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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.

Jana Schierjott et al.

jana.schierjott@erdw.ethz.ch

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1 Response to Reviewer #1: Bradford Foley

Review of "On the self-regulating effect of grain size evolution in mantle convection models: Application to thermo-chemical piles" by Schierjott, Rozel, and Tackley

General comments:

This paper presents 2-D numerical convection models that include grain size evolution, to model the long term evolution of thermochemical piles at the base of Earth's mantle. In particular, the paper focuses on the effects of a composite rheology that includes dislocation and diffusion creep as well as a formulation for grain size evolution, to assess how grain size evolution influences the dynamics of the piles. The main findings are that grain size in the piles is relatively self-regulating, following a long-term trend as a result of mantle cooling and changes in the typical stress strain rate within the piles. Large episodic overturns lead to significant decreases in pile grain size and viscosity, but grain size quickly returns to the previous state once the overturn is over. Another important finding is that although warm temperatures in the piles lead to grain growth, this grain growth is limited by the background rate of deformational work in the piles, such that piles do not become very stiff and resistant to being pushed around the CMB by subducting slabs. I find the findings to be interesting and worthy of publication, and the science overall is sound. I do think some moderate revision is needed to more clearly highlight and demonstrate the main scientific findings, and address a few minor technical issues as well.

Specific comments:

1. This paper could be significantly improved by more clearly organizing it around central scientific questions being answered or hypotheses being tested. As of now it reads like more of a description of model results, without much direction beyond "what happens when we include grain size evolution." I have a couple suggestions for this:
A) Whether pile grain size can increase and allow the piles to become rheologically stiff, and therefore anchored at the CMB, is an interesting question, and could be looked into more thoroughly. The paper indicates that this is not the case, as the pile grain growth is limited and downwellings impacting the piles cause the piles to be rheologically weakened. This raises some questions that could be explored in more detail: What is it that prevents the piles from stiffening? Is there internal convection that supplies enough deformational work to keep grain size from growing too much?

Is it downwellings hitting the piles that cause the stress/deformational work that keeps grain size from growing drastically? Likewise, during major overturns where there is significant weakening and grain size reduction of the piles, it would be useful to show the rate of deformational work in this instance.
¶Indeed we had a hard time deciding how to present the results of our study. We first formulated several scientific questions but obtained a very complicated structure with redundancies. In the end we decided to first offer a global presentation of the fields, followed by 0D averages for each convection regime, 1D profiles ordered by convection regimes, and only then attempt to answer scientific questions.
¶Thus, we do not think that, at this point, changing the structure of the paper through minor revisions would help in clarifying scientific questions. However, we do answer your points in the Discussion section, where clarifications fit well into the design of the paper.
¶In short, we answer the following scientific questions (also in the paper):

- Ambient mantle mechanical conditions (stress and strain rate) reach and propagate through the thermo-mechanical piles. In other words, we find that the piles are not mechanically decoupled from the mantle. Therefore, the idea that the piles can be much stronger than the mantle is not supported by our results. This regime might exist, we just did not observe it in our simulation using experimental (reasonable) coefficients. Moreover, this means that the viscosity of the piles does change with the convection regime as stress and strain rates vary.
- Yes convection stresses keep the grains from growing too large. This is shown in figure 3 (equilibrium grain size vs time), Eq. 16 and discussed in section 3.4. More precisely, mechanical work, as you say, controls the grain size.
- both downwelling and upwellings generally contribute to the ambient mechanical work. It would be very hard to know exactly if downwellings or upwellings dominate the ambient mechanical conditions but we observe that downwellings are important. This can be explained by the fact that the bottom boundary layer is not potentially unstable like the lithosphere is in the episodic regime. Solomatov (2004) does attempt to answer the question of partitioning of stress contributions between upwellings and downwellings (as you know), but his study was performed in a very simplified framework, which might not fully apply in our case.
- Unfortunately, at this stage, we cannot plot the mechanical work itself without quite some programming (or rerunning all cases). However, one can have an idea of what the mechanical work would be by multiplying the stress and strain rate invariants. Figure 3 shows that both those fields are relatively homogeneous around large structures (either whole mantle or around a large downwelling during an overturn) so the mechanical work is very likely to also be rather homogeneous.

B) The fast pile grain size "recovery" is also interesting. How about using the model results to compare the recovery timescale seen from the numerical models to the theoretical prediction for recovery time, to demonstrate that the expected recover time scale indeed holds? Also, the authors should be able to work out what is stabilizing grain size and viscosity as the mantle cools (in particular for the cases shown in the appendix). There must be some trend in grain size (or viscosity) acting coupled to the change in pile temperature to keep grain size nearly constant over time. Finally, another interesting point is that grain size variations limit lateral viscosity variations; e.g. plumes have a similar viscosity to the surrounding mantle because the higher temperature is cancelled out by larger grain size. The authors could look into what conditions allow this to hold. For example, if the grain growth activation energy is much larger than the activation energy for diffusion creep, would plumes become more viscous than surrounding mantle? Or would deformation still limit the grain size?

¶These questions are indeed very important from a fundamental point of view. Some of them are answered in another article in preparation, which should have been

published before the present manuscript but technical difficulties made it impossible to finish as it explores a much larger parameter space and answers theoretical questions. Still we can partially answer your requests:

- The recovery time scale is a very parameter-dependent quantity. We chose to mention its existence in our discussion but we do not to claim that all parameters leading to its estimation are known in a robust way. We rather give an estimation and do not attempt more. We think the idea that stresses penetrating through piles might hold for a large range of rheological and mineralogical parameters but the grain size itself in the pile is hard to really assess. Since the petrological nature of the LLSVPs is highly uncertain, we chose not to provide a prediction, only an estimation.
- Yes we did want to mention the competition between temperature and grain size. We have a dedicated paragraph on this topic (section 3.4). The paper in preparation will be able to answer more on this idea that the difference of activation energies of growth and rheology will dominate (and even potentially invert) the temperature-dependence of the rheology. Since this idea has been proposed in the past (Solomatov and Korenaga do mention this) we did not detail it too much in the present paper. Overall, still our observation that stress does propagate through the LLSVPs seems to indicate that stresses would also make it through viscous plumes. We observe that mechanical quantities tend to homogenise in the mantle and through whichever anomaly.

2. Throughout this paper, the authors should be looking at the deformational work rate, not just stress. Work rate is what is controlling grain size reduction, and therefore the most relevant thing for the typical grain size in the piles and amount of grain size reduction seen when downwellings interact with the piles.

¶We added to Figure 4 a plot of the average work rate occurring in the pile (replacing the plot of average density). From this plot we can see that when stress is high the work rate is also high. Hence, our interpretation does not change. In any case, we agree, the work rate is better and now our paper has a much stronger argument than before.

3. The authors should discuss whether the resetting of grain size at the post-perovskite phase change has any significant effect on the results, in particular for grain size evolution in the piles.
¶The influence of the post-perovskite phase change is negligible because grains grow back very fast in any case due to a low deformational work rate and high temperatures close to the CMB. ¶Moreover, the radial velocities are usually small so a very limited volume of material goes through the Post-Perovskite phase transition. We have added comments on this in the text.

4. The results indicate diffusion creep generally dominates in the piles themselves, and dislocation creep can be active around downwellings or other high stress regions at the CMB. Given that we have observations of seismic anisotropy in some regions near the core-mantle boundary, the authors could do a more thorough comparison of their results to these observations. Comparing the settings where anisotropy is observed to where the models predict dislocation creep to be active would provide a good test to the model results.

¶We have edited the paragraph and added some details:
The anisotropy observed in some parts of the D”-layer (Lay and Young, 1991; Lay et al., 1998; Garnero, 2000; Kendall and Silver,1996), specifically in regions of high stress (Karato, 1998), can be explained by regionally occurring dislocation creep due to downwelling-induced high stresses as has been proposed by (Karato, 1998). Seismic anisotropy resulting from dislocation creep in the rest of the D”-layer can better

be explained by material layering, aligned inclusions or flow fabrics due to a strongly sheared thermal boundary layer and crystalline alignment as has been suggested by for example Kendall and Silver (1996) and Doornbos et al. (1986), respectively.

5. Equation 7: What is the purpose of the "dislocation creep efficiency" parameter? A composite rheology formulation should be able to deal with this self-consistently, and have the temp, grain size, stress, pressure, etc dictate which mechanism dominates and controls the viscosity entirely on its own.

¶Sorry, we have reformulated the text to explain this better. The rheological coefficients used in η_{df} and η_{ds} would independently lead to the viscosity profile of the Earth for both diffusion and dislocation creep if the global stress and strain rate of the Earth occurred (e.g., in case of plate tectonic). So if we solely used diffusion creep or solely dislocation creep, we would probably obtain the viscosity profile of the Earth. However, this is not what we want here. We rather want to have diffusion creep dominating in the lower mantle and dislocation creep dominating in the upper mantle. The dislocation creep efficiency is a number we have defined to favour diffusion or dislocation independently in the upper and lower mantle. This does not mean that the rheology is forced at all times. The rheology (effective dislocation creep/diffusion creep fraction) still depends on stress, grain size, pressure, etc., is time-dependent and depends on the self regulating processes happening during convection. But if plate tectonics occurs, then the effective rheology will be the one predicted by the dislocation creep efficiency.

6. Below equation 14: ": : :where TCMB = 4000 K is the average temperature at the core-mantle boundary, ftop is the maximum (at 3000 K) and fbot the minimum damage fraction (at 4000 K). In order to set the damage fraction to zero at surface temperatures of 300 K, the term in (14) uses -300 in the exponent." Something's off here. By equation 14, f doesn't go to 0 at the surface, it just goes to ftop (the exponent goes to 0). Also ftop is the maximum at 300 K not 3000 K.

¶Yes indeed the text was wrong. The equation is correct. We have changed the text to:
where T_{CMB} = 4000 K is the average temperature at the core-mantle boundary, f_{top} is the maximum (at 300 K), and f_{bot} the minimum damage fraction (at 4000 K).

7. The calculation for the pile grain size recovery time for the Earth uses the typical stress and strain rate in the ambient mantle to calculate the deformational work rate. But stress and strain rate in the piles could be different. Better to analyze the flow patterns in the piles that determine the typical work rate in these regions, as I've suggested above, and use this in the estimate for the modern Earth.

¶If one thinks that stress and strain rate are different inside and outside the piles, then indeed using global mantle flow kinetics to estimate pile conditions would not be meaningful concerning the piles. However, our plots of the 1D profiles inside and outside the pile indicate that the viscosity is similar in the pile and in the surrounding mantle. In such case, the ambient flow should be a good indication of the pile conditions.
¶We were first aiming at an article in which numerical simulations would be carefully compared to Earth observations. However since grain size evolution makes it hard to obtain the mobile-lid regime, we did not obtain a large set of simulations with a behavior comparable to that of the Earth. Nevertheless, we were surprised about the self-regulating behavior of the pile for each convection regime so we decided to write the present paper. However, we do not believe our study is general enough to make an actual comparison with the Earth, we would rather simply provide estimates.

Technical corrections:

Lines 42-43: I just don't follow what this sentence is trying to say
¶We have changed the sentence to
"By analysing deep mantle-sensitive Stoneley mode data in a joint P- and S-wave inversion this recent work showed that at least the upper parts of LLSVPs might be lighter than the ambient mantle."

Line 101: "Intruda" likely a typo
¶We changed it to "Intruded material is

Line 219: I think it is better to refer to this as a wattmeter since it is deformational work driving grain size reduction and not just the stress
¶We removed piezometer.

Lines 252-253: Are the small grain sizes of 5 microns seen everywhere in the lithosphere or just at plate boundary areas?
¶They are mainly that small in areas of plate boundaries. In the rest of the lithosphere they can be large as 100 μm. We added " Small grains (around 5 μm in plate boundary areas and up to 100 μm elsewhere)...".

Line 292: "This prevents the Earth to cool down more" should say prevents the Earth from cooling down more
¶We have changed the wording to the suggested phrase.

Line 296-298: How is the second stagnant lid phase defined as stagnant lid, if surface velocities are nearly as high as in the mobile lid phase?
¶The stagnant lid phase is defined to be when the average surface velocity is less than 1cm/yr. Although the surface velocity is close to this threshold in the second stagnant lid phase, the simulations don't show rapid overturns or subduction events so it can be classified as stagnant lid. After 4.3 Gyr there is some mobile component. We distinguish this now in the text:
"During the second stagnant lid phase (3.5-4.3 Gyr) . . . . [. . .] The pile temperature can further decrease during the second stagnant lid phase because there still exists some movement at the surface, manifested by dripping of lithosphere."

Line 324: "Vigorousness" should be "vigor"
¶changed to vigor

Line 406: Here is a place where the authors could look into more detail at stress and strain rate in the piles, and what sets the typical level of deformational work in the piles and hence limits grain growth

¶We now plot the mechanical work rate in the pile as a function of time.

Line 480: Saying that the models can and cannot confirm the idea that plumes form at the pile edges is very confusing. If the results don't confirm this idea then they don't confirm it! Please clarify the text here.
¶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 to 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 the case of the 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.

Lines 492-493: Larger grain sizes in the plumes not affecting the viscosity: Does this mean that the viscosity is not sensitive to grain size, or that the grain size just isn't growing all that big? Confusing as written. As I suggest earlier, this issue of temperature vs. grain size tradeoffs for viscosity is something that should be looked at in more detail.
¶We have edited this part to:
"Our results show that grain size has a great impact on the viscosity in numerical convection models. Similar to results by Dannberg et al. (2017), we observe strong lateral variations in grain size and resulting viscosity in our simulations, particularly during resurfacings or prominent downwellings. Overturn events lead to a distinct 'bimodal' behaviour in which one half of the spherical annulus shows a distinct decrease in viscosity and smaller grain size than the other half (figure 3, 1.58 Gyr). Downgoing slabs are surrounded by regions with lower grain size, high strain rate and reduced viscosity. This finding agrees well with what Dannberg et al., (2017) reported. However, in times without any particular downwelling event we do not observe strong lateral viscosity variations in the lower mantle. Viscosity is relatively uniform having values between 5×10^{22} Pa s (around piles and regions of high melt content) and 5×10^{24} Pa s (regions with high melt content).
Most of the lower mantle has a viscosity on the order of 5×10^{23} Pa s. Solomatov Moresi (1996), Karato Rubie (1997), Solomatov et al. (2002) and Korenaga (2005) suggest that higher temperatures in plumes could result in higher viscosity due to larger grains. This suggestion cannot be supported with our simulations, but might be probable if different grain growth parameters, for example stronger grain growth, were used. In our simulations, the expected increase in viscosity due to larger grain size in plumes is buffered by the higher temperature of the plume itself. The surprisingly high viscosity of regions with a high melt fraction is not a physical observation but results from how the overall viscosity is computed. We only use the grain size in the solid matrix to compute the viscosity and neglect the impact of the melt content which is usually fine, which is usually fine except for regions with a particularly high melt content."

Appendix:

I find this terminology of "continuous" versus "episodic" very confusing, as well as the further classification of "events, then constant," "constant, then events," etc. I'm not really sure what this classification is supposed to help the reader see. Maybe better to just show some example models individually and indicate where stagnant, mobile, and episodic overturning phases occur, so we can see how these effect the grain size evolution?
¶Generally, the results all show the same behavior, meaning we see large drops in grain size right after an overturn event, or a relatively constant grain size if the run does not show any overturn or downwelling events. The appendix arose from the fact that we initially decided to structure the paper differently, where we tried to find dependencies of the constant or episodic behaviour on the input parameters. However, this proved to be impossible and we re-structured the paper around the stagnant lid, plate-tectonic-like and overturn phase. The figures in the end are only there to demonstrate that the simulation results of the pile material show a similar behavior and basically only depend on the convection regime. We have removed the appendix since the figures don't really help to understand the points we try to make in the paper.

Lines 553-554: That basalt is not mixing in with the piles is an important point that needs to be explained further and compared with McNamara/Mingming Li work where they argue for basalt incorporation into piles

¶This part we have removed. We realise that it is interesting and might be of high importance but we didn't study this observation in detail, therefore we cannot give any detailed results or explanation.

Appendix A3: Plotting density alone is not so useful. What really matters is the density difference between the pile and surrounding mantle. For example, the decrease in density seen due to the piles rising is not really dynamically meaningful as it is due to decompression. We need to know the density relative to surrounding mantle to see if the buoyancy has changed.

¶We have removed the appendix. We decided, following the comments, that the appendix does not add anything valuable to the paper.