Forum Reply

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Reactivated lithospheric-scale discontinuities localize dynamic uplift of the Moroccan Atlas Mountains

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We thank Teixell et al. (2014) for their Comment and welcome this opportunity to discuss the related issues further. In general, we do not take issue with most of the issues that were raised. Instead, we feel that we had already acknowledged most of their concerns in the original paper, to the extent that a short format allows. Also, none of the issues raised appear to speak to the robustness of our main findings: a trend of shear wave splitting delay times that corresponds to topography and topographic anomalies across the Atlas Mountains, and multiple consistent steps in the Moho and lithosphere-asthenosphere boundary (LAB) that are aligned with surface geologic features—the South Atlas fault and location of recent volcanism in the Middle Atlas.

Speaking to their main points, we first note that the main text had already acknowledged that the South Atlas fault (SAF) was inferred to be a shallow dipping thrust fault based on active source studies and structural geology mapping (Miller and Becker, 2014, and references therein). Beyond that, we are not aware of other imaging efforts which would show the downward continuation of the fault to depths much deeper than the Moho, besides other recent receiver function studies. In further support of our interpretation, independent tomography studies do show an abrupt change in seismic velocity across this part of the Atlas at lithospheric depths (Bezada et al., 2014; Palomeras et al., 2014). We also note that we do not provide a dip estimate for the inferred fault or step (Miller and Becker, 2014, their figure 2); receiver functions and our station spacing do not provide the necessary resolution to resolve fault steepness. However, the step does occur between the two stations that are on either side of the SAF, which are located ~28 km apart (Miller and Becker, 2014, their figure 1).

Whatever the continuation of the SAF and the mechanical workings of the crust and lithosphere, the observation of a consistent step in both Moho and LAB still stands. We still find this observation highly suggestive of strain-localization within the lithosphere, perhaps guided by a reactivated lithospheric zone of weakness, or localized at the edges of a delaminated lithosphere (e.g., Bezada et al., 2014).

As for the interpretation of lithospheric structure in terms of isostatic computations we note, second, that earlier efforts on interpreting Atlas topography based on more restricted datasets, and the well-known ambiguities of such estimates, were already extensively acknowledged in the main text (Miller and Becker, 2014, and references therin). There are indeed additional, appropriate references, but those reference earlier models (e.g. Zeyen et al., 2005; Ayarza et al., 2005; Fullea et al., 2007; Babault et al., 2008; Jimenez-Munt et al., 2011). Our computations are motivated by new constraints from our passive source imaging efforts. We use fixed crustal and lithospheric values and adjust asthenospheric density optimized to fit the regional topography (Becker et al., 2014). With these values and our layer thickness estimates, we find that the lithospheric thickness variations that would nullify the observed anomalous topography from crustal isostasy are too large to be compatible with our LAB estimates. This substantiates that anomalous topography in the Atlas arises by means of an active mantle upwelling, such as that expected based on the Moho/mantle temperature anomaly map of Fullea et al. (2010), or the plume inflow hypothesis of Anguita and Hernan (2000) and Duggen et al. (2009). Moreover, the anomalous shear wave splitting signal which leads us to infer channelized flow as guided by the lithospheric offsets clearly underlies the actual topography peak, as well as the freeair-inferred anomalous topography (Miller and Becker, 2014, their figure 3), which is independent of crustal or lithospheric layer estimates. Given these findings, and the fact that we do not image anomalously thickened crust, it seems clear that there is a residual topography signal that is aligned with the localized seismic anisotropy. Detailed modeling of how contributions to topography partition into lithospheric thickness or density variations and active mantle upwellings will be the subject of future study that is now possible with better seismological constraints on the deep mantle (e.g. Palomeras et al., 2014; Bezada et al., 2014).

Third, Teixell et al. take issue with referencing and we regret that some of the subtleties of the historical development of the arguments were lost by brevity and limits on the number of citations. However, we note that we had already cknowledged all of the core models which had been developed prior to our study (Miller and Becker, 2014, and references therein). Fullea et al. (2010) is cited in the Figure 1 caption because the location of the Quaternary basalts in the figure was digitized from their Figure1, but throughout the text we correctly cite the references for Quaternary volcanism with Anguita and Hernan (2000) and Duggen et al. (2009). The Sebrier et al. (2006) is a typo as it should be Seber et al. (1996). The references for shortening in the Atlas should have been Brede et al. (1992), Beauchamp et al. (1999), Gomez et al. (1998), and Teixell et al. (2003) and resulted from a clerical error. These are cited elsewhere within the text and listed in the references. We much appreciate the efforts of Teixell et al. to set the record straight and apologize for this mishap.

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