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## ***Interactive comment on “The diverse crustal structure and magmatic evolution of the Manihiki Plateau, central Pacific” by K. Hochmuth et al.***

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General comments: This study provides valuable information on the crustal structure and geodynamic evolution of the Manihiki Plateau, and also enhances our understanding on Large Igneous Provinces (LIP). Using wide-angle seismic (WAS) data is certainly the best option to address the main goal of the manuscript. The good quality of the data allows to identify rather well the different seismic phases. 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” -. How-

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ever, 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 with 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 assessment 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), 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). In any event, I encourage the authors to consider these reviews and present a new version of the manuscript after major revisions for another peer review.

Specific comments (Lines in the word file): Line 176 to 180: There was no processing after relocalization of OBS/H? If there was any, mention it here (Band pass filtering, deconvolution. . .). Line 179: "We assigned picking uncertainty. . ." Be more specific; how much it was? It was the same uncertainty for all phases? 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

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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 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. 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 grey area is poorly visible and leads to confusion. 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. Line 205 to 206: “We calculated the Poisson’s ratio...” Specify that this calculation is only done along transect AWI-201202200. Why did not you present the Poisson’s calculation of the other line? 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. Line 219: Specify that the P-wave velocity-density conversion using Hamilton (1978) is only applied for the sediment cover velocities. 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. Lines 241 to 242: “Bathymetric....faults and grabens”. Where are these Bathymetric and seismic reflection data? Reference please. Line

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249: “Seismic reflection data reveal” Again the authors talk about seismic reflection data that are neither referenced nor presented. Please add some reference. If the seismic reflection data are not published yet add the reference of the cruise report. Line 273: “S-wave velocities show a block-like structure” What does it mean? Line 295 to 297: “the Penrhyn Basin.....are typical for serpentized 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 serpentized crust in his gravity modeling, but they based this assumption in few serpentized 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 serpentized crust to a basaltic crust. However, I agree with the fact that the uppermost basaltic crust might be partially serpentized, but I would not emphasize that in the figure but in the text with a very short line. 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. Line 345 to 347: “The boundary between. . .as well as reflections at 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. 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. Line 501: Change “Gravity anomalies are mainly attributed” for “Short wavelength gravity anomalies are mainly attributed”. Line 594 to 597: “On the Western Plateaus, faults. . .along with

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further sedimentation”. I do not see how faults reaching the basement indicate that stretching was coetaneous to second stage of volcanism. 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? Fig. 16: Show the crust-mantle boundary in all panels. It would help to understand the crustal evolution.

Technical corrections: Table 1 (Line 888): Change “kg/m<sup>2</sup>” by “kg/m<sup>3</sup>” Line 125/Fig.1: “Drilling at Deep Sea Drilling Project (DSDP). . .” Please show the DSDP location in Figure 1. Fig. 1 (line 896): Change “refraction” by “reflection” 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. Fig. 5b (middle panel): Please plot the record from 2 s to 7s. Fig.6, 7, 8, 9, 10 (models): Black the stars of the malfunctioning stations Fig.11 and 12 (upper panel): Show the RMS value for this gravity fit.

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Interactive comment on Solid Earth Discuss., 6, 1863, 2014.

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