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Interactive comment on "Gravity Effect of Alpine Slab Segments Based on Geophysical and Petrological Modelling" *by* Maximilian Lowe et al.

Anonymous Referee #2

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The manuscript links seismology, petrology and gravity to discuss the effect of Alpine slabs on gravity anomalies. The petrological data mainly exploits composition to derive rock density information used in the modelling of two key scenarii: constant density distribution, and compositional + thermal variations with depth. Various other parameter values are varied to estimate the slabs' gravimetric signal. Beyond gravity anomaly calculations, gravity gradients are also presented but their significance is a question.

Overall, the calculations and motivation are interesting, but the goal, why and how this is done could be much better emphasized, even if the results are not conclusive in imaging slabs, but in estimating the amplitude of slabs' contribution towards the total gravity field. I'm still hesitant whether the initial goal of this work was to reproduce some features of the Alps, or to provide order-of-magnitude effects of various model

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Discussion paper

assumptions – probably the latter, but this could be more clearly expressed.

There are, in my reading, a number of points to clarify in the motivation, approach and discussion of the paper, as many elements remain less described than optimal. At the same time, goals could be better expressed from the beginning, as when the reader arrives to section 4, there is already some confusion on why and how certain steps were done, and what comes next. I try to highlight possible improvement points in my comments below.

There are also a number of moderate to major concerns regarding the methodology or choices that require, in my opinion, more than polishing the text. I also describe these here below.

Moderate to major concerns.

1) Seismic tomographies

Various results from seismic tomographies are mentioned in the introduction, but some significant papers are not included. Namely, numerous papers by the Prague group, discussing Eastern Alpine slab structures and proposed dual origin about 30 years ago. Since this is one of the main assumed models, I recommend these references are included and thoroughly cited in the manuscript. See also the EASI profile's receiver function results put in context of tomographic results in the Eastern Alps.

Moreover, the reader is referred to the paper by Kästle et al. 2020 for further discussion of Alpine tomographic models. I find that part of that publication is misleading as their Figure 3 juxtaposes different tomographic model pieces as if this was an accepted approach, while it is certainly not. Their Figure 3 has no colour scale – caption refers to Figure 3 itself – one can guess it is meant to refer to Figure 2, which shows that colour scales vary from tomography result to tomography result (5, 3, 4 % in VP, then 2.5 % in VS, from columns 1 to 4). Not only do these tomographies differ in the scale of shown anomaly amplitudes, but they also differ in the amount of data, their coverage, their resolution, and inversion details. Therefore, for non-specialists, it is misleading to refer to this work as the Alpine tomography reference.

This is indeed a critical point as the model setup later on in the manuscript (e.g. Fig. 5) is also based on several tomographic sources, and is therefore subject to the inherent variability between tomographic models and their resolution. Errors stemming from merging several sources, or smoothed geometries across various choices, must be estimated in order to check their effects on the final results.

2) Velocity to density conversion

In section 2 of the manuscript, it is not always clear which kind of velocities are or will be used, only S or also P, and in which way. Please adjust the text to make this clear.

In section 3, a conversion factor is introduced that allows to convert seismic velocities to density. Although there is a list of references for the range of values, it would be beneficial to know whether this refers to P or S wave velocity conversion factors. Moreover, it would be very useful to see a formula for the conversion, how this is used, what are the units, does it refer to absolute or relative numbers? The numbers in the current manuscript leave me hesitant about this. Is there a T and P dependence of this conversion factor? Is it linear with no offset at zero? (I.e.: is the form y=a*x or y=a*x+b?) Why is the choice of 0.3 is taken in this study, what are the uncertainties of this choice?

3) First model setup and calculations

In section 3 some clarification is needed to resolve the followings.

Line133 says: "The converted 3D relative density distribution includes all heterogeneities in the Alpine lithosphere", but it is not clear how the depth range 0-70 km (the bulk of the lithosphere, and the most influential for the gravity values) is converted, or constructed in terms of density values. This is VERY critical in my opinion to be able to compare synthetics with observations.

Line142 says: "In the gravity field, a gravity high with a magnitude of ∼40 mGal is observed over the Alps." This is not what is shown in the data. Later, I see this approaches what is on Figure 4, but there is no mention here about the forward model calculations, how they were done, how comparable are the results to the observed data. Why is this 40 mGal if the data showed a negative anomaly approaching -200 mGal (Figure 1). Is this related to the model definition between 0-70 km depths?

(Or, if the goal is not to compare it to observation, but simply to give estimates of slabs' gravimetric effects, then this needs to be clearly stated – and in this case it is not clear why gravity data is presented in Figure 1?)

4) Slab definitions

It seems that the slab volume (its definition) can be debated. Line 165 mentions 44 km (abstract and L199 refer to 40 km?) depth as the beginning of slabs. Why was this value chosen, and more importantly, what uncertainty do the RF Moho uncertainties represent in terms for slab volume? Then, Line 168 says that the 0% tomographic isoline was chosen to define a slab shape. Same question as above: why this value as chose? It seems very "optimistic" to call a slab anything that has fast, even marginally fast velocities. For example, is +0.01% anomaly part of a slab? Or +0.1%? Or can it be shown on cross-sections that this choice has a negligible consequence? Finally, on line 174, please specify how the thickness parameter was chosen (or was it inverted for?).

5) LitMod models

There are a few elements of LitMod model definitions that would be worth better arguing, or at least describing.

Line 250 and around: this is too succinct, please discuss why these model compositions were chosen (how were those models assembled, do they refer to continental material?), and how well they represent the Alps. See also around Line280-282 (e.g., why is a Proterozoic slab composition selected?).

Line 263 and around: choosing the LAB to define models is a different way that what was presented for constant-density models. Are the model results going to be comparable?

In particular, it is quite surprising to read about the constant LAB depth choice as a reference. Sparse data and that it is in discussion is a weak argument not to consider those data – the situation is the same for tomographic models. And if Artemieva's model shows +/-20 km variations in depth, why not include those?

Line 284 mentions a -100°K temperature anomaly. Is it for the slab? (Sentence ending on L285 says sub-litho.). Is this anomaly kept constant across the entire slab? How reasonable this is compared to the geodynamics of the Alps? Why aren't the thermal equations resolved if this is said to be an option in LitMod? Subsequently, models 7 and 8 are described to do include temperature variations (in space? I assume...), but Table 2 says T-anomaly of -200°K. What is the situation, then?

Figure 8 shows vertical slabs, and with different thicknesses. Why these two choices?

6) Discussion

The discussion is a mostly fair description of the results, stating some of the difficulties, limitations, and unresolved elements. Yet there are a few statements that can be contested:

L387 says: "For all three modelling approaches (section 3) a measurable gravity effect of the subducting slab segments is seen". I agree that all models produce gravity anomalies that are measurable. But I have doubts that this signals are distinguishable within the total field, when one considers field observations. This is even more critical as the anomaly levels vary a lot between constant-density and LitMod-based models. (Moreover, section 3 is not about modelling approaches, is that number a typo?)

L388 says: "The independent slab segments are distinguishable to a certain degree". I think this statement is over-interpreting the results. If the images would be shown to someone not familiar with deep structure of the Alps suggested from seismology, I have doubts whether that person would point to independent slab segments. See also previous comment.

L390-397: here the comparison is between constant-density and LitMod model results, but the way these models were defined (tomography contours from 70 km depth, resp. from LAB at 100 km depth) raises questions on comparability.

L401-407: here is a clearer message on why these calculations are useful. Maybe consider taking this into the intro?

L415-417: the sharpness is pre-defined in the models, especially for temperature (which affects densities), and reality is probably less sharp than this.

7) Writing

Although the message of the paper can be mostly followed and understood, there is some variability in the level of information and in being "to-the-point". A proofreading after revision could smooth these out. Information pertaining to the same topic are sometimes found in rather different parts of the manuscript, these could be better grouped. An example: which method is used to calculate synthetic gravity anomalies, and what kind (how many) different model resolutions have been used. L135 says 0.2° * 3 km but does not mention the method, L197 mentioned the method but has 0.2° * 20 km. Many statements say slabs extend to 200 km depth, L199 says 220 km. It is a bit difficult to keep up with these information bits.

Minor comments on text that I have not really or well understood.

-Line14-15: unclear what is "crustal thickness at 40 km depth" – please resolve this oxymoron.

-I find the abstract could be more specific and to-the-point. Which kind of gravity data is used and how to constrain the models? What are those significant pattern differences, and which model seems more realistic?

-L31-33: revise unclear sentence: “A major role. . . plays the Adriatic microplate”

-L34: Adria’s rotation as seen by GPS could be cited here.

-L50: before mentioning E. Alps, maybe mention the size of the potential slab gap between C. and E. Alps?

-L51: correct “is been”

-L51-54: briefly explain on which observables this “classical” view is based; and, then, which were the arguments for challenging these.

-L59: dual subduction was first proposed by Prague group, in the early 1990’s, and not by these two very recent papers. Several publications from the Prague group discuss this in detail, please mention their interpretation of a dual subduction, possibly in chronological order (before Lippitsch).

-L67: correct “dominate”

-L66 and Fig. 1a: a much clearer description of what the Bouguer anomaly map is needed. What is XGM 2019? How was it obtained? What kind of resolution to expect? Or refer to what is written below. On Fig. 1b please make the fault lines more apparent (thicker line, or another colour).

-L74-79: a better and more detailed description of the motivation, approach and goals would be very welcome here. For example, petrology is not mentioned here at all.

-L81-83: please add how this model was obtained. Satellite (which satellite?) data only? Or also land data? To make it resolved at 25 km, what assumptions were taken? Local isostatic equilibrium of the topography? (If yes, is it surprising to see isostatic equilibrium in line 88?)

-L90: use of GOCE should be mentioned earlier. What was the purpose of using these gradients?

-L101: correct the reference year

-L105: how is the model constructed between Moho depth and 70 km depth level?

-L106: “depth-dependent average shear-wave velocity 1-D model” – is there some redundancy in the description?

-L108-111: the choice of 200 km depth as bottom of the model, and that anomalies below this level, seems ad hoc. Would it be possible to quantify/to justify? The El-Sharkawy et al. results extend to 300 km depth.

-L117: see earlier comment on dual subduction and its references.

-L139: the label says Vsv is used, is Vsh also available? Why choosing one rather than the other?

-L165: Please correct “crustal mantle boundary”.

-L192 and 198: what is the resolution of the MeRE2020 tomographic model used here, and how does it compare to 0.2° horizontal and 20 km vertical tesseroid size?

-L197: finally comes the calculation details. I think this deserves defining a separate sub-section.

-L206: is it density anomaly instead of density?

-L210: uncertainty is mentioned for the first time. I agree this is important and I'm happy to see this word here. But could you please refer to uncertainty of gravity, models, tomographies also earlier in the manuscript, so that it does not come as a surprise here, and we know why parameter values were chosen as chosen?

-L211: it seems that a new section starts here, with results? If yes, please clearly state it (new sub-section). For the first model result presented here (drho=60kg/m3, Hslab=80km): why was this particular model chosen?

-L222: is it thickness rather than volume?

-L245 (twice): correct spelling Perple_X.

-L246: this is not what Perple_X does, this is one thing Perple_X can do. The choice of input (here 6 oxides only), the thermodynamic databases, list of solid solutions, etc. are all user chosen. Maybe the description is simply how LitMod uses Perple_X?

-L251: correct homogene to homogeneous, sub to sub-

-L275: this could be presented much more nicely, with several columns representing the type of information (lithosphere, sub-lithosphere, slab config., T anomaly, etc.) and the lines showing the information itself. From the current version it is very difficult to have an overview of tested models.

-L295: is this the beginning of results? If yes, mark it with a sub-section, for example.

-L306: please clarify what causes these <1kg/m3 variations, temperature?

-Figure 11: please match “Configuration” in caption and “Hypothesis” in the figure.

-L372: typo in mantel

-L386: how realistic is this -100°K anomaly, and, therefore, the 16 mGal anomaly?

-In the conclusions, I’d recommend mentioning that future results based on AlpArray Seismic Network data will be of high interest in better defining slab geometries AND properties.

Appendix

-L634-635: some references to the data would be useful.

-L637: correct “longwave length”

-L641-645: are the obtained results in E on the same order of magnitude that you would expect? What do these maps mean, then? What is the support for the interpretation on lines 650-652 (positive signal of 0.5 E could be linked to slabs)?

-Figure 14: please use the same colour scale and range as on Figure 13.

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