"The diverse crustal structure and the magmatic evolution of the Manihiki Plateau, central Pacific"

by K. Hochmuth, K. Gohl, G. Uenzelmann-Neben, R. Werner

Authors' response addressing the comments by M. Prada (referee)

Italic letters indicate *a quote from the revised manuscript or literature* cited in the text.

General comments	
Referee comment	**Author's response**
However, there are several issues concerning the methodology. The authors decide to proceed with a forward modeling method, which depends on previous assumptions and is not as robust as an inversion method. Few of these assumptions are argued by the authors - e.g. Line 183: "We used bathymetric and seismic reflection records . . . ..to constrain modeling parameters for the seafloor and the thickness of sedimentary covers" -. However, there are no explanations on the assumptions made for the initial thickness and geometry of intracrustal layers, as well as the criteria used to modify thickness and/or velocity gradients while proceeding whit the forward modeling. Additionally, one of the biggest limitations when using a forward modeling method is that one may not constrain the non-uniqueness solution of the model parameter, in other words, one may not quantify the model parameter uncertainty - in this case P- and S-wave velocity and depth of reflectors - . In this study, for instance, it would be strongly necessary the as-sessment of the depth uncertainty for the intracrustal reflectors, since they are poorly constrained by few P-wave reflections. This is a major issue that has to be addressed before proceeding with other velocity-derived analyses (i.e. Poisson's ratio model),	**Forward modeling vs. inverse modeling:** Forward modeling as used in the presented models has, as the referee claims, its disadvantages. The models strongly depend on the individual modeler and his/her starting model parameterization. For using inverse modeling, the starting model can be quiet basic and the numerous iterations of the model during the model process are not bound to subjective judgment, but are choose by statistical parameters (e.g. RMS-fit…). Additionally, inverse modeling allows the modeler to "…explore the model space more freely and find alternative velocity models more easy" (Korenaga & Sager 2012). By using the graphical interface *Pray* (Fromm, 2012), the modeler can easily manipulate depth and velocity functions during forward modeling and compare older versions of the model with newer versions to allow better constrains and a better fit of the model. Additionally, by using inverse modeling, sudden increases in velocity, resulting in a reflection are mostly modeled by the addition of a reflection surface or a floating reflector. In our case, we see strong internal reflections within the crust, which can only be attributed to a sudden change of material. To produce a viable model of the crust of the Manihiki Plateau, it seems important to model this characteristic

and particularly before state any interpretation. I only see two ways in which the authors may improve this study: The authors could proceed with an inversion method to model the WAS data, and in addition, assess the model parameters uncertainty, which is unfeasible when applying forward modeling. This way, tomographic results would be more robust and geological interpretations more reliable. I strongly recommend this first option. In contrast, if the authors decide to base their interpretations on forward modeling results, I suggest to improve the Processing and modeling section by adding more details on the assumptions made for the initial model, the criteria used at the time of varying depth and velocity gradient of each layer, and few lines explaining how reliable are the intracrustal interfaces (reflectors).

feature. Forward modeling allows modeling those sudden increases in density and/or velocity and reproducing the corresponding reflections. During the modeling we also used the implemented travel-time inversion routine to produce a better fit with the picked data in the way of ‘fine-tuning’.

Constraints of the starting and subsequent models:
For the initial set-up of our models for the sedimentary cover and the upper basement, we used the bathymetric data collected during SO-224 as well as the corresponding reflection seismic data by Pietsch and Uenzelmann-Neben (2014). The reflection seismic data was converted to depth with velocity models derived by careful velocity analysis. As additional information on the crustal set-up, we used the available data from the upper and middle crust published by Winterer et al. (1974) and Hussong et al. (1979). We also calculated 1-D velocity depth profiles at individual OBS stations.

Depth uncertainty:
We carefully re-examined the parameter uncertainties in our models especially the depth uncertainty of the intercrustal reflectors. In the following table, we collected the depth uncertainty in km of the individual layers of both profiles.

	20120100	20120200
PuP	+0.4/-0.5	+0.5/-0.4
PumP	+0.7/-0.4	+0.6/-0.4
PlmP		+0.6/-0.7
PmP	+0.7/-0.7	+0.8/-0.5

This table is also added to the revised manuscript.

The results of the re-evaluation as well as more information on the initial model set-up are presented in the

	section *3.2 Processing and modeling of seismic refraction/wide-angle reflection data* of the revised manuscript.

Specific comments	
Referee comment	**Author's response**
Line 176 to 180: There was no processing after relocalization of OBS/H? If there was any, mention it here (Band pass filtering, deconvolution...).	A band pass filter was applied, and we tried to improve the phase quality by deconvolution. However, the data have a very good quality and the different phases could be well distinguished by their different arrival times and velocity distribution. Further processing such as a deconvolution did not improve the phase identification and picking quality.
Line 179: "We assigned picking uncertainty..." Be more specific; how much it was? It was the same uncertainty for all phases?	Phase picking uncertainty selection was applied throughout the phase identification process using the ZP software. The individual uncertainty of every pick is calculated by the signal-to-noise ratio. The uncertainty of the picks ranges from 0.075 s to 0.25 s. The median picking uncertainty is 0.1 s. The uncertainties obtained for reflections and refractions are similar and rather depend on a certain station (e.g. movement due to bottom currents, "malfunction" of the instrument) then on offset and depth. A more detailed overview on the uncertainties assigned to the picks is added to the revised version of the manuscript.
Line 183 to 185: "We used bathymetric and seismic reflection records. . ..to constrain modeling parameters for the seafloor and the thickness of sedimentary covers". Is this all the information that the authors used to constrain the initial model? Are the bathymetric and seismic reflection records published? If so, cite them here please. On the other hand, seismic reflection records are in TWT, is it right? How did the	As mentioned above, we used the bathymetric measurements obtained during the cruise SO-224 as well as the seismic reflection data to constrain the sediment cover and the location of the basement. The seismic reflection data was converted from two-way-travel-time to depth by using the velocity analysis (Pietsch and Uenzelmann-Neben, 2014). Additionally published data from Winterer et al/ (1974) and Hussong et

authors convert the geometry of the sediment-basement boundary from TWT to depth? Did you assume some average P-wave velocity? After these lines, is where I consider that the authors should provide more details concerning the initial model construction, and the forward modeling procedure. To me it seems that there are a lot of predetermined ideas in your models that are not exposed in your manuscript.	al. (1979) along with 1D sections of our OBS data were taken into account. More detailed information on the initial set-up of the model is added to the revised manuscript as well as the relevant citations.
Line 189 to 197: The good quality of the data make easy to identify most of the seismic phases presented in these lines. However, I do not see PmcP in Fig. 2a and PumcP in Fig. 4a. Perhaps the authors may provide a better example of these phases. Also, I would like to ask the authors; what criteria do they use to discern intracrustal refractions (Puc, Pumc, Plmc,...) when no corresponding reflections are observed? I believe that this should be clarified in the manuscript. The authors should also resolve important misfits like those observed for PmP reflections in Fig.4b and Suc refractions in Fig.5b. This issue has to be resolved before presenting a final velocity model. Additionally, include in the manuscript the RMS value for each model, and also few lines explaining the meaning of resolution, which is different from uncertainty.	The good quality of the data allows us to distinguish the different crustal phases by their corresponding reflections but also by the different velocities and intercept times. Additionally, the reflections of the intercrustal reflectors can be observed throughout the central parts of both plateaus, so the distinction between the three/four intracrustal phases is supported by their refractions and reflections. In Figure 2a), the PmcP phase is depicted in dark red colors and is situated very close to the Pmc phase. In Figure 4a), The PumcP phase is depicted in red. In the seismogram this phase is particularly hard to see since the overlaying PucP partly masks it. The observed misfits have been in the Figures 4b and 5b resolved by the re-evaluation of the OBS-data and the model. The different parameter's uncertainties of the model have been re-evaluated. The resolution is calculated for the individual velocity-nodes of the model. The RMS-value examines the fit between the modeled and the picked velocities and is shown in the figure captions of the models.
Line 205 to 206: "We calculated the Poisson's ratio..." Specify that this	The S-wave model of AWI-20120100 is limited only to the upper and middle

calculation is only done along transect AWI-201202200. Why did not you present the Poisson's calculation of the other line?	crust and bears large uncertainties. The calculated Poisson's ratio proved not to be interpretable.
Line 216: "software IGMAS" Is it published? If not, add few lines about the basis of this software or reference previous works that used it and already explained it.	The software IGMAS is now referenced. (http://www.geophysik.uni-kiel.de/~sabine/Sabine_IGMAS.html)
Line 219: Specify that the P-wave velocity-density conversion using Hamilton (1978) is only applied for the sediment cover velocities.	Velocity–density conversion was applied for the sediments and crystalline rocks after Hamilton (1978). In addition, we used the conversions by Barton (1986).
Line 222 to 223: "a perfect fit could not be achieved by retaining realistic model parameters" What does it mean? What parameters? Please, explain yourself.	The 2D-model approach has its limitations due to side-effects. Close-by features such as seamounts affect the local gravity field, but are not visible in the 2D section produced by seismic refraction/wide-angle reflections. Therefore, we evaluated the surrounding seafloor carefully in bathymetry and gravity anomaly maps, for possible causes, alternating the local gravity field. This revealed e.g. that the presence of Suvarow Island on the High Plateau influences the gravity measurements. This can explain the misfit of the data in this region. The gravity model was designed to represent realistic parameters such as the given seafloor topography and sedimentary column.
Lines 241 to 242: "Bathymetric....faults and grabens". Where are these Bathymetric and seismic reflection data? Reference please.	The following references are added to the manuscript: Pietsch and Uenzelmann-Neben (2014), Winterer et al. (1974).
Line 249: "Seismic reflection data reveal" Again the authors talk about seismic reflection data that are neither referenced nor presented. Please add some reference.	The following references are added to the manuscript: Pietsch and Uenzelmann-Neben (2014), Winterer et al. (1974).
Line 273: "S-wave velocities show a block-like structure" What does it mean? Line 295 to 297: "the Penrhyn	The values established by Christensen (1996) for serpentinized crust refer to the uppermost crust in

Basin.....are typical for serpentinized crust (Christensen 1996)” I disagree in this point. Christensen (1996) shows in his study that Poisson’s ratios of 0,35 are representative of serpentinites, and that ratios of 0,30 are closer to basalts and gabbros. Is true that Viso et al. (2005) suggest a layer of serpentinized crust in his gravity modeling, but they based this assumption in few serpentinized rocks dredged in the Manihiki scarp, which, in my opinion is not enough to sustain this interpretation. In addition, the Poisson’s ratio model includes the uncertainty of the P- and S-wave velocity models, which means that Poisson’s ratios of your model may vary significantly. Since Poisson’s ratios presented in this study for this layer are not so far from those of basalts and gabbros (Christensen, 1996), which make sense since we are in an oceanic crust region, I suggest the modification of the interpretation shown in Fig. 10 and 14 from serpentinized crust to a basaltic crust. However, I agree with the fact that the uppermost basaltic crust might be partially serpentinized, but I would not emphasize that in the figure but in the text with a very short line.	our model. We agree, that the combined uncertainty of the P- and S-wave model in the Poisson’s ratio model make it complicated to draw a clear distinction between basalts and serpentinite. We therefore interpret that the Manihiki Scarp and the Penrhyn Basin are mainly formed by basalts and show serpentinization in the uppermost crustal layers. This has been revised in the text as: *“…the Penrhyn Basin on the other hand shows high Poisson’s ratio values of over 0.30 (Fig. 10), which is typical for basalts and partly serpentinized basalts in the uppermost crust.”* The figures 10 and 14 are changed accordingly.
Line 313 to 321: “East of the troughs, the middle crust is divided...seafloor at the Manihiki scarp” Please try to avoid this descriptions and speak about crust as a whole and not as layers, since you do not have constrain on the thickness and geometry of these intracrustal layers beneath this region.	The crust of the Manihiki Plateau clearly shows a layered structure. The seismic reflection data of Pietsch and Uenzelmann-Neben (2014) image intrabasaltic reflections, which can be observed within a layer-like zone throughout the High Plateau. This observation along our seismic profiles is likely to be representative for the intra-crustal structure of most of the plateau.
Line 345 to 347: “The boundary between. . ..as well as reflections at	The Moho is defined as the sudden increase in P-wave velocity between

the Moho itself" Technically, only PmP reflections constrain the Moho location. Pn refractions are only used to solve the velocity distribution of the uppermost mantle, and partially the crust. Please modify these lines.	the mantle and the crust. The PmP reflects directly at the Moho and the Pn refraction shows the velocity distribution of the uppermost mantle. The Moho is visible in the refracted rays in the velocity increase between Plc and the Pn and the reflections at the Moho PmP. Therefore, we did not rewrite this sentence.
Line 350: "which includes the sedimentary cover" The crust, by definition, includes the sedimentary blanket and the basement, so that it is not necessary to specify that. Please remove the quoted sentence.	In the literature, the term 'crust' is often used relatively unclear, depending on the opinion of the different authors, if the sedimentary cover is included or not. Therefore, it is useful for clarity to state that the sedimentary cover is included into the term 'crust' in our case.
Line 501: Change "Gravity anomalies are mainly attributed" for "Short wavelength gravity anomalies are mainly attributed"	The sentence has been changed in the revised version of the manuscript. The term 'short wavelength gravity anomalies' pinpoints the nature of the gravity anomalies e.g. as resulting from seamounts.
Line 594 to 597: "On the Western Plateaus, faults....along with further sedimentation". I do not see how faults reaching the basement indicate that stretching was coetaneous to second stage of volcanism.	We added an additional figure to the manuscript, which shows normal faulting in the seismic reflection data of the Western Plateaus. The interpretation that stretching and secondary volcanic phases are contemptuous results from the fact that the normal faulting observed in reflection seismic data reaches from the basement, across the sedimentary cover and to the seafloor. Secondary volcanism peaks around 80 Ma (Pietsch and Uenzelmann-Neben, 2014), and at this time spreading in the Ellice Basin is still present with extensional forces acting on the Western Plateau.

Figures and Tables	
Referee comment:	**Author's response:**
Line 183 to 185: "The resolution of the S-wave velocity. . .." Concerning the resolution the P- and S-wave models, I recommend the authors to blank those regions of the model that are not constrained by rays, since in Figures 6, 7, 8, and 9, the transparent	The figures are modified to avoid confusion.

grey area is poorly visible and leads to confusion.	
Fig. 13 and 14: How do the authors infer the offset of normal faults? Some of them are deeply-rooted into the crust. Which are the observations that allow to interpret such fault lengths?	The offset of the faults can be seen in the bathymetry as well as in offsetting horizons in the seismic reflection data. The length of the faults is an interpolation and will is referred to as “assumed fault systems” in the revised manuscript.
Fig. 16: Show the crust-mantle boundary in all panels. It would help to understand the crustal evolution.	The reconstruction of the Moho in all different stages is difficult since we have neither constraints on the original thickness of the Ontong Java Nui nor on the nature of its crust-mantle boundary. Showing speculative crust-mantle boundaries in every panel is not very useful in our opinion.
Table 1 (Line 888): Change “kg/m2” by “kg/m3”	This spelling mistake has been corrected.
Line 125/Fig.1: “Drilling at Deep Sea Drilling Project (DSDP). . ..” Please show the DSDP location in Figure 1.	The DSDP borehole location is now visible as the orange star in Figure 1.
Fig. 1 (line 896): Change “refraction” by “reflection”	This spelling mistake has been corrected.
Fig. 4b (middle panel): There is something wrong when plotting the synthetic travel times between Plc and Puc of the left wing of the OBS. Please correct that.	The plotting error has been corrected.
Fig. 5b (middle panel): Please plot the record from 2 s to 7s.	The plot has been modified accordingly.
Fig.6, 7, 8, 9, 10 (models): Black the stars of the malfunctioning stations	Malfunctioning stations are now marked as black stars in all figures.
Fig.11 and 12 (upper panel): Show the RMS value for this gravity fit.	The RMS value is added in the caption.