

We thank all the reviewers and editor for their time and input into our manuscript. We feel that their work has helped us significantly improve the clarity of the text. In this draft we have addressed all the minor technical corrections suggested by both sets of reviewers. Regarding the more substantive comments from Referee #1 we have amended our text some-what and our more detailed and specific response is below. Reviewers' comments in blue, our response in black.

#### Referee #1

In the manuscript, the regional field is attributed to isostatic compensation, where a shallower Moho beneath deep bathymetry produces a positive gravity anomaly that offsets the negative free-air anomaly associated with the bathymetry itself. To estimate this regional field, the authors compute the gravity effect of an Airy-style isostatic Moho model. Unlike a conventional Airy isostatic correction, however, the Moho geometry is not derived from topography. Instead, the free-air anomaly is first converted to an equivalent topography using the Bouguer slab approximation, from which the Airy Moho and its gravity effect are calculated.

From my understanding, this procedure is conceptually very similar to an upward continuation of the original free-air anomaly. The free-air anomaly is converted to an equivalent topographic surface, which is then effectively shifted downward (away from the observation level) by 30 km before its gravity effect is calculated. This appears analogous to upward continuing the original free-air anomaly by approximately the same distance. Because the method does not use topography to derive the isostatic model, it seems somewhat different from what is typically considered an isostatic correction. Instead, it appears more closely related to a novel upward-continuation or regional-field estimation scheme.

I do not think the terminology necessarily needs to be changed throughout the manuscript or title. However, I suggest adding a short discussion clarifying this distinction. Framing the method as a "pseudo-isostatic correction" or an alternative approach for estimating the regional gravity field would avoid the need to justify the assumption of Airy-type isostasy. This assumption of Airy-type isostasy is very hard to justify for the topographic features in your study area, which have wavelengths of ~50 km, generally considered too short to be compensated by Airy-type isostasy. I believe this slight reframing from an isostatic correction to a novel method of estimating the regional field would improve both the clarity and impact of the paper.

We thank the reviewers for their comments. We agree that framing the method as a "pseudo-isostatic correction" is a good suggestion as the free air gravity anomaly is the source of the topography used to calculate the theoretical Moho, rather than observed topography in a standard model. However, the (Airy) isostatic consideration is a critical one. The method is not simply an upward continuation of the free air anomaly to isolate the regional field. It requires the isostatic assumption to invert the sign, so basins (negative free air anomaly) give a positive Moho gravity signature, which when subtracted from the free air anomaly increases the anomaly amplitude, hence increasing the depth of the recovered basin.

Regarding the point that topographic feature in the area have a wavelength of ~50 km, generally considered too short to be compensated by Airy-type isostasy. It is possible that other forms of compensation are present, such as locally dense crust/mantle, giving rise to Pratt type isostatic balance. However, the gravitational impact of such compensation will likely be in-line with that predicted by an Airy model (depending on where in the crust or mantle the dense material lay).

We do note that the Airy isostatic model does appear to explain relatively well the discrepancy between bathymetry derived from the observed free air anomaly and the observed bathymetry. If basin development was largely due to erosion this would point to an Airy-like response, even at the relatively short wavelengths we are considering. Finally, other topographic features on the order of 50 km across, such as Ross Island, have been shown to induce flexural responses at close to Airy (Te 2-5 km) Aitken et al 2012, in West Antarctic crust. Bathymetric depressions on a similar scale, would be expected to behave in a similar way.

Line 234: This sentence about the water layer seems out of place or lacks some explanation. Is the water layer between 0 and the topography from the isostatically adjusted FAA, assuming there is no ice anywhere? If so, this gives almost 2km of water in places. Is it modelled with prisms of density of 1028 kg/m<sup>3</sup>? Is this water gravity effect then subtracted from the isostatic adjusted FAA before comparison with the forward gravity effect of the topography model? Depicting this water calculation step in Figures 4 and S1 might help clarify.

This sentence has been re-written. We used an approach like the Discretization of Tankersley et al. (their Fig 1h), where the gravity effect of water displacing rock below the ellipsoid is modelled with prisms with density of  $\rho_{\text{water}} - \rho_{\text{rock}}$ . In our case we used a fixed level of 500m in place of the ellipsoid and assume all material below this is water displacing rock. This in effect gives the gravity effect of the water/rock boundary as the top of the prism layer is flat.

Line 235: Can you clarify what specifically 'this procedure' that is iterated three times refers to? Is it the calculation of the gravity effect of the water layer.

The reviewer is correct - 'this procedure' refers to the prism based forward model and update of the topographic surface. The sentence has been amended.

Figure S1: Can you add in where the 3 iterations and incorporation of the gravity effect of water described on Line 235 occur?

The figure has been amended as suggested.

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