Overall I thought this was a well written manuscript and a well conducted experiment and modeling exercise, tackling an interesting question. In particular, it is an interesting case study for why demographic approaches to species range questions may improve on occurrence or abundance based SDMs. The combination of sex-specific climate responses and feedback between sex ratio and reproductive success is not something that could be captured with a standard SDM, as far as I can imagine. Although there were some differences in predictions made with and without taking into account this feedback, I also appreciated the authors' balanced treatment of the findings, discussing how the need to incorporate this biological nuance may depend on the questions of interest to researchers. For generalizing this result, a lot seems to hinge on the point they raise about needing to know the costs of reproduction for different sexes for more species. But this paper offers a useful case study for how dioecious species may respond to changing climate.

I had a few comments:

- Overall the authors do a commendable (and appropriate) job of propagating uncertainty in their analyses. However, it was hard for me to tell whether that was also done for the parameters estimated in the previous sex ratio experiment, or if mean parameters were used? This seems quite important as that's the key relationship for distinguishing the two-sex model. Fig S13 shows how seed viability is related to OSR in that experiment, declining over ~ 75% OSR, but also highlights the very large apparent variability in that relationship. It was also unclear to me whether seed number was affected by OSR and was included in the model, or only viability? It would be nice to include in this paper some discussion of why OSR affects seed viability, for those readers not familiar enough with plant reproductive biology. I might have thought OSR would primarily affect seed number rather than viability. Do unfertilized ovules produce non-viable seeds in this species (they're not simply aborted)?
- It seems important to have some discussion of the potential mechanism by which dormant season climate could be important, and how these predictions are different than for growing season climate.
- Why does precip have a negative effect on most vital rates in this seasonally arid region?
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Other

- Since there are mixed models of the vital rates, as a continuous function of size, I didn't follow why discrete MPM were used instead of IPMs. I assume there's a good reason, given the authors' expertise, but not clear why discrete model used and how all the individual transitions were estimated. Maybe the mixed models were discretized, like an IPM ends up doing in practice, and I just didn't understand? It's hard to imagine how climate effects on that many discrete transitions would be estimated.
- And U is 35 tillers; how many size stages do the models have?
- Fig S3 – says 95% CI but two intervals shown
- L104 says most sex coefficients were signif, but this isn't obvious from Fig S3 (most seem overlapping zero); perhaps authors could be more specific about which rates they conclude are signif, or include probabilities of overlap with zero.
- Text says 8 source pops. In Fig 1 I only count 7?
- Fig 2 – maybe show shaded uncertainty regions on the regression?
- L176, 179 – are these referring to the wrong figure panels? E and F show the difs between the sex and no-sex models I believe
- Fig 3 – the plot of past, current and future points is hard to eyeball any patterns. Maybe in addition a histogram or density plot of the values to show any shifts in probabilities?