

The revised paper presents an improvement in several respects. At the same time, the authors refrained from checking / following up on most of our comments, and with the difficulty with some of their biological interpretation.

In particular, we still find the switch to a linear model to be somewhat of a step back in terms of interpretability. A key example is in the confusion that arises about the relative importance of mutation rate variation compared to linked selection in determining levels of nucleotide diversity in *Drosophila*, where after all this paper's bottom line is quite diverged from the rest of the literature.

Another (smaller) example is in their answer to R1.7, where they interpret lower autocorrelation of the iSMC recombination transition parameter as evidence that "recombination rate varies more often than the mutation rate." Couldn't lower autocorrelation instead result not from frequent variation in recombination rate window-to-window, but relatively few windows with extreme shifts in recombination rate relative to their neighboring windows?

While the authors could have done more to engage with the comments and critique about difficulties with interpretation, at the end of the day we think the presentation is now self-contained and publishable. It is a worthwhile contribution to the literature and ongoing discussion in the field.