Reviewer Comments:

Reviewer #1 (Remarks to the Author):

This is my second time reviewing the manuscript and I greatly appreciate all that the authors have done to address my concerns from the original version. The figures are much more interpretable, the addition of point 5 (lines 363 – 368) to the discussion rounds it out nicely, and the inclusion of the “adaptation lag” analyses broadens the scope of the paper even further. One remaining question I have is with regard to the adaptation lag analysis. Looking at figure there seems to be a slight U-shape to the plot for the no migration data, with an increase in lag at low rates of climate change. Do the authors have a comment on why this is?

**-There are two factors at play here. First, the y-axis is scaled by generations of climate change, which means that at low rates of climate change, the populations are much closer to the optimum, despite having higher “generation lag”. Second, there is a slow down in adaptation when close to the adaptive peak, consistent with Fisher’s geometric model. When further from the adaptive peak, all positive QTL will be beneficial, but when closer, some may overshoot the optimum and be deleterious.**

**This point is fairly tangential to our core results and we would rather exclude discussing it in the interest of space.**

Minor comments:

The start of the sentence on line 57 is strangely phrased, seeming almost combative. Could consider rephrasing or removing.

**-This wasn’t intended to be combative, but we agree it comes off that way. We’ve removed the entire start of the sentence. Line 34**

Line 69, change “extensive” to “extensively”

**-This line was removed during reformatting.**

Line 417, remove “introgress”

**-This line was moved to the Supplementary Discussion and has been fixed.**

It would be helpful to point the readers to the figure(s) that illustrate the points being made on lines 418 – 423.

**-We don’t have a figure that directly outputs fitness comparisons, but introgression of deleterious alleles is shown in Figure 1, which we have added a reference to (now in the Supplementary Discussion).**

The figure legend for Figure 4 does not appear at the end of the manuscript.

**-Corrected.**

Reviewer #2 (Remarks to the Author):

The authors have substantially revised their manuscript, and produced what I now consider to be a clear, novel and broadly interesting investigation of the possible impacts of adaptive introgression in the context of climate change. My remaining comments can be addressed largely by some additions to the Discussion.

1. The finding that higher std dev of QTL mutational effect sizes leads to less breakdown of species boundaries needs to be discussed in the context of the recent work on Fisher's geometric model and speciation (e.g. Fraisse et al. 2016 Evolution; Simon et al. 2018 Evolution Letters).

**-We don’t think this model is directly applicable to our study. The geometric model of speciation suggests that species will have different QTL for the same fitness landscape, and that hybridization brings together combinations of those QTL that are unfit. In our case, the climate QTL effect STDev is more about the potential for each population to chase the optimum. With low QTL effect size STDev, there aren’t enough QTL to adapt in a single population, so they fall far from the optimum and any positive introgressed QTL becomes very beneficial. Given this and space limitations, we don’t think it’s worth discussing this.**

2. Line 437-439. I think it's important to note here that, as seen in Figure 4, adaptation lag only becomes a problem when the rate of climate change is high, and hence introgression is increasingly beneficial with increasing rate of climate change.

**-This is a good idea. We’ve added a sentence to make this point. Line 171**

3. Perhaps consider citing Martin et al. 2019 PLOS Biology, which provides a very clear example of the effect of recombination on introgression.

**-Great example. We’ve added this to our citations for recombination and introgression, which is now in the Supplementary Discussion.**

4. The addition of intrinsic incompatibilities has definitely improved the manuscript, however there are of course many ways to model intrinsic incompatibilities, and the authors used only one. I think this is acceptable, but it would be worth adding a brief discussion of what we might expect under different models, in particular I am thinking of the 'pathway type' incompatibilities of Lindtke and Buerkle 2015, which were shown to be significantly more robust to migration.

**-We have added a sentence discussing this other possible intrinsic isolation method, and our expectations about it. Line 104-109.**

Reviewer #3 (Remarks to the Author):

The authors appear to have adequately addressed the concerns from the previous round of reviews. I have no further comments for the authors.