Dear Editor,

Below please find explanations on comments from the reviewer and our replies. The reviewer accepted all our replies and was very positive about the revised manuscript. We accepted most of reviewers new technical corrections – see marked manuscript R2. Nevertheless, the reviewer left a couple of technical corrections to the Editors decision, and we would like to provide some explanations about those comments for your easier decision. The comment-replies are structured as specified below.

In red - reviewers initial comment
In black - authors initial reply
In green - reviewers final reply
(if needed) In black - authors additional explanation

Comment #1

It is not clear how useful it is to present maps showing S-wave velocity and density. In my opinion, they could be removed since they are the result of simple (empirical) P-wave velocity conversions. The resulting maps do not contribute anything to the main conclusion of this study.

Density was also calculated-interpolated, only the input data set was much smaller. We included the S-wave velocity for completeness (since we had P-wave velocity and density). This was done to facilitate easier usage of the S-velocity as a starting model for some future research (ambient noise or surface wave tomography, earthquake shaking estimation, etc.). If the reviewer feels that the S-wave model still needs to be removed, we can move S-wave figures to supplement.

The way the model parameters have been estimated is much clearer in this new version of the manuscript. I would leave the decision to the editor where to present the S-wave velocity model (main text or supplementary). As the description remains very short, it could stay in the main text as well.

We agree with the reviewer and would leave the S-model in the main text if the Editor agrees.

Comment #2

As an important point of the critical discussion of model limitations, the authors should comment on the fact that they have jointly interpolated P-wave velocities from active seismic profiles and those derived from gravity constrained density values. Is this reliable? There seems to be a systematic jump in velocity when crossing the boundary between the different domains. Does this mean that such abrupt changes in the interpolated maps do not reflect real differences in rock physical properties but inconsistencies due to different methods?

As the above mentioned data were the only available measurements in some areas there

wasn't much choice to begin with. How reliable some of the transitions are is hard to estimate but we strongly believe that a good part of "jumps" between different domains reflects true physical properties. The models acquired from gravity data which were used in our work were taken from work of Šumanovac (2010) where the author calibrated gravity modelling based on active seismic profile parallel to one of the gravity profiles. We have added the following paragraph to the discussion chapter:
"It seems that the velocity in the Internal Dinarides, where we only had inverted gravimetric profile data available, is slightly higher than in the rest of the model. At this point, we cannot discern if it is an actual feature, or some artefact due to lower quality data. The fact is that this is a different tectonic unit, so it is not impossible that it has different features. If we have omitted these data from interpolation, we would have even worse results, because in that case the values would be purely extrapolated. The approach we chose gave at least some constraint to the velocity values in this part of the model."

The authors have been more precise and more critical in describing the results in the revised manuscript. A new, general point on this chapter: the travel time calculations have been performed to test the presented 3D model. The tests appear in the "Discussion" chapter since the authors obviously want to discuss the reliability of the model in this way. However, since they present these tests with a description of the data / techniques used and results obtained, the "Discussion" chapter loses much of its "discussing" character. This is definitely not a standard way of structuring a scientific paper, but I would leave it to the editor at this very late stage of the review process, to decide whether the authors should modify this.

We agree with the reviewer that it's not a standard way of structuring but feel that leaving it within the discussion section greatly improves the readability of the manuscript and improves the overall discussion by including model testing within discussion on model characteristics.

Comment #3

Line 434: Figure 3 - Choose the same colour palette for maps of the same physical unit. All interface depths (in m or km) including the Moho should have the same colour palette (here Moho depth is presented in the same colours as vp-values in Fig. 4, which should be avoided). Uncertainty (also in m, respectively km) could be shown by a different colour palette.

This was done on purpose. Notice that sediment and carbonate bottom depths have values going from 0 to a certain value (6 km and 15 km respectively), the chosen colour palette better shows the features (0 km is shown in white, while the maximum values are shown in black). Same is true for the errors. On the other hand, that colour palette does not adequately represent the features of the Moho, and therefore we chose the same one as for the Vp.

I would leave this decision to the editor, since this actuall regards the standards of the journal.

We stand by our first answer as this colour scheme greatly enhances visual representation of different values within the model.

Comment #4

Lines 620 - 622: To test how well the newly derived 3-D model represents the true structure, we calculated the travel times for a regional earthquake recorded on representative seismic stations in the wider Dinarides area (Fig. 8) - This model validation approach should be mentioned in the introduction, the description of the modelling approach and shortly also in the abstract.

In the abstract, we have rephrased the sentence: “The newly derived model has been compared with the simple 1D model used for routine earthquake location in Croatia, and it proved to be a significant improvement.“ to “To validate the newly derived model, we have calculated travel times for a regional earthquake recorded on a number of seismic stations in the Dinarides area. The calculated travel times have been compared with the travel times in the simple 1D model used for routine earthquake location in Croatia, and it proved to be a significant improvement.” We haven’t mentioned it in the introduction nor in the modelling approach to reduce repetition.

I appreciate that this important step is mentioned in the abstract now. Instead of describing the technique in the discussion chapter, however, I would move it to the “modelling approach” chapter (see also below).

On this one, we feel that it should stay in the “Discussion” section. The testing of the model does not have any connection with how the model was assembled i.e. “Modelling approach” and is much more useful when discussing the performance of the new model.

Comment #5

Lines 623 - 631: The 1D model’s topmost layer is characterized by P-velocity of 5.8 km/s, and the deeper crustal layer has the P-wave velocity of 6.65 km/s. For the same model the uppermost mantle velocity is 8.0 km/s. We then compared the travel times from both models with the true measured travel times. We used the Pn and Pg phases of the 2020 Petrinja Mw6.4 earthquake. The location of the earthquake and the stations that recorded the wave onsets are shown in Fig. 8. For the same stations we calculated the travel times using the 1D and the new 3-D model. For travel time calculation we used the Fast Marching Method (de Kool et al., 2006) as implemented within the FMTOMO package (Rawlinson and Urvoy, 2006). - This is methodology and should be moved to a chapter "Modelling approach".

This is simply a description of the 1D model we used, and the means of calculating the travel times, so we think it should remain in this section.

I would still argue for moving this paragraph to the “modelling approach” chapter (see also the comment in the PDF), but I would leave the final decision to the editor.

As before we feel that describing this 1D model in the discussion gives better overall covering when testing the model. If we described this in the “Modeling approach” it would be lost until we got to the point where we actually use this model. Also, we are not using this 1D model to create a new model but to test the performance of the new model hence it should be in the discussion.

Comment #6

Lines 656 - 657: generally closer to the actual observed travel times for all the epicentral distances shown than those calculated using the 1D model. - This is not obvious from the figure.

The abscissa in the figure shows the difference between observed travel time and travel time calculated from 1-D (black points) and 3-D (blue crosses) models. The differences with 3-D model generally lie closer to the dashed line, representing zero, meaning they are closer to the observed travel times hence the conclusion that generally travel times for 3D model are closer to the observed ones.

Can you still comment on why there is a trend of 3D model points to show negative time difference, while 1D model points mostly show positive values (for both Pg and Pn phases)?

The travel time calculated using the new model is slightly smaller than the observed travel times. Total travel time is influenced by all the velocity anomalies along the ray path, and at this point we cannot say for sure which part of our model is causing discrepancy. For Pn phases, the upper mantle is mostly affecting the travel time, but that one was not derived in this work, but simply taken from Belinić et al. (2020), and they have reported the potential problems (especially for the uppermost mantle which the Pn phases cross).