Review of revised manuscript by Heinemann et al., for GMDD

I reviewed the manuscript previously and mainly commented on the appropriateness of the model’s response to changing dust fluxes, the description of the ballasting parameterisation, and the inclusion of sediments. The authors have in my view addressed many of these comments appropriately. However, the authors have not been able to directly address the issue of a lack of iron limitation as this very understandably requires a significant amount of additional work to reparameterise and spin-up the model. The authors have been upfront about this and have added a lot of clear discussion about this in the manuscript.

Additional comments:

The effect of glacial iron fluxes on diazotrophs occurs in the tropical Pacific which is separate from the ballasting effect which occurs mainly in the high latitudes. It's unfortunate that the authors cannot directly address this, but more context/discussion could be given for the current results about the diazotrophs in the model, e.g., has this been quantified for LGM dust fluxes before?

The authors have now demonstrated there aren’t any significant interactions between iron fertilisation and ballasting associated with the LGM dust forcing. But, any fertilisation effect in the Southern Ocean would be enhanced or diminished by the associated ballast effect which should be noted in the discussion. Additionally, any associated PIC:POC changes could be also different in the Southern Ocean.

The authors have added figures showing timeseries of organic carbon export, opal export and PIC:POC ratios. I am concerned that there are still significant trends in the ballasting simulations. The authors state that the reduced organic carbon export is due to nitrate depletion at the surface, which is fine, but what is causing this depletion? Figure 6 shows results from the first 100 years whilst Figure 5 shows deviations from these initial trends, which is somewhat confusing. I think this should at least be stated more clearly.