil moisture levels for this plant known? Can you cite a study regarding the low nutrient availability possibility stated in line 305?  
2. Plants flowered earlier in colder and drier microsites. This is very surprising, since the typical trend for ectotherms is to accelerate phenology under warmer conditions. Consider adding a few sentences in the section of the discussion that addresses this issue.  
  
Finally, I have some minor questions, edits and suggestions to improve the readability and clarity of the manuscript:  
  
Line 13. Is the word "steps" referring to trophic levels? Interacting species? Both?  
  
Line 16. Consider changing "among-individual" to something more common like "intraspecific" or "within-population".  
  
Line 18. Insert "myrmecophagous and seed predator" before "butterfly", otherwise the mention of ants in line 23 will be a surprise for readers not already familiar with the study system.  
  
Lines 81 to 83. Objective three is not clear. Are the two steps plant phenology and plant context? How are these steps related? One after another or are they alternative paths? Consider adding a figure to explain the predictions of possible relationships.  
  
Lines96-97. Express difference of seed production in percentages.  
  
Line 98. It is not clear when do caterpillars drop to the ground. Is it during the fifth instar? Or can it be any instar after fourth?  
  
Lines 105-107. Consider splitting this sentence.  
Lines 115-118. If available, consider including pictures of the phenophases of G. pneumonanthe shoots.  
  
Line 130. Consider adding "To estimate ant abundance" at the beginning, so it is clear what aspect of the ant community was assessed.  
  
Line177. The first sentence, up to the second comma is not clear, consider rearranging the paragraph.  
  
Line 218 to 221: Is this appropriate? Please justify doing this.  
  
Line 242 to 244: this sentence is not clear.  
  
Line 305. Do you have any evidence of moister microsites being poorer in nutrients? Is there any study that can be cited here?  
  
Lines 325 to 327. Not clear, consider rephrasing.  
  
  
  
  
  
Reviewer #2: The authors examined the relationship between small-scale microclimate factors known to drive different interaction outcomes and consequent plant reproductive performance. Findings emphasize the importance of interactions that mediate indirect effects of microclimate variation on plant performance. I appreciate the consideration of how biotic interactions can inform our understanding of links between plant performance and microhabitats, and their use of an SEM analytical framework to incorporate the many interacting hypotheses. I also like that they tie small-scale environmental variation to emergent temporal differences (phenology) and herbivory consequences. Because of the complexity of the relationships considered, the manuscript loses some focus for the uninitiated reader, and though the conclusions are appropriate, the discussion of results overshoot the available data at times. For instance, though this nicely illustrates how microclimate modifies interactions though different pathways, it is not set up on the plant side to really weigh the relative importance of direct versus indirect pathways. Also, without being firmly grounded in the overall outcome of microclimate variation on plant performance, the informative value of the intermediate links among the different players diminishes. Greater clarification of details and analytical framework is also needed, as there's a lot of moving parts and complex inter-dependence among subjects. I enjoyed the paper overall, and with a careful reconsideration of some areas discussed below, it would be a useful addition to the interaction literature. I've grouped my comments, questions, and line comments into overall topics below, and hope they prove useful.  
  
Importance of clearly defining overall effect  
To appreciate the contribution of these different pathways to the overall ("net") relationship between plant reproductive output and microclimate, it is important to clearly link results back to the overall effect of microclimate variation on plant performance. This grounding provides necessary context for illustrating the importance (or not) of these direct/indirect pathways to consequences for the higher order impact of microclimate. Does considering the indirect pathways modify our understanding of potential influences of microsite variation on plant performance? (e.g., temperature did not display an overall effect, but you highlight temperature's importance through indirect pathways)  
Comment: The analysis description should reference predictions, as there's a lot of complicated structure here. Line 165 (overall relationship) is key to everything that comes after - BUT is not clearly indicated as a prediction - the direct pathway is only applicable within the SEM construct, as otherwise you're mixing scales of inference  
Line 270: You go straight into the direct versus indirect, without mention of overall effects of microclimate. This work does not suggest microclimate isn't important, or that it might not be mediated through direct effects on the plant that are not included as pathways (other factors interacting and unaccounted for).  
Lines 282-84: Overall impact of indirect effects versus overall impact of microclimate?  
Line 327: thus, resulting in an overall influence of microclimate  
  
Neighbors and neighborhoods:  
Shoots were mapped, not plants, so neighborhood measurements are not necessarily reflecting independent individual phenological responses; there is likely complex interdependence. As I understand it, the authors should clarify that correlations may not only be spatial, but also could include multiple shoots from the same plant and nearby focal plants as well as separate individuals. They are testing impacts on individual flowers (# seed, egg presence), and though they use separate focal plants, there might be demographic correlations among neighbors and with phenology that are not only due to proximity.  
Line 122 - 5 shoots belonging to 5 different plants, or fewer if few available in subplot - these could have been "neighbors" as subplots were 3x3  
Line 151-153: Neighbor density = # shoots in 3-m radius around target - excludes the focal shoot but not necessarily other shoots that served as focal shoots - so not independent of other observations. They control for this with spatial stats and testing with SEM, but also makes correlated errors (Appendix) understandable.  
Line 257-258: It seems to me that response of neighbor density to variation in microclimate provides some evidence of the overall influence of microsite on the plant population. I would expect low plant growth/productivity with lower temps in drier sites and high growth/productivity with higher temperature in wetter sites. It would be interesting to know whether the degree of ~ neighbor deviance (how different you are from neighbors) might explain some patterns.  
Question: Shoot phenology was based on the most advanced bud, though others might be present on the shoot - how does number of flowers on a shoot relate to resource density for the herbivore (especially in terms of earlier/later snapshot phenology as measured)?  
  
Scale of data collection versus scale of inference - especially moisture and plants  
The data used are collected over a short season at a single site, and (I presume) really indicate a snapshot within the entire phenological trajectory of the local population. This is a complex analysis, and for one data point/range of conditions is a good proof of concept employing single flower counts and fruits. I think these results are interesting, but the next step would be to compare outcomes in different fields/locations that occupied different ranges of studied gradients and take more thorough measurements of both gradients before making broad inferences. For example: Line 235: was this range sufficient to drive detectable differences? Also was a single timepoint measurement of soil moisture adequate to reflect soil holding/ differences in dry down and reflect biologically meaningful differences?. The two microclimate variables are not measured with the same intensity.  
Seems like microsite effects on plant characters would really have to swamp all the indirect pathways to truly display direct effects. There's a lot of small effects strengthening indirect pathways, but very little included in the model to explain variation that might tie microclimate directly to plant reproduction outcomes. Microclimate may directly affect plant-level mechanisms such as growth rate, seed quality, or belowground storage that are not considered. I'm not saying additional complexity is needed for this analysis, but I do think stated results should be qualified to acknowledge that the study was designed to really tease out mediation through the indirect pathways, not necessarily to understand as much about the direct pathways (Line 279-80).  
Question: Is it true that small-scale conditions have rarely been explored? (Line 46,15, etc.) Aside from the ecological literature, the agronomic/horticulture literature thinks about soil temperature and moisture a lot.  
Question: Climate refers to large-scale, long-term patterns. Are you saying microsite variation can inform predicted responses to climate variation? (e.g., 59-62) - or shifts in the range of variation? What's the difference between microclimate and microsite differences as discussed in the literature?  
Question: So host plant abundance measured as density = herbivore resource that interacts with preference for early flowering?  
  
More clarification required  
This analysis has a lot of moving parts, and a careful reconsideration of how it is explained is needed to help the reader follow the logic.  
Line 79-83: It would help if these predictions were employed to help the reader follow the complex set-up and network of connections  
Line 131: introduces "points" (N=254) which are henceforth referenced. It took a second reading to see that these were the corners of occupied subplots (Line 110: N = 154), and were used to interpolate measurements over space for individual shoots within subplots. This was confusing initially, because plants were measured in subplots, but variables were measure at points. Maybe say "points (= subplot corners)" for reminder when discussed again, to remind reader how points link to the gridded subplots.  
Line 207-210: I thought at first a box was missing from the diagram (see "Figures" comments"). I assume this is why the TxM box to Fruits is missing. Please make this explicit and mentioned it in the figure legend.  
  
Miscellaneous line comments:  
Line 113 - parenthesis error  
Line 184-185: you aren't measuring changes in phenology or context, but differences associated with variation. These are points/individual snapshots at a single time point.  
Lines 186-198: lead with the response variable - will help reader link associated model with predictions  
Line 215-217: Not surprising as these are complex and don't include actual plant parameters except at the individual flower level.  
Line 259-261: as expected from previous studies  
Lines 264-5: I would say it's more along the lines of, "… highlights the important role of indirect effects that mediate small-scale patterns in performance related to microclimate."  
Line 286-288: plant stress responses could also respond to microsite variation and consequently affect herbivore interactions  
Line 293-4: I agree  
Line 302: Maybe why you see any differences at all.  
Line 314: because of competition? Seems like warmer, moister soils would be more productive  
Line 319: what do you mean by "important" - the strength of the effect was lower?  
Conclusions (334-345): set out results appropriately.  
Figures:  
1) Need to indicate why TxM->repro box is missing. You may also want to reference the enumerated analysis models in appropriate variable boxes  
2) and 3) how did you determine the cut-offs for these lines?  
  
  
  
  
  
Reviewer #3: The authors present the results of an observational study investigating the interacting effects of soil moisture and temperature (microclimate), ants, herbivory by butterfly larvae and plant density on seed set in a long-lived native grassland forb, where microclimate is defined at the 3 m scale. They find that the direct effects of microclimate on seed production are less important than the indirect effects, mediated by the other factors included in this study, particularly species interactions. While effects of climate on reproduction are often examined across populations (rather than within) and thus geography, I agree with the authors that we know much less about neighborhood level effects, particularly small-scale variation in microclimate, may mediate the outcomes of species interactions. Overall, I found the paper easy to read and the results were clear. However, I struggled to distinguish this manuscript from the earlier papers published by the same authors (2018 Oikos and 2019 J of Animal Ecology). The paper would be a stronger contribution if it included a much more in-depth treatment authors about why microclimate might vary within this particular system and why the authors chose the spatial scale they did. Below I detail these concerns and provide other suggestions for improving the manuscript.  
  
1. I encourage the authors to be much clearer about what distinguishes this paper from their earlier work. For example, some of the results that were cited in the intro (L76-76) were nearly identical to results written in this paper (L259), and as far as I can tell, Figure 3 is more or less a repeat of Figures 2A and 3B from the 2019 paper. While I recognize the addition of the soil moisture data, I think it is important to clarify what about these analyses is new, and only publish the ones that are new. This could start in the introduction (L71). Instead of, "It has previously been shown," write, "In earlier work, we found…" Then tell the reader more directly what this new study adds. Further, while I recognize that the SEM adds an additional level, many of the underlying models are the same ones used in the 2019 paper, and it appears that the SEM itself examines many of the same pathways as the 2018 paper (which also included data on soil temperature).  
  
2. In the introduction, quantify the spatial scales - what is large versus small? What do you mean by small in this particular context? It takes a while to get to the methods where the 3 m scale is brought up. Related to this point, why was a 3 m scale chosen? To me, this seems big for a neighborhood scale in terms of competition, anyway. And the spatial autocorrelation figures suggest that the autocorrelation drops off rather quickly (that 1 m is not that different from 3 m if I'm interpreting the figures correctly).  
  
3. Can you write more about your expectations for why and how the microclimate varies across the field? Is there a gradient from wet to dry due to elevation? What would make temperature vary by 3 C across such short spatial scales? Trees? This is a critical component that is missing from the paper. As much as this can be explained in the methods, but also in the discussion, will strengthen the manuscript. I kept searching for a mechanism as I read. I noticed that it one of the earlier papers mentioned grass height in relation to temperature. Are there other factors? Can you make plots showing how temperature and soil moisture vary across the field? (analogous to Fig 1 in the 2019 paper)  
  
4. I was initially concerned about spatial autocorrelation, and then happy to see how it was corrected for in the statistical analyses. I would have appreciated another sentence or two describing how this works, because I think it's key for this study.  
  
5. In Figure 1, can the line thicknesses be adjusted to show the varying strength of the effects? Change the colors to show the difference between positive and negative effects? Here might also be a good place to highlight what is new compared to the earlier research.  
  
6. Finally, in the discussion and/or conclusions, a longer commentary about what researchers might expect in other systems would be helpful. I think most people would not expect say temperature to vary across a grassland - but when should we be looking for these microclimate differences? What about alternative hypotheses for individual variation? Surely there is a genetic component as well, or would you argue that the differences observed are all environmental (this interaction between microclimate and herbivory).

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**Please find below a few general notes on BAAE journal style.**  
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The final submission should contain only one version of the manuscript. There should be no annotations and no correction marks. You may, however, type changes/additions in colour.  
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Title page

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"Conclusions" (optional"  
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***Basic and Applied Ecology***  
**Appendices and Supplementary Material**

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Basic data such as species lists or compilations of data describing environmental conditions of numerous study sites may be extensive in some studies. While this kind of information is valuable and of interest to colleagues working in the same field, it may be less so for the majority of readers of a journal.

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Data in Brief (optional):  
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