Response to the referee

We thank the referee for their critical assessment of our work. In the following we address their concerns point by point.

Referee Point 1) — Since neither set of models using the two different opacity tables provide a good match to the properties of the Jao gap, the possibility that the convective kissing instability is not the correct explanation for the gap has to be considered.

The first paper to offer an explanation for the existence of the gap (MacDonald & Gizis 2018) states 'Convective mixing is treated as a diffusion process with the diffusion coefficient determined from mixing length theory.' and 'The fully implicit nature of our code also prevents the convective kissing instability discovered and described by van Saders & Pinsonneault (2012).' The treatment of convective mixing as a diffusion process is probably the reason why MacDonald & Gizis find a single episode of convection zone merger (for models in which merger occurs) and hence a single dip in the luminosity function, which seems to be the case for the Jao gap (inferred from figure 1 of this paper).

In contrast, the use of instantaneous mixing leads to multiple episodes of convection zone merger and this could be the reason why the authors find two dips in the luminosity function (inferred from their figures 7 and 8). The authors need to give in the paper a convincing argument as to why the instantaneous mixing approximation is valid, particularly as using the diffusion approach seems to more physically consistent with current understanding of turbulent mixing. This should involve estimates using mixing length theory of the mixing time scale at all stages of the merger, with special attention to mixing time scales within one mixing length on either side of the point of contact between the merging convection zones. This region is where the mixing time scale is most likely to be the longest.

The mixing time scale then needs to compared to other relevant time scales including the time scale for deuterium to come into equilibrium and the time scale at which the helium-3 abundance is modified by nuclear reactions.

If it turns out that the instantaneous mixing approximation is not valid, then there needs to be discussion of alternative approaches and their consequences. The thrust of the paper could be changed to show that the CKI is not the correct explanation of the gap.

Reply: We disagree with the referee that the a diffusive model of convective mixing should be prefered over an instantaneous model of convective mixing. Much of the following has been included as text in the manuscript.

As the referee says the primary point to clarify here is the ratio of the overturn timescale to the timestep length. DSEP treats convective mixing instantanously throught a single shell and we evolve models composed of 5000 radial shells. Convective overturn timescales for M-dwarfs have not insignificant uncertanties on them (with different sources reporting overturn times between 70 and 300 days). However, all of these estimates are order of magnitude shorter than the time steps used (which bottom out at around 1 Myr). Therefore, convective mixing is extremly well approximated by instantanious mixing.

To get a better sense of this for our models we find the overturn time for a single shell at the radial depth where convective kissing instability happens in our models and compare this to our time step length. For a single shell the overturn time is [DAYS], which is [FRACTION] shorter than the time steps we used.

Moreover, through private communication with Gregory Feiden, a leading expert on the Jao Gap, we have recived independent cooberation of these time scales.

Therefore, we do belive that instanious mixing is a valid appoximation to make for these models. The thrust of this paper will therefore remain the same.

The text addressing this has been added into the section entitled Modeling.

Referee Point 2) — The convective mixing method used by MacDonald & Gizis needs to be properly described along with the resulting differences from using instantaneous mixing.

Reply: Additional text has been added to the introduction of the paper addressing the resulst of MacDonald & Gizis.

Referee Point 3) — On line 184, the authors say that the OPLIB tables were created to resolve the discrepancy between helioseismic and solar model predictions of chemical abundances in the Sun. I find this to be an odd way to phrase the problem. Presumably the authors are referring to the difficulty of making solar models that match the sound speed profile (or almost equivalently the depth of the surface convection zone) determined by helioseismology given the composition constraints provided by the then recent new measurements of the surface abundances, notably the oxygen abundance. The authors should rephrase the problem and also discuss whether using OPLIB opacities help resolve the problem or not.

Reply: We agree with the refee that the invocation of helioseismic discrepencies is confusing. Because this is not a helioseismic paper we drop from the text, instead just mentioning that the OPLIB tables make use of the most up to date physics, which is the most relevant point to our work. We do note here though that the OPLIB opacities did not serve to resolve any discrepencies.

Referee Point 4) — The distinction between low temperature and high temperature opacity sources made in the paragraph beginning on line 196 is somewhat artificial. Presumably the transition between low temperature and high temperature opacity is chosen to be between $10^{4.3}$ and $10^{4.5}$ K because there is where the Ferguson et al. opacities and OPAL opacities are close to each other. Perhaps the evolution could be affected if a different temperature range for the transition is used. The authors should stress in this section that as far as modeling the gap the main impact of using different opacities is on the radiative zone, and give the temperature and R (or density) ranges that are relevant to the radiative zone(s) so that the reader can see from figure 3 the expected change in opacity, and also if log R = -1.5 is truly a representative value in the radiative zone.

Also, from figure 3, it seems that the OPLIB opacities are lower than the OPAL opacities for temperatures greater than $10^{5.5}$ K and not 10^{5} K as stated in the figure caption and also on line 203.

Reply: The range where DSEP ramps from low temperature to high temperature opacities is determined by the temperature range where molecules can start to form. It is important for the low temperature opacities to be the only opacity source by the time the first molecules start forming. We choose to ramp the opacity source so that there is not a hard discontinuity. This same ramp has been used as standard in all DSEP models since 2008, see Dotter et al. 2008 for further details.

Referee Point 5) — In section 3.2, mention is made of the solar surface Z/X ratio but the actual value is not given. Is it the value recently determined by Magg et al. (2022), Z/X = 0.0225 or some other earlier value? The authors should state the actual Z/X value used.

Also, the authors need to say whether or not they include gravitational settling and element diffusion in their solar modeling, and if they do, say how it is done (e.g. are elements grouped or treated independently). The authors should also include discussion of how well their solar models replicate the sound speed profile determined from helioseismology.

Reply: The calibrated Z/X value has been added into the text in section 3.2 along with clarification that we do include gravitational setteling with elements grouped together. We do not include a disscusion of sound speed in our manuscript. This paper is not a disscusion of seismology so a diversion to that would be disctracting for readers and out of place.

Referee Point 6) — Presumably, the authors use their solar calibrated models to set the mixing length ratio and initial abundances for their calculations of the evolution of models of low mass stars. Do they use primordial or present-day solar abundances? Why should the mixing length ratio be the same as the solar calibrated? There is evidence that the mixing length varies with stellar properties (e.g. Trampedach et al. 2014; Joyce & Chaboyer 2018). A better fit to the location of the gap might be obtained by adjusting the mixing length ratio. The authors need to address these questions.

Reply: We use GS98 solar abundances [CITATION] for all models. While, as the referee says, there is substanntial evidence of a metallicity dependence for the mixing length parameter this dependence has only been shown to have a substantial effect on higher mass stars. However, to fully address this point we have run an additional grid of models with the mixing length parameter dramatically lowered ($\alpha_{ML} = 1.5$). Results of that grid are shown in Figure [FIGURE]. Of primary note however, is that the Jao Gap loaction is within one sigma of where we detect in with our solar calibrated mixing length. Text has been added into the paper clarifying that we use a solar calibrated mixing length but that the actual Jao Gap location of our models is only very weekly sensitive to mixing length.

Referee Point 7) — On trying the web interface for the OPLIB, it seems that the process of interpolating between rho and R is unnecessary. Once T is specified, it is possible to get the same set of R values as for the OPAL tables by specifying the starting value of rho. Then only interpolation in T is needed to get a table in the same form as the OPAL tables. The authors need to clarify why they chose their approach. Also, it would be helpful to include in figure 15 a line plot of fractional difference against log T for log R = -1.5 or a different R value if -1.5 is not found to be representative for the radiative zone (see point 4).

Reply: While it is possible to pick out particularl R values and get them directly with the OPLIB webform it is not possible to get the same grid of R values from the web form as the type 2 OPAL tables report opacities over. This is because the grid of temperatures OPLIB populates does not line up with what would be needed. Therefore, the interpolation scheme described in Appendix B is required.

Minor

Reviewer Point 8) — Typo in line xy.

Reply: Fixed.