Massive Stars Paper - Referee Response 2

We thank the referee for the additional comments on our manuscript. Please find below our responses to the referee's comments - our responses are highlighted in blue text.

The text in italics is extracted from your response.

1. We have added Figures 7 and 8 to now display the accretion rates and mass histories of all stars in both Halo A and Halo B. Previously, in the old Figure 7 we showed just the mass accretion rates for the most massive star and the median accretion rates. The accretion rate onto a star includes mergers that occur during that timestep

Physically, should a merger with another star count as "accreted mass" in terms of allowing a star to be/remain above the critical accretion rate? If not, please note it in the manuscript. There are obvious spikes in both Fig. 7 and 8 and in particular in Fig. 8 there are spikes that bump the accretion rate above the critical rate. In Fig. 8 a clear spike appears for the Most Massive Star and from the text it appears that it's indeed a stellar merger. Please note this in the caption of the figure as well.

Stellar mergers are counted as part of the accretion rate. Simulations of massive star formation (e.g, Meyer et al. (2020)) do show that stellar mergers do increase the "bloatedness" of a star and hence including the mergers in the accretion rates is likely to be valid.

The stellar merger in the left hand panel of Figure 8 has now been noted.

2. —— *Stars are never "still" and have relative velocities of between 5 and 20 km/s*

The widespread fragmentation and the turbulent environment can easily justify this motion, however I also note that the dark matter particle mass is 170 solar masses: this would induce shot noise for stars less massive than a few hundreds of solar masses. Please comment on this in the manuscript.

This has now been addressed in the manuscript in Section 2.4. The impact of shot noise is alleviated by smoothing the dark matter particles on a scale of approximately 1 parsec (i.e. the dark matter density is smoothed).

3. —— *We have added some additional text to the paper to clarify our reasons for using HaloA with a LW field and HaloB with one (footnote 3).*

Footnote 3 reads "Initally our simulation plan had been to run four haloes with and without a background LW field at a refinement level of 24 (16 times higher than we report here). However, the simulations proved intractable and computational expensive and hence we reduced the scope of the campaign. We are now in the process of undertaking a new more expansive campaign." and does not explain the reasons for choosing HaloA (rather than haloB) to be run with a LW field and HaloB (rather than haloA) to be run without the LW field nor it expands on the consequences in the interpretation. I copy below the set of questions from the first report, which I'd like to see addressed in the manuscript: Why HaloA with LW radiation and HaloB without and not viceversa? Why not both with and without LW radiation? Why not only one, but with and without LW radiation? This last point is important: HaloB does not form massive stars, but there are two factors (i) no LW radiation, but also (ii) a lower mass ("HaloB, on the other hand, contains essentially only a single site of star formation due to the significantly smaller halo mass."). HaloB forms less massive stars but also fewer stars overall, so it seems that there is less fragmentation although the absence of LW radiation should favor fragmentation… the text says ""HaloB is therefore the control simulation and allows us to examine the impact of what happens when no LW field is present" but the control would have been sounder if it were the same halo to be simulated with and without radiation. Although I am not asking to run a new simulation at least a stronger justification in the manuscript is needed.

The reason we did not run Halo A without a radiation field is that we simply did not have the computational capacity to undertake this run. The same reason is true for not running Halo B with the computed radiation field. As explained this was not our intention at the start of the campaign. However, our original strategy proved both too computationally expensive and too challenging for the hydro solver. This in turn had knock-on effects on our ability to be as "simulation complete" as we would have wished. We have adjusted our footnote to explain this more clearly.

4. —— *The work of Toyouchi et al. (2020) expands on the work of Park & Bogdanovic (2017, 2019) by investigating not only the more rarefied environments but also high density environments similar to those investigated by Inayoshi et al. 2016. Toyouchi et al. (2020) find that in the denser environments with \rho*\inf > 10^6 cm^-3 *that the dense clumps provide a sufficiently negative force to cause a declaration of the black hole (for black holes with masses of M*{BH} > 10^4 \msolar). *We have updated the text to clarify this*

The text is remains confusing. Pfister et al. (2019) found that black holes with masses less than 105 M☉ do not sink to the centre of a galaxy, but instead exhibit random walk characteristics. However, their simulation neglected the impact of any radiative feedback from the black hole. -> If they had included radiative feedback from the black hole the result of "not sinking" would have been magnified. Furthermore, what Toyouchi et al. (2020) show is that if the gas density is sufficiently high then radiative feedback has no effect, and standard (not faster) dynamical friction deceleration is recovered. Quoting from Toyouchi et al. (2020): "The absolute value of the acceleration with n∞ = 106 cm−3 is |aY| ~ 10−5 cm s−2 , in good agreement with the dynamical friction in the BHL case with the same density. This suggests that the radiative feedback no longer affects the orbital evolution of IMBHs in such a dense environment. The resulting timescale of the BH deceleration is only τdec ~ 0.01 Myr, much shorter than the dynamical timescale in galactic gas disks. " Finally, looking at your Fig. 10 (top-right panel) the high density conditions are not very common.

It was not our intention to imply that radiative feedback decreases the potential sinking time. Instead our intention was to describe how dense gas (or a dense stellar environment) is required to efficiently cause a BH to sink to the centre. Pfister et al (2019) conclude that BHs embedded in dense bound stellar structures or envelopes would be ideal while Toyouchi et al. show that the radiative feedback (from a BH traversing such environments) will not adversely affect the sinking time-scale. We have updated the text to clarify this.

5. —— "however our massive black holes should be sufficiently massive to avoid widespread ejections (Holley-Bockelmann et al. 2008)." The key parameter in gravitational recoil is the mass ratio of the merging BHs and not the absolute mass (Holley-Bockelmann et al. 2008 was considering mergers of an IMBH with much lower mass BHs, that's why in that paper there was a sort of mass dependence). You are considering mergers of BHs with similar mass therefore the kick velocity is non negligible. Please amend the text.

Indeed the referee is correct - the gravitational recoil is dependent on the mass ratio. We thank the referee for pointing this out. In our case we consider the merger of two black holes with equal masses and hence a q value of 1.0. We also assume that both black holes have zero spin. In this case the gravitational recoil is not necessarily going to result in the expulsion of the BH from the host halo. We do not account for the case where the black holes are spinning and/or the spins are misaligned but nonetheless for the high-mass ratios examined here the approach is consistent with Holley-Bockelmann et al. (2008)). It is certainly true however that the merger remnant in these embryonic galaxies is very likely to be ejected from the halo given the low escape velocity compared to typical recoil kick velocity of hundreds of km/s for even small deviations from q = 1. Having said that whether the black hole is ejected or not from the halo is not directly relevant to the discussion in our paper which focuses on whether the event would be detectable by LISA. The implications of recoil however would be extremely relevant for characterising future MBH occupation fractions.

6. —— *We have now updated the text here to clarify that the model we describe here is the merger of two 5000 Msolar mass black holes at z = 15 which gives a SNR of ~ 32. This is consistent with what others find and in fact is derived using the equations (and python code) available from Robson et al. (2019). The referee is indeed correct that the higher SNR values are only found in approximately the last day prior to the merger but even in this last dive such a binary will complete a few hundred orbits. We have modified the text accordingly.*

The text remains confusing: "The Signal-to-Noise ratio (SNR) from the merger of two black holes with masses of 5000 M at z = 15 is approximately 32 and hence well within LISA's detection parameters. The merger of a 10,000 $M_\odot$ binary black hole with a mass ratio of 1 at a redshift of 15 will enter the LISA band approximately 2 months before merger and complete thousands of orbits before the final plunge. With this number of cycles LISA will be able to detect the redshifted mass with a precision of close to 1% (Sesana 2013), with the strongest signal and hence the highest SNR being achieved in the final few hours and hundreds of orbits.

“ One thing is to enter the LISA band, i.e., have the right frequency, another is to enter the LISA band with a sufficiently high SNR. My understanding from the written text is that 32 is the SNR at merger, but what is the SNR 2 months before merger? What makes the text also confusing is that, as I noted in my previous report, the mass estimate can be obtained from the information at or close to merger and not from the early low SNR cycles. Note also that if BHs are less massive, then they’d merge outside the LISA band (this can be easily seen from your Fig. 12), but they should be within the reach of 3G (3rd generation ground-based) detectors such as the Einstein Telescope. It would be worth noting this since most of the stars/BHs in your simulations have masses below 5000 solar masses.

For an equal mass binary at high-z as described here where the frequency evolves considerably over the course of the LISA mission time the SNR can be calculated by integration over the strain tracks (Robson et al. 2019). We can therefore estimate the SNR over different intervals.

The SNR ratio approximately two months before merger adds up to approximately 0.5, by approximately 1 day before merger the SNR has increased to approximately 5, 1 hour before merger the SNR is up to 27 and reaches an SNR ~32 about 3 minutes before merger. We have updated the text to reflect that the SNR is calculated by summing over the frequency interval in which the binary overlaps the LISA band.

We also note that the merger of such BHs will also be within reach of 3rd generation GW observatories.

We include here a plot of the SNR Vs. Frequency for the referee.

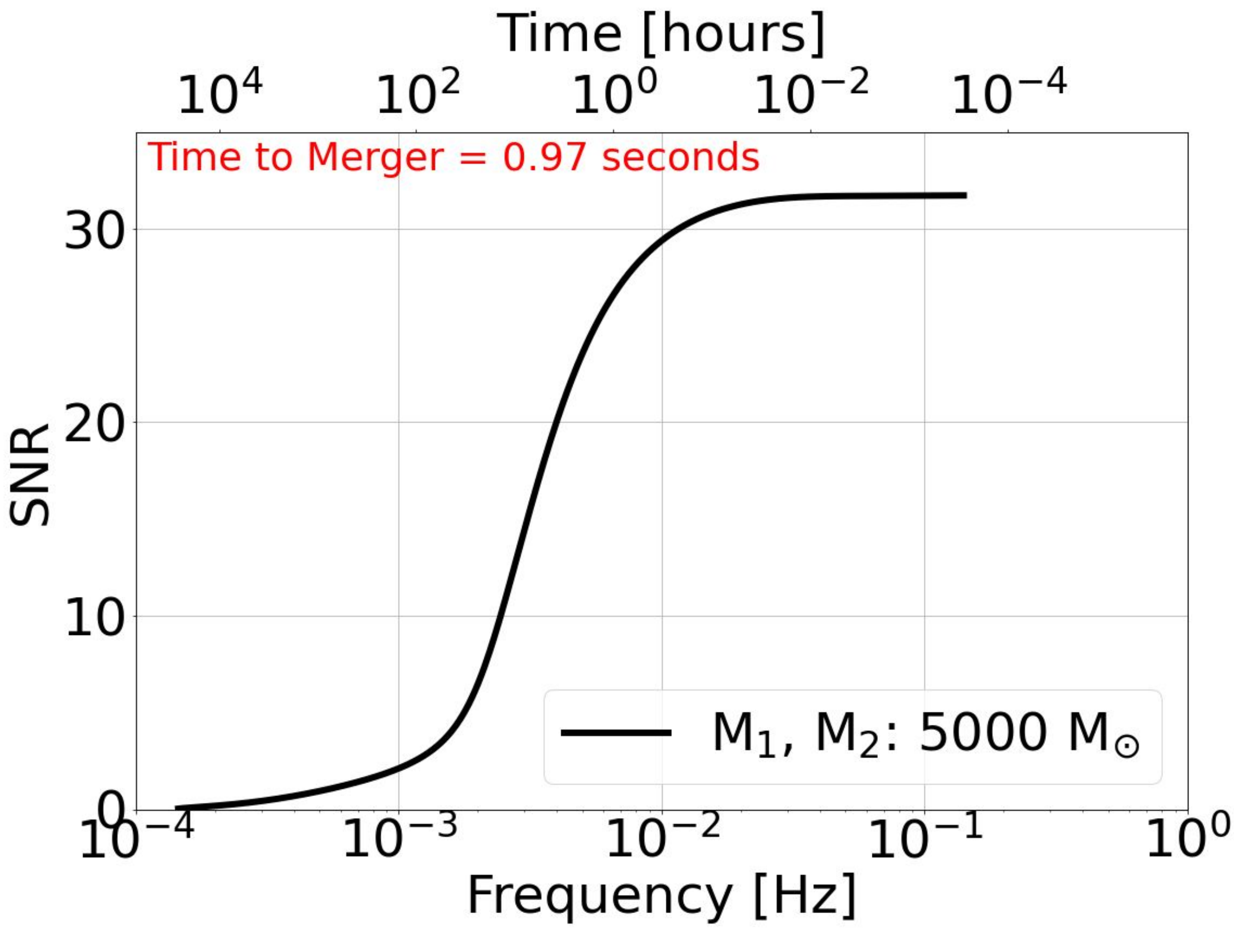

Figure Caption: The plot shows the SNR as a function of frequency. After the signal enters the LISA band the total SNR signal is calculated by integrating over the SNR track. The SNR is initially very low but increases above 10 approximately 12 hours before the final merger. What is shown here is the SNR up to approximately 1 second before merger. The bulk of the SNR is achieved between 12 hours and a few minutes before merger. Closer to the actual merger the LISA sensitivity actually decreases as the signal exits the so-called LISA “bucket”. Hence the SNR signals stops increases at frequencies above \nu ~ 1e-2.

– There are some minor typos, e.g., “Initally” quoted above. Since the journal does not do copy-editing, please take care with proofreading