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Interactive comment on "Increased LLVP density recovered by seismologically constrained gravity inversion" *by* Wolfgang Szwillus et al.

Anonymous Referee #1

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Dear editor and authors: I have read this manuscript and am under the impression that it requires a major revision. Here are some comments/questions that I hope can be useful to you:

- What is the benefit of using "vote maps" over tomography models? Density does not necessarily need to be "scaled" from an existing tomography model: the scaled tomography model for example could be used as a target model towards which your inversion could be "regularized". This could allow for density/velocity decorrelation where required by the data. A similar philosophy was followed e.g. by Simmons et al. to derive their "Gypsum" model (2010). Why not try this approach as well?

- In relation to the above: at lines around ∼50 the authors imply that "converting seismic velocity anomalies into density anomalies implies a purely thermal origin". I don't think

that this is true. Many studies combine P and S velocity heterogeneities to estimate both thermal and compositional effects.

- lines 55-60. I disagree that there are "few compatible direct observations of core-mantle boundary deformation". See e.g. Soldati et al. GJI 2012, Soldati et al. G3 2013 (And a lot of literature before and after). Incidentally, you seem to imply here that "the gravity effect of any deformed boundary [...] can simply be calculated and removed"... but is this what you do? You seem to neglect the deltarho caused by the CMB in your inversion, to then compute it a posteriori? And how about the 660 and other internal discontinuities?

- does this paper help us in any way to understand the earth better than we already do? The density maps (Figures 5 and 7) does not reveal anything new, or does it? the CMB map does not add anything to the many earlier observations and models (which at least to some extent would be useful to discuss, starting with Morelli and Dziewonski 1987).

In summary, constraining the density structure of the mantle is an old problem that motivated a lot of work. I appreciate the authors' effort to contribute, but I have the impression that a major revision - expansion of their current work is needed for their contribution to be really helpful.

some minor comments

- lines 70-71 "we stay close to the original source of the data" –> not sure what this means. - line 114. "anomalous regions that is each" –> that are each... their... their ... unknown density valueS - page 6: the inversion algorithm could be described in a more transparent fashion. A clear notation should be adopted; for instance, either all vectors should be boldface, or none; it should be stated explicitly that g is a vector (if I understand correctly) whose i-th entry corresponds to the point r_i lat_i lon_i within the earth, etc. When you say "Aij is the gravity effect of potential density anomaly j on measurement point i", do you perhaps really mean that A_ij rho_j is the effect in question? Is A really a "design" matrix? What is a "design" matrix? Does the formula for the entries of A follow from eq. (4)? Could you then write explicitly a mathematical expression for A_ij? Or, if I misunderstood, explain explicitly how in practice A_ij is calculated? In what sense are L curves "ad hoc"? (I think you are right that there is a problem with L curves, but I don't understand what you mean by "ad hoc".) - caption of fig. 3 "topgoraphy" - line 205 "are the similar" - line 358: "We assume a negative S-wave velocity deviation of 2 per cent"... After so much work to improve estimates of density anomaly, why is it OK to use such a rough estimate for velocity? Also, it has often be suggested that tomography might be systematically underestimating heterogeneity amplitude.

Sincerely

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