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Solid Earth
Discussions

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.

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1 Response to Reviewer #2: Anonymous reviewer

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

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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.
We have added an explanation on this to the manuscript. In fact this is surprisingly simple: when most of the melt is erupted, the lithosphere is thick and therefore cools the LLSVPs very well when it reaches the CMB. When most of the melt is intruded, the lithosphere is thin and tends to drip down instead of exhibiting large-scale resurfacing events. Anyhow, the focus of the paper is not the link between LLSVPs and lithospheric processes. The change of eruption efficiency only arose because we aimed for Earth-like convection regimes and eruption efficiency is a potential way to receive it in whole-Earth geodynamic models.

Moreover, the reader never understands the internal dynamics of the pile (velocity field, internal convection, mixing with subducted material ecc).
Indeed this is a disappointing problem also for us. We cannot really see the internal dynamics of LLSVPs as we are computing long term mantle dynamics. The resolution is rather low so we chose to only look into pile averages to try to report a result as robust as possible. Increasing the resolution is very difficult as the simulations already took a very long time to run. We re-wrote the focus and goals of the paper slightly to make it easier to grasp that not the internal behavior of the piles is the focus but their interaction with the mantle and their properties. We didn’t observe any mixing, wherefore we did not focus our paper around this topic. It may have been worth to investigate further in which cases mixing would have been observed, but this would have meant a different scope of the paper and a different parameter study. Although

we state in a few sentence that there is no mixing, we will remove it because we do not provide a detailed study on this topic.

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.
We now provide much more information on melting and crust production in the text.
In other words, if the focus of the manuscript are the piles, then the authors should be more specific and quantitative.
We add statements in the article to explain that we are looking at the big picture instead of details as we run long term simulations with limited resolution. We try to state more clearly now that the goal of the paper is to demonstrate the general behavior and evolution of LLSVPs and their influence on the overall dynamics of the Earth's mantle instead of detailed internal convection or small-scale mixing. We try to provide numbers and be quantitative, but specific numbers are difficult to provide since grain size evolution parameters themselves are highly uncertain. Therefore, we provide averages of pile properties which is already more advanced and quantitative than other 'pile-paper'.

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.
We have added a paragraph describing previous whole-Earth studies that use grain size in some sort or another in their rheology definition.

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.
Yes. This was a typo that we have now corrected.

Lines 40-44: it would improve by being more specific (i.e., quantify density differences, and how they vary with depth).
We have added the estimated density difference in the text.

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.
We have added it.

Line 53: I find this sentence useless ("Since LLSVPs remain physically unreachable numerical and experimental studies try to constrain the parameter space").
We have removed it.

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.
Yes. We have add much more detail on this, as suggested.

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.
We have added the earliest citation (to our knowledge). There are not that many actually.

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.
True, we indeed did not mention the mixing of basalt and pile material and entrainment of pile material in the ambient mantle a lot. We have therefore removed this and stated the other points of our paper more clearly, as you suggest below.

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.
We observe that stresses and strain rates propagate through the pile, therefore they are certainly involved in the global deformation. We also actually see the piles moving with the flow. Indeed we do not quantify entrainment but the fact the piles are pushed around is clear in our results. Actually a lot of people believe that piles are fixed, following what the people from CEED are claiming. This is the reason why we have written this statement here.

Line 98: Your sentence ”If the melt is generated at a depth lower or equal to 300 km, the basalt is...” seems incorrect. If partial melting occurs at 300 km depth the

liquid composition cannot possibly be a basalt (already at 100 km depth the melt has a picritic composition). I suggest to add a citation to strengthen your statement.
Apologies, this is one of our common mistake, we mistake eclogitic melt for basaltic melt as the eclogite becomes basalt at the surface in our code. We correct this in the manuscript.

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 degrees means that at lithospheric depths your element size is 78km). Do you consider latent heat of melting?
We did not detail this much as this paper is not focusing on melting and crust production. All of these questions are answered in a manuscript of Diogo Lourenço (and A. Rozel and P. Tackley) that is still in its last round of review (now minor revisions), and also in the doctoral thesis of Diogo Lourenço (online on ETH's web site). We answer your questions by adding clarifications in the text.

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).
We have removed this part because the buoyancy number in any case does not play a significant role in our paper since we do not investigate a vast parameter space of the primordial material.

Line 124-125: The simple statement ” we vary the intensity of dynamic recrystallisation” needs to be explained. What is the physics behind? What does this mean?
We have clarified this in the text.

Line 141: Rewrite eq.(2)
We have corrected this typo.

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).
We now detail in the manuscript what tensor contraction is (this is a bit unusual indeed). We have also added the original radiogenic power H_0, the decay half life and the partitioning coefficient of heat sources during melting in table 1.

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.
We now distinguish the values mentioned in table 1 and in the text.

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 ?
We define that the piles' composition must be primordial material but can include some basaltic composition and even up to 10% of ambient mantle. Every cell that contains some percentage of primordial material is considered pile for sure. But it can also only contain 30% of primordial material and 60% of basalt and 10% of ambient mantle and will still be considered pile.

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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.
Sorry, this is a mistake. It is meant to be divided by 2.

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.
We have removed the buoyancy ratio from the manuscript.

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”.
Sorry, but we would rather not change this expression. Since we define "rheology" in the beginning and on every figure it should be clear to the reader. Furthermore, since rheology is the study of deformation of material we do not see a problem with calling the dominant deformation mechanism "rheology". We also always explicitly state which deformation is dominant.

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).
we now mention the panels explicitly. The organisation follows the convection regimes. Several figures illustrate different aspects of each convection regime. This is the reason why we cite 2 figures together.

Line 250 and Figure 3:

(1) it is very hard to detect the white and the black lines.
We first plotted these figures with thicker lines but it becomes harder to see the pile fields (too strongly overlapped with the 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.....
We now provide the solidus temperature function. Yes indeed, high melt fractions can seem unrealistic if we compare to the present day Earth, except that Earth's estimated current CMB temperature is close to the mantle solidus. Yet our simulations can significantly deviate from present-day Earth's conditions. In the early stages of the simulations particularly, it is not rare to get melting in the lower mantle, and this may be realistic for the early Earth as people are now studying a long-lived basal magma ocean for example. Moreover, when the stagnant lid regime is reached for a long time, the mantle can be strongly insulated from the surface and sometimes warms up substantially. Since our simulations follow a very self-consistent design (nothing forces the evolution of the internal temperature), we have little control on what happens in the models, which explains why we chose to report observations in the present paper instead of attempting scaling laws of internal quantities. Certainly one can also use different solidus temperatures and also add the influence of water (a complete different problem) but of course this is not the point of this paper focused on the grain size evolution problem.
(3) In the lower mantle it is incorrect to talk about basalt, you should use "basaltic composition".
Yes sorry about that. This is unfortunately a common mistake that we do in our team as the "composition" is either "basalt" or "harzburgite" in the code. So we end up writing this in article. We have correct it.
(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.
We add a comment on this in the text. Unfortunately we cannot load this figure even more.

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” ??
We have removed the density field plot from our paper since it does not provide significant interesting information as we do not focus our paper on the interaction of pile material with downgoing eclogitic material. Anyhow, due to the definition of "pile" parts of downgoing eclogite can become pile. Even though we do not observe large entrainment, it can be that small parts get entrained.

Line 268: Provide the solidus used to calculate melting at the CMB.
We now give the solidus in the text.

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.
Yes, apologies, our observation was just wrong. In fact the material that melts was present before, close to solidus temperature and was decompressed by the return flow of the downwelling. Indeed the downwelling is cold and therefore is not melting. We have simplified the text as this was not really helping to make our point in the manuscript. This was indeed very confusing.

Line 293, line 295, and Figure 4: I do not understand why the pile density varies.
We have removed the text and the figure about density. This was not really helping to make any important point. To answer you: the density was varying because of both temperature and pressure changes. This happens because we do compressible convection. Plotting density was a little misleading because these adiabatic density

changes do not drive convection.

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.
We comment in the text. Yes these velocities are large but the load is much larger than present-day Earth’s load. A 300km lithosphere destabilising as one plate would generate very large stresses. With a non-Newtonian (stress-dependent) rheology, such velocities make sense.

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.
Indeed. Yes, this is also what we answered above. We have removed this confusing observation.

Lines 439 to 443: Rewrite.
It is rewritten.

Line 470: Provide reference of articles suggesting that piles ”spatially determine subduction zones”.
We have edited the paragraph to:
Our thermo-chemical piles are also not surrounded by plume generation zones (PGZ), as suggested by Burke et al. (2008), but plumes rise directly from the piles as well as from their margins. They, as others (Torsvik et al. (2006), Torsvik et al. (2010)), conclude that LLVPs (in geodynamics referred to as thermo-chemical piles) have been stable in time because the downward projection of Large Igneous Province

(LIP) sites can be linked to the margins of LLSVPs after rotating them back to their original eruption sites. LIPs in the 200 and 500 Myr age range let them conclude that LLSVPs have been occupying the same location for the same duration. Stable piles can only be confirmed with our models in case of absence of strong downwellings (subduction zones), hence for the last 200 to 500 Myr because we observe that downwellings govern the piles' spatial distribution. If there are no strong downwelling events disturbing the location of the piles, we can observe piles stable for at least 300 Myr. However, without dominant downwellings, we do not see plate tectonic-like behaviour in our simulations, implying that we either observe stable piles or plate tectonic-like behaviour, but not both simultaneously. Even without a plate tectonic-like convection regime in our models, it is difficult to draw conclusions about the actual stability and spatial distribution of LLSVPs. Problematic is that we neither employ realistic plate velocities, nor use three-dimensional models.

Line 509: Why is pile density self-regulating??
We have removed this.

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