

We thank all the reviewers and editor for their time and comments on our work – they are useful and highlight sections/assumptions which have not been adequately described. Our response is below and our manuscript has been updated in-line with our responses. Our line numbers in this document refer to the marked-up revised text.

Reviewer 1

General comments:

This work provides a novel addition to the field of sub-ice shelf bathymetry modelling. It addresses the major limitation of this technique, the effective removal of the often long-wavelength signal associated with crustal gravity effects. I think the paper can be improved with a better explanation or justification of some of the methods, and some more discussion added about the assumptions made and their limitations.

Calculation of the isostatic Moho: I have a few reservations about the methods used to calculate the isostatic Moho model. I'm not entirely sure why the free-air gravity was used with a Bouguer slab correction to define the topography used in the isostatic calculation. Why not just use an initial (interpolated) topography model like BedMap2? I assume the differences would be small, but you would be removing what seems to be the most tenuous step in this process.

We agree with the reviewer that the initial isostatic calculation is the critical and novel step.

Use of the free air anomaly as an input is a deliberate and important choice, especially when considering the alternatives. We now more fully justify this in the revised text (L219-220).

The main reason for using the free air anomaly as a starting point for the isostatic model is that it contains the signature of the bathymetry, although distorted by an isostatic signal. Therefore, using the free air as an initial proxy for bathymetry/topography in the isostatic calculation includes the 'real' bathymetric pattern.

Using an initial interpolated topography model like Bedmap2 includes no information about the bathymetry, therefore can-not be used to robustly predict the pattern of associated isostatic compensation. In-fact BEDMAP2 is a case in point in the Dotson-Crosson area. As there was no knowledge of bathymetry the floor of the basin was simply assumed to be ~100 m below the base of the floating ice shelf. This makes the BEDMAP2 bathymetry beneath Dotson/Crosson even shallower than the surrounding ocean. This is opposite to what we know to be true from both the gravity anomaly data, and the direct observations.

If you retain the Bouguer slab correction, it would be good to have some more clarity on how you calculated the topography from the Bouguer slab, as I don't follow how you accounted for locations where the topography is a density contrast between rock and water, rock and ice, or rock and air.

We acknowledge that this was un-clear and have now clarified the description of our use of the Bouguer slab formula (L221 to 228). For simplicity we assumed a slab of rock displacing water when calculating the initial bathymetry from the free air anomaly using the Bouguer slab formula. This assumption is correct for the ice shelf regions which are the focus of this study. Away from marine areas the recovered topographic amplitude will be over-estimated, by ~6% in ice covered areas and ~40% in ice free areas. As bathymetry/topography away from the ice shelf regions is not a focus of the study we chose to ignore this error. We acknowledge that ignoring

the correct factor for calculating the amplitude of the topography in surrounding regions may distort the amplitude of the isostatic correction in the marine areas. However, there should not be a significant shift in the pattern of the isostatic model from our assumption. As the amplitude of the isostatic correction is further adjusted by subsequent assumptions to give a model which fitted our new observational data we believe that overall our assumptions are valid.

As the isostatic gravity correction is the main novel concept in this paper, I think a slightly more thorough discussion of the assumptions is required so future users of the method understand the limitations. For example, how does the assumption of no lateral strength affect the results? With some lateral strength (as expected for a small study region), the Moho model would likely be more subdued, and therefore less of the shorter-wavelength gravity signal would be accounted for by the Moho model, meaning the inverted bathymetry model may contain more short-to-mid wavelength features than if no lateral strength is assumed.

We agree with the reviewer on the point that the isostatic correction is the key novel part and will update the main text to clarify the method, specifically, regarding the amount of lateral strength of the lithosphere when considering the isostatic model (L206-209). The reviewer is correct that a more rigid lithosphere would smooth the response at the Moho. However, in the application of our correction the isostatic effect is subtracted, adding signal (negative amplitude), which increases the depth of the recovered basin. As rigidity increases the isostatic signal is spread over a broader region, removing shorter wavelengths from the correction. The Airy model is therefore the one which provides the greatest possible short wavelength adjustment, and hence the deepest basin.

If your ice shelf basin contained some sediments, how much would this affect the isostatic Moho and the resulting bathymetry?

Sediments within basins will always be an issue for gravity inversion of bathymetry. We now note this issue in our discussion (L353-361). In summary – sediments are low density, so would tend to give a negative gravity signal and an over-estimate in depth. We see the opposite with depth underestimated, and the maximum isostatic correction required to give a good fit, suggesting low density sediments are not a major issue. Also the presence of eroded bedrock in many inshore areas of the Amundsen Sea points towards erosion dominated regime, lacking thick low density sediments.

Gravity processing: It would be nice to have some more details on the gravity reduction. Did you remove the effects of the ice and water loads from the free-air gravity before the inversion, or before the preliminary topography estimate? It would be helpful to understand how the method works to include more figures of the intermediate steps, mainly the isostatic gravity anomaly, but maybe also the preliminary topography and the initial gravity-derived topography. For the various Bouguer calculations, did you use a density of 2670, and if so, why?

As suggested, we have now included a supplementary figure showing the intermediate steps in recovery of bathymetry from gravity data to guide the reader. This includes the isostatic correction and implementation of the topographic correction field.

Regarding gravity processing: We work directly with the free-air gravity anomaly, without consideration of ice or water loads prior to the inversion. The production of the free air anomaly is now clarified in the text (L129 to 135).

We choose not to correct for ice/water loads prior to inversion as these are in effect the unknown parameters we are trying to recover. Inverting directly from the free air anomaly therefore minimises any corrections which may have been done with inaccurate or incomplete topographic/bathymetric data. We note many authors do use topography/bathymetry as a prior observation, and then refine the bathymetric surface. However, our approach is to create a 'pure' gravity derived surface, then adjust it to fit available topographic data.

For density we use 2670 kgm^{-3} for rock and 1028 kgm^{-3} for water. The density used is the 'standard' value for rock used in the Bouguer correction based on global averages for typical topography. Choice of the density value will directly impact the amplitude of the recovered bathymetry, and this remains an uncertainty in the method. However, use of non-standard density values would also require justification. We now clarify that in the case of the Dotson-Crosson region the exposed geology on Bear Peninsula which reveals metamorphic and igneous rocks point towards typical densities of 2670 kgm^{-3} .

Specific comments:

Line 39: bathymetry only dominates for the free-air anomaly, so I would either change gravity to free-air gravity or change dominates to contains.

Amended

Line 64 and 68: with "initial" clarify if you are talking about past published inversion (and which ones, Jordan et al 2020?) or the first attempt of your inversion in this paper.

We are referring here to the previous work presented in Jordan et al 2020 and have clarified this point (initial becomes previous).

Line 70: possible rewording: which is changing the observed gravity -> which is a component of the observed gravity

Amended.

Line 71: reword for clarity: The underestimate of the bathymetry suggests that the expected negative gravity anomaly associated with deep bathymetry is being offset by something causing a positive gravity anomaly.

Re-worded.

Fig 3: can you include flight lines over the gravity data? For the gravity, since it is a free air anomaly, the sign is important. Could you use a linear diverging colormap (i.e., blue to red) that is centered on 0 and has the same positive and negative limits (-70 to +70 mGal) just like the colormap in Fig 2a. I'd also suggest a linear colormap for the topography in b, the current

colormap exaggerates the difference between 0 and 100 m, and 0 m is relatively meaningless in Antarctica since neither groundingline or coastline is at 0 m.

We have added the flight lines of the used gravity data to Fig. 3a.

We have adjusted the colour scale for the free air gravity anomaly as suggested by the reviewers.

We have retained the colour scale on the topography image. This colour bar is derived from a standard GMT colour bar optimised for display of bathymetry and topography datasets when split linearly between its end points. While the scale it is not strictly linear it has some distinct advantages. First it conveniently visually breaks the bathymetry into three broad regions, deep (~ -1000 purple, intermediate dark blue, and shallow light blue ~ -500), without using red/green distinction. Secondly it places a distinct colour switch at zero. Although questioned by the reviewers, we feel this is a useful distinction as it separates areas which cannot easily un-ground from those vulnerable to marine incursion. We agree the current coast/grounding line will often ignore this specific depth contour, but we feel it is an important distinction when considering the general pattern of past/future ice sheet evolution and vulnerability.

Line 103: It would be good to have additional details on the gravity processing. What was the general flight line spacing, speed, altitude etc. Cross over errors? Levelling procedure. Was the free-air anomaly corrected for the gravity effects of the ice sheet or the water thickness? In typical inversions, the water layer is included in the initial model and therefore accounted for. But with your technique, I don't see where the gravity effect of the water column is accounted for.

We have updated our description of the gravity data collection and processing (L129-134).

Regarding subsequent correction for ice sheet or water/ice thickness. We chose not to conduct any correction for water or ice thickness prior to inversion. These values (bathymetric depth) are the key unknown values in this region and the aim of the inversion is to constrain these. In our inversion we consider the full free air anomaly is due to an interface between rock and water. The full water column thickness is therefore implicitly included in the inversion.

Line 147: Maybe replace reference elevation model with starting elevation model for clarity.

We consider “reference elevation” is the correct term. We do not adjust these elevations, as they are derived from direct observations. The gravity derived surface is ‘twisted’ to fit this reference data.

Line 165: For such a small region it is harder to justify assuming 0 lateral strength in the lithosphere. Given some lateral strength is expected, this would essentially smooth out your isostatic Moho. A smooth Moho would account for less short wavelength gravity variations, leaving those shortwavelength anomalies within your signal used in the inversion. Therefore a smoother Moho will result in more variation in your topography and vice versa. Due to this, your assumption about lateral strength has a direct effect on the amplitude of your bathymetry variation. By assuming no strength, you are putting much more of the short-wavelength variation

into Moho, and therefore less into the bathymetry. While I agree it is a fine assumption, I think this aspect of it is important for you to state.

We agree that, increasing crustal rigidity would flatten the predicted Moho. However, increasing rigidity (flattening the Moho) will not increase the amplitude of the recovered bathymetry. Because the isostatic gravity model (positive anomaly where the Moho is upwarped) is subtracted from the free air anomaly the higher the amplitude of the isostatic effect beneath the basin the more negative the corrected gravity anomaly will become, and hence the deeper the predicted depth of the basin will be. At the most extreme for an infinitely rigid plate there would be no isostatic correction and the result would match that of Jordan et al 2020, which underestimates the real bathymetry by ~400 m.

We have clarified the text on this point so it is clear that Airy (zero strength) model will give the maximum depth (L206-209).

Line 173: Please describe how you did the Bouguer slab transformation. Did you take free air gravity (not corrected for ice or water) and divided it by $(2\pi G (2670 \text{ (crust)} - 1 \text{ (air)}))$. Wouldn't you need to use a spatially variable density contrast, where it is 2670 - 1 on land, and 2670-1040 for regions of topography < 0. By using density of air instead of water, you are increasing the density contrast, which should result in more subdued topography, not amplified as I think you stated. Where did you get 1.6 from?

The reviewer is correct on this point - for the Bouguer slab calculation we used the effective contrast of rock against water, as this is the density across the boundary we are trying to recover. We now better acknowledge in the text that this distorts the predicted topographic amplitude into onshore areas, but as these are not the focus of the study, we consider it a reasonable simplification (L225-228).

We agree with the reviewer that the amplification of the isostatic correction is not well explained (this point was also raised by reviewer 2). The preliminary topographic estimate is calculated using a density of rock against water. This maximises the amplitude of the topography/bathymetry. However, we know this topographic amplitude is an underestimate (from observations). Calculating the isostatic correction from topography with an underestimated amplitude will also give an incorrect (too small) correction. To amplify the isostatic effect of the topography we arbitrarily assume the topography recovered above is displacing air rather than water for the isostatic calculation. This amplifies the recovered Moho and associated correction by ~1.6 times. We chose this adjustment as it provided recovered bathymetry which fitted the observed data well. However, we acknowledge that the scaling of the isostatic correction is an assumption which may well be location dependent. This is now laid out in the revised text (L230-233).

Line 174: The max underestimate is ~400 m, and only in the ice shelf region, as other regions are much better constrained. So does a maximum of 400 m really justify creating a new topography from the Bouguer slab equation?

The fact that the bathymetry shows such a significant and one-sided error supports re-calculation of the bathymetry.

Fig. 4: replace isostatic moho gravity anomaly with isostatic moho gravity effect.

Revised.

Fig. 7: for b, you should use the same magnitude of upper and lower limits for the colorbar, and currently it distorts it to make it seem like the grid is dominated by negative values.

We thank the reviewer for catching this error - the negative part of the colour bar was mislabelled – the colour-bar is symmetrical about zero between -400 and 400. We note the errors are dominated by negative values because isostatic compensation was not considered in the original Jordan et al 2020 model and the pattern of isostatic compensation in the region is complex. It is interesting to note that where isostatic pattern is simpler adjusting the bathymetry based on observations alone works well.

Line 284: Change from Moho gravity anomaly to Moho gravity effect, or isostatic gravity effect.

Revised.

Technical corrections:

We will update the revised paper in-line with these technical suggestions.

Ensure consistent use of either sub ice shelf or sub-ice-shelf in title and text

Line 25: add word "bathymetry": Using new direct bathymetry observation ... to clarify.

Line 75: by basin do you mean ice shelf region?

Line 94: add 'past' before gravity-derived to clarify these are not the inversion results from this paper.

Line 170: Mantle should be lowercase

Reviewer 2

The paper presents a case study on some of the currently most-discussed issues in Antarctic sciences – how to determine sub-glacial / ocean bottom topography especially in the transition zone from the Antarctic continent to the ocean. Reliable and precise knowledge of this topography has utmost importance for a number of questions, such as for the control of ocean water inflow beneath ice shelves (as the authors stated) but also, for example, for the definition of fluxgates for the determination of the ice-mass loss across the grounding line (in the input-output method).

The authors present a method to improve estimates of ocean-bottom topography beneath ice shelves described in an earlier paper (Jordan et al. 2020). They show that the inversion results can be improved considering isostatic compensation which accounts for larger amplitudes as well as for larger variations (gradients) in the working area. In this way, long-wave errors can be mitigated. As such, the paper provides a valuable method to re-assess and improve ocean-

bottom topography resulting from gravity inversion (where no direct measurements are available).

However, there are a number of substantial issues such that I suggest a major revision of the manuscript.

First of all, the paper should be fully self-explanatory without the need to read previous paper(s) of the same author(s), which it isn't. Of course, it should not be necessary to repeat entire sections of previous publications, but a short and concise description would be advisable where needed. For instance, it is not clear, how the free-air gravity anomalies were formed and which surface they refer to.

We agree with the reviewer that more information about the gravity dataset should be provided – this has been updated in the revised methods section (L129-L135). We have also updated the description of the terrain adjustment method for recovering bathymetry, so the reader does not need to refer to the earlier paper to understand this one (L240-L255).

Second, in my view the paper is not that well written. It lacks thoroughness in many formulations, and consistency in using special terms. For instance, the usage of the terms “error”, “accuracy” and “uncertainty” seems to be arbitrarily.

We have endeavoured to tighten the use of terminology, especially around errors/uncertainties as suggested. We note, however, that in some cases, such as the horizontal location of sub-shelf sensors, there is ambiguity in the quantification of the error, so it remains challenging to precisely describe the errors in more than colloquial terms.

Third, to me it is surprising that there isn't any formula given at all. I think it would be much easier to explain and clarify relations when respective formulas are used.

We have now added equations where they add clarity to the process we applied.

Fourth, I miss an overall discussion of the state of the art, how the presented investigation fits to current research and results obtained by other authors. The resulting product could be compared to available products on sub-glacial and ocean bottom topography to assess similarities and discrepancies, e.g. Pritchard et al. 2025 (BedMap3), Morlighem et al. 2025 (BedMachine Antarctica, version 4), Ockenden et al. 2026 (subglacial topography inferred from ice flow perturbation analysis) or Charrassin et al. 2025.

We now better discuss our work in the context of other bathymetric inversions in the introduction (L45-61).

We have also followed the reviewers suggestion of comparing our results with previous work in the Dotson/Crosson region such as Bedmachine 1, 3 and 4, as well as Charrassin et al. 2025, and bedmap2 and 3 (L308-324). In some cases the difference is clear, however, for some data products Bedmachine 3, 4, Charrassin et al. 2025 and bedmap3 the depths are identical, and the source of the bathymetry is not well described. Looking at the data it appears to be an adjusted version of Millan et al. 2017 gravity derived product. What our comparison shows is that the isostatic method does indeed appear to be a better product in this region (but this may in part be a product of improved gravity coverage between 2017 and 2019 surveys).

The other suggested dataset for comparison of Ockenden et al. 2026 is only reported over grounded ice. Therefore comparison over the ice shelves where gravity inversion is relevant is not applicable.

In this way, also the structure of the paper needs to be improved. This refers especially to Section 1 “Introduction”. There, some general background is given only in the first short paragraph, whereas the largest part deals with the discussion of the results inferred so far by the same author (Jordan et al. 2020) and the discrepancies detected when new direct observational data have become available. This discussion should not be part of an introduction.

As suggested we have expanded our discussion of previous work on recovery of sub-shelf bathymetry in the introduction.

We feel that discussion of the discrepancy between new observations and our original inversion is a key and important foundation to our new study. We have therefore retained this, but split it out as a separate sub-section in the introduction for clarity.

A further issue I would like to discuss is the usage of the term “bathymetry”. I know that it is widely used as synonym for “ocean-bottom topography”. In my opinion, however, “bathymetry” refers to the measurements, i.e. to the applied methods/techniques to measure ocean-bottom topography – such as “gravimetry” refers to measurement techniques to observe gravity or gravity differences. Terms such as “ocean-bottom topography” or “seafloor topography” should be preferred.

We understand this point – however, we feel that as a synonym for ocean-bottom topography “bathymetry” is a reasonable and well understood term. We now clarify the definition at the very start of the paper.

Detailed remarks

- Please check orthography to be consistent throughout the paper – e.g. “sub ice shelf bathymetry” in the title, “sub-ice-shelf bathymetry” at line 22; “free air gravity anomaly” vs. “free-air gravity anomaly”, etc.

Amend as suggested

- Line 24: “bathymetry and topography” – see my remark above; clarify if you mean both seafloor and/or land surface topography and/or subglacial topography.

bathymetry is now explicitly defined as “ocean-bottom topography” in earlier sentences.

- Line 73: Which “gravity anomaly”? Be consistent to use the correct term(s) throughout the manuscript.

Amended to free air anomaly.

- Line 80/81: “mostly likely” – please correct.

Revised

- Line 94 (Fig. 2): Subfig. 2a presents a difference of the previous and the new ocean-bottom topography. Please state, how the difference was formed (w.r.t. A minus B). Moreover, I wouldn’t call it “error” – if you use the term “error” it implies that one of the datasets represents the truth which I doubt is the case. The same holds true for Subfigs. 2b and 2c. (difference vs. error). Please name the abscissa axes in both subfigures (e.g. “profile length”).

The difference in this figure was calculated as gravity derived topography (Jordan et al 2020) minus interpolated direct observations, including new sub ice shelf data – this is now updated.

We retain the term error as we are assuming that the direct observations are the ‘true’ bathymetry. There are important uncertainties in the direct measurements which are discussed, but overall we consider them to be the ‘truth’ against which the gravity inversion can be tested and errors assessed.

- Line 116: What does “positional accuracy” mean? Is it 3D or 2D (horizontal position)? Again, see remark on the usage of the terms “accuracy” and “error”. Maybe you mean “precision” in this case? “Relative errors” (or precision) should not have a unit (it can be given as percentage). Where are the values (“<40 m”) coming from?

Positional accuracy in this context is horizontal accuracy. The estimate of error is based on comparing different swath’s and assessing the offset. This is now clarified in the text.

- Lines 120-122: Term “accuracy”? What do you like to state with “the horizontal accuracy is relatively high (<1 km) compared with the resolution of the gravity data (> 5 km)”?

As stated in the paper it is hard to define the absolute accuracy of the position of the AUV. However, features on the in-bound and out-bound tracks appear in approximately the same locations, i.e. a peak seen in position x on the way in is <1 km of the same position on the way out. As we are considering this data as a constraint on the gravity inversion which has a wavelength resolution of 5 km or longer means that the uncertainty in the direct observations in bathymetry from Autosub is not really a major factor.

- Line 128: “Uncertainty”? (see remark above)

Revised

- Line 146: Where is this value (37.2 m) coming from? How was it calculated?

This value is taken from the metadata associated with the published seismic record. The estimate of the seismic error was taken from the difference between depths of seismic observations and two direct soundings of water depth through boreholes. The chosen value is the higher of the two errors, providing a more conservative estimation of error.

- Line 147: Here you introduce the yet new term “elevation”? Please clarify – are “surface elevation” and “ocean-bottom topography” compiled into one dataset? If done so, why does it make sense? How was the conversion from raster to point data done?

It is the topography where the ice is grounded (or absent above sea level) and swath derived bathymetry offshore. This is the fundamental ‘top of rock’ surface we are trying to resolve to input into ice sheet/oceanic models.

The existing raster from Jordan et al 2020 was converted to point data by assigning the XYZ value of every filled cell to a point value. Where no data existed no point data was created.

- Line 150: How was the “weighted mean” calculated?

This was calculated in GMT using the function blockmean (with variable weights depending on the data source). This is a standard function. The methods section is updated to note this (L180-188).

- Lines 165...: Please increase clarity when discussing the application of the isostatic correction. Do you imply isostatic compensation in this region, such that the measured gravity does not contain sufficient signal generated by topography? How do you come to the assumption to use the density of air (hence ~ 0) instead of water – is this just “try and error”? What term is really calculated in the end – a correction due to the Airy isostatic model, an “isostatic Moho gravity effect”, ...? (Correction and effect should normally differ by the sign, a correction being the negative of an effect.)

The reviewer is totally correct, we imply isostatic compensation in this region, such that the measured gravity does not contain sufficient signal generated by topography.

The assumption to use the displacement of air is the result of a try it and see approach, which is now clarified in the text. However, if we had not used this approach we would have had to amplify the amplitude of the Moho (and its gravity effect) arbitrarily by another means.

Our final calculation is the Moho gravity effect – this is subtracted from the free air anomaly, to reveal the true amplitude of the gravity anomaly associated with the water filled basin. We now include a supplementary figure showing all the fields as they are applied across the processing flow.

- Line 173-174: What do you mean with “...a simple Bouguer slab transformation of the observed free air gravity anomaly to elevation”? Please clarify.

In this case we use $h = g / 2\pi G(\rho_{\text{rock}} - \rho_{\text{sea water}})$

- Line 171: correct “O’Donnell et al., 2019” (capital D).

Updated

- Line 177...: Why can you use Fourier Transform to calculate the isostatic correction?

A fft can be used as it assumes the source of the anomaly is from a single interface at depth with a uniform density contrast across it (Parker 1973). The gravfft module of GMT combines both the calculation of the isostatic surface and the resulting gravity anomaly. In this study we assign a crustal rigidity of zero, but we could assume a more rigid crust if we thought that was appropriate – this flexural surface can also be calculated using fft methods. In this location a weak (Airy) model seems to work ok and is geologically appropriate.

- Line 180: How was the inversion done? A short paragraph should be sufficient, where you may refer to the previous paper for details.

As suggested the description of the inversion method is now included in our updated methods section (L240-253).

- Line 210 / Fig. 5: In the caption, it is stated that “sub-shelf bathymetry” is shown. However, what about the land-based ice regions?

The elevations in land-based regions are derived from radar observations (via the topographic adjustment) – this is now clarified in the figure caption. This provides a smooth transition from radar/swath derived elevations outside the ice shelf to gravity derived elevations within. Total coverage in the figures is limited to areas where there is also gravity data.

- Line 230 / Fig. 6: Again, instead of “error” I would use the term “difference” (and which quantity has been subtracted from which?). Explain what the lines and yellow triangles denote.

We retain error in this case as we are comparing ‘true’ direct observations with the gravity derived model. Line work/symbols are from figure 1 denoting observations. This is now clarified in the caption.

- Line 233: correct “in the and Crosson”.

Corrected

- Line 242: What do you understand by “model error” and “standard deviation” in this context? Where is the value “90 m” coming from?

This is the standard deviation of the error between our new recovered bathymetry (isostatic correction but no sub-shelf control points) and direct observations made by ALR (correct value is 70, not 90m). This provides what we consider to be the best estimate for the expected error associated with gravity derived topography in this region.

- Line 243: Correct “un-resolved” --> “unresolved”, and “short wavelength bathymetric features” and “... topography” --> “short-wavelength” (with hyphen).

Updated

- Line 244: What do you mean by “intrinsic errors in the gravity observations”? What uncertainty measure are they given?

These are the errors in the gravity observations (estimated to be 1.67 mGal based on crossover analysis of Operation Ice Bridge data). Text is now updated to clarify this point (L344-347).

- Line 245: Correct “may-not” --> “may not”

Corrected.

- Line 246: Correct “Tankersley et al. 2025” --> “Tankersley et al. (2025)”, same in line 273

Corrected

- Line 249: Correct “un-known” --> “unknown”

Corrected

- Line 280 / Fig. 7: Again, a new naming “Moho gravity anomaly”??

The Moho Gravity anomaly is now explicitly defined as “The gravity anomaly associated with the compensating Moho surface, i.e. the Moho gravity anomaly (g_{moho}),” in the methods section.

- Line 289: Correct “Free air gravity anomaly” --> “free-air gravity anomaly”.

Corrected throughout paper.