In this article, the authors investigate the influence of recycled oceanic crust properties (density excess and viscosity) on the preservation of lower Earth mantle primordial heterogeneities, consisting of bridgmanite material resulting from the crystallization of the Earth early magma ocean, and which are referred to as BEAMS. For this, they perform more than 20 simulations of thermo-chemical convection with three sorts of material, including basalts, harzburgite and primordial material (BEAMS), and varying both the viscosity and the density of basalts. After 4.5 Gyr, simulations may be sorted out in 4 regimes or sub-regimes depending on the evolution of the BEAMS and recycled basalts. These include full mixing of both basalts and BEAMS with harzburgite, mixing of BEAMS with the formations of basal piles of basalts, and preservation of BEAMS either with the formation of piles of basalts or with basaltic layering above the CMB. The authors note that the occurrence of each regime is controlled by the excess density of basalts and, to a lesser extent, by their viscosity. They conclude that dense recycled basalts is needed to preserve BEAMS over periods of time comparable to the age of the Earth, and that higher viscosity further helps this preservation.

This article is well written, and the research it present fits well the scope of *Solid Earth*. The simulations of convection are clearly described and are carefully performed with a state-of-the code. Results and interpretation are also clearly discussed and are supported by the authors simulations. I have only minor comments, mainly points of discussion on BEAMS, and I recommend this article for a publication in *Solid Earth* after some minor to moderate revisions. Below are some comments and suggestions that the authors may include in the revised version of their paper.

1. The authors neglect internal heating. This is fine, except that they probably underestimate the potential effect of internal heating, and that this aspect should deserve more discussion. Adding internal heating is likely to affect the balance between plumes rising from the CMB and descending slabs in the favor of the later. I guess that more vigorous slabs may have some impact on the preservation of BEAMS. Second, it is (as the authors pointed out) likely that heat producing elements will concentrate in basalts. This may in turn affect the evolution of basaltic piles or layering in a way similar to the impact of excess heating in primordial material (as recently investigated by Guerrero et al., 2024). Finally, adding internal heating is important for the mantle heat budget and in particular to get CMB heat flux within the expected range of values. Again, no additional simulation is needed here, but an extended discussion on that topic would be welcome.

2. To separate the different regimes, the authors use threshold values of the local fractions in primordial material and basalts, and in the fraction of CMB area covered by basalts. Are they any specific reasons for fixing these parameters to the values chosen by the authors (if yes, explain), or are these values mostly arbitrary ?

3. I find the discussion on the characteristic viscosity for each regime (page 10) and the corresponding figure 4 not very convincing. More precisely, when accounting for the variability of viscosity within each regime, the difference between two different regimes sounds less

significant. I agree that there is a general trend (models that preserve BEAMS have on average higher viscosity), but the most viscous model in MP have nearly the same viscosity than the less viscous model in BL, so I'm not sure that these profiles are a key aspect of the authors results. Also, MP and MO regimes have less simulations than BL, and I guess than running more models would enlarge the range of viscosity. Finally, it would be interesting to show (or say) which model correspond to the upper and lower bounds of each radial viscosity profiles in figure 4.

4. Line 286. q_{bot} (bottom heat flux, I guess), has not yet been defined in the paper. It might also be interesting to plot the evolution of q_{bot} and q_{top} with time.

5. The authors point out that the basal basaltic layer is animated by convection. I'm not sure this is the case. The fact that the radial velocity is not zero and vary laterally within these structures does not guarantee that convection operates in them. First the authors should compare these velocities with those in the mantle. Second, and more importantly, if the basal layer is animated by convection, the temperature profile within this layer should consist of a thermal boundary layer at both the top and the bottom and an adiabatic region in between. If the temperature profile is linear, then the layer is not animated by convection, and heat is transported by conduction. Same thing for the piles of basalts. For similar reasons, on line 345, I would change 'reduces convective vigor within the piles' to 'reduces heat transfer within the piles'.

6. I'm less optimistic than the authors regarding the seismic signatures of BEAMS. First, BEAMS are large structures, so I guess they would be resolved by the most recent tomographic maps, especially those based on waveform inversion. Second, while excess bridgmanite have no or very slight effect on shear-velocity, the BEAMS enrichment in iron (~ 2-3%) should produce a relatively strong signature (namely, a substantially reduced shear-wave velocity), enhanced by the fact that BEAMS are slightly hotter than average mantle. Overall, I do not see any reasons why BEAMS shouldn't be detected by available tomographic models.