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Interactive comment on "On the self-regulating effect of grain size evolution in mantle convection models: Application to thermo-chemical piles" *by* Jana Schierjott et al.

Anonymous Referee #2

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This manuscript presents the results of 2D simulations in spherical geometry investigating the effect of grain size evolution, with application to thermo-chemical piles. Grain size is important because it affects viscosity, and it modifies the effective temperature-dependence of the viscosity. Modeling grain size is challenging, because grain size depends in a complex way on several parameters, such as stresses, phase transitions, temperature and composition. The manuscript is worth publication, after revision.

The manuscript reads as a diligent description of model results, but, all in all, it seems a bit pedantic. In particular, the connection between lithospheric processes and the deep-seated thermo-chemical piles remains unclear. For example, in line 337 we read

that "the pile-temperature mostly depends on the eruption efficiency", but it is never explained how the eruption efficiency (i.e., the percentage of basalts erupted at the surface or intruded as gabbros) can effect the temperature of thermo-chemical piles at the base of the Earth's mantle. Moreover, the reader never understands the internal dynamics of the pile (velocity field, internal convection, mixing with subducted material ecc). It is also impossible to understand how the authors obtain (50%!) melting at the base of the mantle, nor how melting would affect viscosity or grain size. In other words, if the focus of the manuscript are the piles, then the authors should be more specific and quantitative.

The novelty of the simulations resides in the composite rheology and in the fact that viscosity is grain size-dependent. This aspect should be presented more clearly, already in the introduction, where the reader expects to find a pedagogic and insightful presentation of diffusion and dislocation creep (you do it in paragraph 2.3, lines 155, but I think it comes too late). The paragraph you have in the introduction (starting at line 68) is too technical (for example your sentence "grain growth when conditions favor high grain boundary energy" needs to be better explained). I also suggest to expand the few lines describing diffusion-dislocations creep in the mantle (for example, your sentence "However, several other studies indicate that in many regions dislocation creep is active" is too dry and we do not learn much, nor do we gain insight to compare previous studies to your new results). In the introduction we should also talk about seismic anisotropy.

In the following I give my comments (in a line by line order). Line 31: it is the opposite!! Pacific LLSVP is roundish. African LLSVP is elongated. Lines 40-44: it would improve by being more specific (i.e., quantify density differences, and how they vary with depth). Line 50: I would add a citation : U. Christensen, A.W. Hofmann, Segregation of subducted oceanic crust in the convecting mantle, J. Geophys. Res. 99 (1994) 19867-19884. Line 53: I find this sentence useless ("Since LLSVPs remain physically unreachable numerical and experimental studies try to constrain the parameter space"). Line 62: Here you should say more, and your sentence " Only very few studies have considered a composite and grain size-dependent viscosity [ref]" is unsatisfactory. At this point the reader needs to understand: (1) what previous authors have done and found, (2) what is new in your work with respect to what has been already published. Line 74: Your sentence " Among others, Cordier et al. (2004) suggested...." skips to cite previous papers before Cordier et al. (2004). I do not recommend this practice. Line 82: Your sentence "By also considering a primordial layer we are able to elaborate on the origin of LLSVPs" (e.g. subducted basalt, primordial reservoir or the basal mélange (Tackley, 2012))" does not seem true to me, since (1) you are never specific about the composition and internal dynamics of the pile, (2) you do not span a range of buoyancy ratio B. Line 85: Your sentence " We investigate whether piles behave as obstacles to convection, whether they get pushed around or even entrained by mantle flow" also does not seem true to me, since you never quantify entrainment, you only say that they are pushed around, but this is well known. Line 98: Your sentence "If the melt is generated at a depth lower or equal to 300km, the basalt is..." seems incorrect. If partial melting occurs at 300km depth the liquid composition cannot possibly be a basalt (already at 100km depth the melt has a picritic composition). I suggest to add a citation to strengthen your statement. Line 101: Your sentence "Intruda is therefore warmer than the ambient lithosphere which results in lithosphere-weakening" needs to be explained, namely for the "lithosphere-weakening" part. What are the modeled melting rates? Over which length-scales do you intrude the lithosphere? Over which time-scales do the intrusions cool? How correctly can you solve for lithospheric processes knowing that your grid resolution is quite poor (512 elements for 360° means that at lithospheric depths your element size is 78km). Do you consider latent heat of melting? Line 120: Your definition of the Buoyancy number is confusing/wrong: (1) The numerator is a density difference (RHOprimordial - RHOsurrounding mantle). Why do you use (RHOprimordial - RHObasalt), your mantle is NOT a basalt, it is 80% harzburgite and 20% basalt. (2) The denominator is also problematic, since RHO0 is NOT the average of RHOprimordial and RHObasalt, but it must be the RHO entering in the

Rayleigh number (never given in the tables). Line 124-125: The simple statement " we vary the intensity of dynamic recrystallization" needs to be explained. What is the physics behind? What does this mean? Line 141: Rewrite eq.(2) Line 148: Rewrite the last term of eq. (4). In the equation you have an internal heating term, but we never find the value of H. Line 201: The definition of full mechanical work is wired, I guess a typing problem. (Check also eq. 15 and 16). Line 207: Here we find Tcmb=4000K, whereas in Table 1 Tcmb=5000K. Why? More generally, your ftop, fbot, and the physics behind eq. (14) are unclear. Line 220: Your criteria to detect the pile (>90% of primordial + basalt) makes it impossible to detect entrainment of surrounding mantle into the pile. For example, if you have (80% primordial + 20% surrounding mantle) how is this considered? Normal mantle ? Line 225: I do not understand eq. 18: Tpile > (3000K + Tcmb). In table 1 Tcmb=5000K, so how can Tpile be greater than (3000+Tcmb)? It means Tpile=8000K, which is impossible. Line 244 and Table 3, Table 4: Warning !! In the text (line 244) we read that for density 3140 kg/m3 the ratio B=0.14, BUT, reading the figure caption of Table 3 we find that when the density is 3140 kg/m3 the ratio B=0.24. Line 247 and elsewhere: The ratio of strain rate due to dislocation creep and strain rate due to diffusion creep is defined as "rheology" and throughout the rest of the manuscript "rheology" has this meaning. I think this is very confusing and I invite you to call it "the rheology ratio" but not "the rheology". Line 248 and Figure 3: Warning, two panels are never mentioned, neither in the text nor in the figure caption. I'm talking about the two panels at the left. What are the green lines? (In line 287 you say something about figure 3, while presenting figure 4, and also in line 461.... well, all this is poorly organized). Line 250 and Figure 3: (1) it is very hard to detect the white and the black lines. (2) partial melt higher than 50% ! This is very high, but in the text you never talk about partial melting, you never provide the solidus used..... (3) In the lower mantle it is incorrect to talk about basalt, you should use "basaltic composition". (4) It 's impossible to see that "basalt is pushed aside". You need to have a figure with a zoom on the region of interest. Line 261: I do not understand what do you mean by "the newly formed parts of the pile". Since the pile does not entrain, but it is merely displaced, how do you generate ”newly formed parts of the pile” ?? Line 268: Provide the solidus used to calculate melting at the CMB. Line 270: It is wired: first we read that ”basaltic material ” melts up to 50%, and then we read ”once the basaltic material has warmed up”. How is this possible? Your statements are neither quantified nor justified. Show a P-T diagram with real temperatures and the used solidus for each composition, and then the reader will understand. Line 293, line 295, and Figure 4: I do not understand why the pile density varies. Line 301 and Figure 4: The modeled surface velocities can be higher than 10e3 cm/yr and up to 10e4 cm/yr, these values are huge (10-100 m/yr!!) and deserve a comment. Only saying ”a lot of cold lithosphere simultaneously moves down” is insufficient. You need to quantify subducted volumes and you need to convince the reader that surface velocities at 10m/yr are not an artifact of the numerical simulation. Line 310: I do not understand why the density of the pile changes because of ”relocation” of pile material”. Density variations caused by pressure variations are not an intrinsic density change, they are just an effect of compression/decompression. Lines 439 to 443: Rewrite. Line 470: Provide reference of articles suggesting that piles ”spatially determine subduction zones”. Line 509: Why is pile density self-regulating ??

Final comment: once you have reviewed the manuscript I suggest to rewrite parts of the abstract in a more concise, punchy, way.

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