Authors’ reply to reviewers

Comments from Reviewer 1

I have read and reviewed the manuscript titled “A temperature-driven model of

phenological mismatch provides insights into the potential impacts of climate change on

consumer-resource interactions” (JAE-2021-00431). The authors develop theoretical tools to

understand the impact of changing temperatures on the phenology of a consumer and its

resource. They further apply these tools to the spruce budworm - balsam fir system and

predict the possible changes in their phenology and the resulting mismatch that might

occur.

The manuscript takes concrete steps towards solving an important issue – prediction of

changing phenology in the context of anthropogenic climate change. The theoretical tools

developed by the authors are agnostic to the accumulation function used and are thus

widely applicable. The authors develop these tools for couple of different scenarios of

temperature increase, which is useful. The application to the empirical system is tightly

interwoven with the theory and provides a nice demonstration of the tools developed.

**#** Writing comments

While I think the manuscript makes significant contributions, it would benefit from better

development and perhaps some restructuring. The manuscript is currently written as an

empirical paper (with Introduction, Methods, Results and Discussion). However, this causes

the authors to jump back and forth between the definitions of the functions and their contributions which can be a bit hard to follow. For instance, Section 2.1 is titled Theoretical

Development, where the authors develop their main equation (Eq 1) and provide background on the subject. However, the equation is solved under two different scenarios

in Sections 3.1.1 and 3.1.2. These solutions enable the application to data and are thus the

main contribution of the paper. As it currently stands, the significance of this gets lost a bit

on the reader.

To fix this issue, I would suggest restructuring the manuscript so that the theoretical

development (the equation and the solutions under constant increase in temperature and a

warm spell) comes immediately after the introduction. This will allow for the development

of the theoretical tools and the authors can use this opportunity to emphasize the

generality of their tools. After the theory has been developed, the authors can then use the

budworm-fir system as an example to showcase these tools. In this section, the

accumulation functions can be specified and predictions specific to this system can be

developed (Figs. 3, 4 and 5). I would strongly suggest adding an illustrative figure here to

demonstrate the life cycles of the budworm and the fir with an annual timeline including the

stages of diapause and quiescence. This will make the phenology mismatches clearer and

easier to describe. Overall, the new structure will help the flow significantly and will

highlight the novel contributions better.

**#** Scientific comments

Apart from the comments on the structure of the manuscript, I have some scientific

comments as well. I list these below:

1. The authors base their entire analysis of the impact of rising temperatures on

consumer-resource systems on the heat accumulation function. While heat

accumulation is crucial in how temperature might impact consumer-resource

systems, temperature is also expected to impact physiological processes in other life

stages. Moreover, the model completely ignores ecological and evolutionary

dynamics of consumer-resource systems. Therefore, it is important that the authors

explicitly address these limitations in the text and place their findings in context.

2. The authors use a specific function as 𝑅(𝑥(𝑡)) for the budworm. The study that the

authors reference also lists a couple of other formulations. It might be useful for the

readers to know why that specific function was chosen as opposed to others.

3. It isn’t immediately clear whether the generated temperature series under different

warming scenarios (in Fig. 5) are analyzed using the constant difference equation

(Eq. 5) or the warm spell equation (Eq. 6). While I assume it is the former, it will be

helpful to explicitly state that when discussing those results.

4. In Section 3.1.2, it might be beneficial to explore different combinations of sensitivity

and phenology between the consumer and the resource in a systematic way. Right

now, the authors focus on the case where the resource emerges before the

consumer and is more sensitive at lower temperatures. It might be worthwhile to

examine other scenarios here as well – consumer emerges first, consumer has higher

sensitivity etc. If pressed for space, the authors can focus on the cases they think are

most biologically relevant. This analysis will show that the tools are potentially

applicable in situations which might be significantly different from the budworm-fir

system.

# Minor Comments

There are a few relatively minor issues that need to be fixed in the manuscript. I list some

here for the authors’ convenience. These mostly pertain to reproducibility and typesetting

issues and should be relatively easier to address.

1. Currently, the theoretical figures lack parameter values. Even though some of the

figures are illustrative, it is useful to provide the parameter values for the purposes

of reproducibility and further analyses. They can be added to the figure captions or

be provided in the Supplementary Information.

2. In Fig. 1D, the dark dashed scenario is not shown. Further, it might be helpful to

mark 𝑡b\* and 𝑡e\* on this figure. The mismatch can thus be clearly stated as 𝑡b\*− 𝑡e\*,

which will correspond to the text.

3. For reproducibility, the authors should also list the tools used for the analyses –

programming language, any relevant software packages etc.

4. There are some typesetting issues (e.g., Eq. 3) and some minor notation errors (e.g.,

Eq. 7 should say 𝑥i and not 𝑥1) in the manuscript. I would suggest a careful read of

the manuscript with these in mind at the end.

In summary, the advances made in the manuscript are interesting and useful. However, the

manuscripts will benefit from some restructuring and some additional elaboration of the

theoretical tools.

Comments from Reviewer 2

The authors present a theoretical framework and case study for determining how changes in

temperature impact phenological mismatch between insect herbivore and its food resource. This work builds on a pre-existing mathematical model for emergence and budburst time as a function of temperature, by 1) using this model to link interacting species as a way to measure resource – consumer mismatch, 2) extending this modeling framework to allow for approximating how mismatch changes given different temperature effects and then making generalizations from these efforts, and 3) validating the theoretical predictions using heat accumulation functions fit to spruce budworm-balsam fir data. Overall, I found this paper to be an excellent and worthwhile contribution to this area of research and I really enjoyed reviewing it. For the most part, the paper is clearly written and logically consistent. My main criticism is there is a lack of methodological details regarding fitting these emergence models to the actual SBW/fir tree data and interpreting the resulting trends as a function of latitude (Fig 5). Also, there were various points where conceptual clarity could be increased. I should point out that my differential calculus skills are too rusty to properly vet some of the math components of the main text and all of the supplement. Therefore, my specific comments below regarding the theoretical models are not criticisms of the method itself but rather from the perspective of the potential audience for this paper that may wish to apply these methods to their own system.

# Specific Comments:

Line 107: Your theoretical model is somewhat more restrictive than applying to any

consumer/resource pairs that “responds to the same climatic factor”. It also seems restricted to

defoliators whose most vulnerable life stages respond positively to plant vigor (as opposed to

plant stress), overwinter outside as opposed to inside their host plant, and who emerge from

overwintering directly from into the life cycle stage that corresponds to feeding on new plant

growth (as opposed to emerging as adults which must then lay eggs first, which must then hatch before early instars are able to feed on new growth). These are a specific set of conditions that form a somewhat sizable subset of all insect herbivore/plant interactions and it would be worthwhile pointing this out more explicitly. Just to be clear, I would also stress that while your mathematical approach for modeling mismatch could eventually be expanded to different insect herbivore guilds, etc., I am not advocating that you do that here in this paper but rather that you better clarify the subset of plant/herbivore interactions your model as is currently applies to.

Fig 1C. Should the y-axis be labeled R(x(t)) and not R(x)? I could be wrong about this.

Line 126: I had a hard time understanding what you meant by “the proportion of the

corresponding life-cycle stage that they have completed.” Perhaps clarify this.

Line 144 and Fig1D: Is it necessary that both species emerge at the same F value (F= 1)? I see an F = 18.6 for the fir model (line 310), yet in this Fig 1D conceptual plot it seems as though both species accumulate the same amount of heat in order to emerge.

Line 148-151: I would add t\*e and t\*b to Fig 1D to better link the plot to the text conceptually.

Line 172: If SBW development is delayed due to late emergence (increased phenological

mismatch), wouldn’t this in turn impact the life stage entering diapause the following winter?

Meaning, could you have a situation where mismatch is temporally dependent on mismatch the prior year due to altered insect development time as a function of emergence date (i.e., runaway mismatch)?

Line 206-212, 309-312: How exactly did you fit this model? I don’t see any of the details regarding that here. The lack of methodological details doesn’t provide any context for evaluating whether PRCC (lines 212-213) is needed/sufficient.

Line 246: I see how the phenological shift due to changing temperature depends in part on the

derivative of the accumulation rate (what you term its sensitivity) for temperature spells (your

second case) in Eq. 6 but it is less clear how R’ matters for the first case (a constant temperature increase) in Eq. 5. In Eq. 5, it looks that you integrate R’ from t0 to the original emergence date. If that is the case then wouldn’t species who have steeper accumulation curves have larger phenological shifts to any temperature increase? This seems different than the second case (Eq. 6) where the shift is more about R’ at the time of the temperature increase. I think you should clarify here how R’ plays a somewhat different role in both cases introduced on lines 234-235. This confusion over the differing role of the rate accumulation derivative in Eqs. 5 and 6 spills over into lines 257-265 a bit.

Line 254: Should this be R(x\_1(t\*\_1)) and not R(x(t\*))?

Fig 2A. There is no solid and dashed. There is dashed and dots. Also, where is the predicted value coming from in this paper? This was unclear.

Line 269: Would you need R(x(t)) for both species? Here you are using R(x) for fir and R(x(t)) for SBW. I thought you had to fit R(x(t)) for fir using the Uniforc model? Sorry, I guess I am not seeing how you link both of these to x(t) here.

Fig 2B: Label the plots with R’\_e and R’\_b rather than R’\_1 and R’\_2 to match the text and improve comprehension.

Line 294: c subscript 1

Lines 313-319: I don’t think you can really draw the conclusion that the accuracy of both models is satisfactory from what appears to be a small amount of withheld data. For the fir Uniforc model, your measured budburst window is quite large relative to meaningful mismatch values (1 month for one site and 2 weeks for the other). These bounds are so large it seems that even a poor model could predict the median budburst in the 2013 in this interval and for 2014 your median budburst falls outside your interval. For the insect emergence time, the predicted and observed values also seem discordant. I don’t really know a better way to assess predictive performance here since there are not any details on how you fit this model, but some type of cross-validation approach would be more robust here.

Line 330-331: I don’t see a strong effect of latitude on mismatch visually in Fig 5C for the 1996-2016 period temperature data. You have not demonstrated a mismatch trend across latitudes statistically and I suspect a trend line between mismatch and latitude would be flattish, especially given the uncertainty in the response (mismatch). The mismatch by latitude relationship seems more pronounced for the RCP scenarios than the historical data.

Lines 332-333: Your SBW model returns median emergence (I think, based on line 318) and does not model the variance in emergence date for the population. Given this, I am not sure how you can assert that the whole larval population has time to emerge prior to budburst at lower latitudes. Is there some ancillary life-history information that you are bringing to bear here to connect a 5-10 pre-budburst SBW emergence with allowing enough time for entire larval populations to emerge?

Fig 5: I am unclear about what these confidence interval represent and I think that the ones for

white boxes (BioSim) differ from grey boxes (RCP x.y). For the former does the interval represent parameter uncertainty in the emergence estimates and does the latter represent uncertainty due to the different RCP simulations for each group? You should clarify this in the caption.

Line 338: Do these RCP scenarios differ in terms of amount of warming? If so, you should state that back when introduced on lines 221-224.

Line 340-343. You reference Fig 5A for budburst but 5A is labeled emergence, and vice-versa. I don’t know which plot is for which species.

Line 348-349: How much do these mismatch differences you see across latitudes matter

biologically? The SBW-fir system is well studied and I am wondering if you can infer the degree to which survival or other life history traits may be affected by these mismatch differences in 5C. You cite one paper early on that looks at this (Lawrence et al., 1997).

Lines 352-354: There does not appear to be any difference in mismatch variance across the three RCP scenarios for the northern sites. The intervals on Fig 5C for the northern sites look virtually identical. What am I missing here?