Responses to reviewers in bold

Reviewer comments in blue

Round 2 review:

Reviewer 1: Katharina Hochmuth

I like to congratulate the authors on a great review. I have no further comments and recommend a swift publication.

Best regards, Katharina Hochmuth

Reviewer 1: Guy Paxman

I was previously reviewer 2. I want to reiterate that this is an interesting and well-constructed manuscript that makes a valuable contribution towards our understanding of sub-Ross Ice Shelf geology, and is likely to be highly cited as a result. The authors have made a substantial effort in revising the manuscript, and there are many tangible improvements to the text and figures. I have a few final suggestions / questions that might help further clarify parts of the manuscript. These are largely minor points, and once addressed the manuscript will be suitable for publication.

Guy Paxman, Durham University

Manuscript comments

An overarching comment is that, while the data, methods, and results are well described over the main manuscript and the supplement, the authors may wish to consider whether some more key points could be shifted from the supplement to the main manuscript to aid the 'casual' reader who may not wish to dive too deeply into supplements. I have indicated a few places where this may be possible, but I do appreciate the word limit is tight. By comparison, the level of detail in the discussion section is much greater, so I wonder if there are sections here that might be abridged in order to accommodate the above.

• Line 48-50: minor point, but the opening sentence / paragraph do not explicitly mention that the Ross Embayment and Ross Ice Shelf are in Antarctica. I appreciate that this is mentioned in the Abstract, but I think inserting the word 'Antarctica' at least once in the opening sentence of the Introduction would be helpful to the non-specialists reading this paper.

Good points, I've added "Antarctica's Ross Embayment" added to line 48.

• Line 52: minor change to 'Following the onset of extension in the mid-Cretaceous,...'

Fixed

• Line 60: *offshore* drilling

Fixed

• Line 61: shouldn't this be a reference to Fig. 1a?

Yes good catch!

• Fig 1: for the benefit of the reader, please explain in the caption what the two black dashed lines are. Also, why are these two flights (ROSETTA-Ice) shown with the dashed lines, while the two labelled lines in the Ross Sea (OIB) have no markings?

I have changed the caption to read: "... airborne magnetic data from ROSETTA-Ice (over RIS) and Operation IceBridge (black dashed lines)..." and have added black dashed lines ontop of the the Ross Sea OIB lines.

• Line 81-82: if words are really tight you could probably remove this final sentence.

Okay I will keep this in mind as a section to remove if necessary.

• Line 85-87: the references to the supplementary figures and text got a bit confusing here as they don't appear to be referred to in order. For example, the first supplementary Figure to be referred to is Fig. S4, and the first supplementary Text is Text S6. Perhaps aim to sequenced the supplementary items in the order in which they are referred to in the main manuscript, and double check that they are referred to at appropriate places in the text.

I have moved the assumptions portion of text S6 to a new text S1, so now all the supplement texts are referenced in order, and I have fixed the figure references to be in the right order.

• Line 105: *BedMachine* bathymetry.

Added

• Line 106: suggest deleting 'map basins and'.

Deleted

• Line 107: 'geophysical anomaly patterns' is a little vague. What does this refer to specifically? Potential field anomalies?

Changed to "These sub-RIS results together with free-air gravity data allowed us to infer the locations of regional scale faults beneath the RIS"

• Line 110: It would be useful to briefly state where these gravity anomalies come from.

Added ROSETTA-Ice

• Fig. 2: super nit-picky, but you refer to the line segment in the caption as '403-1', whereas in Fig. 1b it is labelled as '403.1'. Suggest keeping the notation consistent throughout. I'm also a little wary about the sentence at the end about symbology being described in the supplement. The reader should be able to understand the figure without needing to look-up that information. Can I suggest including any missing essential information in the caption, or in the legend in the figure itself.

Good catch, I've switched all the references to using a decimal (403.1). I've updated this figure (and Fig S2) to help address your concerns. I've added a small map showing the location, as suggested by review 1, which made space to lengthen the text in the legend which should better explain the symbols. The caption has been updated to explain the symbols better.

• Line 117: how about starting this sentence with 'We find that...' or 'Our magnetic depth-to-basement calculations show that'?

Added "We find that"

• Line 120: missing a closing parenthesis here.

Fixed

• Line 121: how does 'several regions having <50 m sedimentary cover' fit with 'only ~1% of the area having <100 m of sedimentary cover' (line 118)? Perhaps a slight rephrasing is needed?

Most of the thinnest sediment covered regions are within the MSH, which includes a few small regions of <50m cover. This is poorly phrased in the manuscript and probably a little hyperbolic so have updated to: "... with only ~3% of the area having <200m of sedimentary cover." and "... with several regions with as little as 100m of sedimentary cover."

• Line 124: is ~400 m the average?

It's the difference between the average thickness of the East and West Antarctic sides (as defined in the colorbar histograms). WANT avg. is 1286m and EANT avg. is 1711m.

• Line 129: *median* sediment thickness uncertainty?

I think adding median here would be a little confusing. I've added "representative" instead.

• Line 130-131: If I understand Table S1, this is also a median uncertainty? Maybe say so explicitly here? Also, is it just coincidence that misfit between your results and ANTOSTRAT basement depth is the same as that between your results and the 8 seismic survey locations? I wonder about moving this sentence up above the sentence about sediment thickness uncertainty, so you can group the discussion of basement depth comparisons. I'd also appreciate a bit more information about the range of uncertainties you see across the region (e.g. min / max values), to get a sense of whether the uncertainties are roughly symmetrical about the median, or skewed towards the upper or lower bounds.

Yes, the Table S1 values are median misfits as well and I've added "median basement misfit" here. We are trying to differentiate these 'misfit' values from our uncertainty values, so are referring to the 8 seismic comparisons as median misfits, and out uncertainties (estimated using median misfit) as "representative uncertainty". Yes, it was just a coincidence that the median misfit between our sediment thickness and the 8 previous seismic estimates (480m) was the same as the median misfit between OIB and ANTOSTRAT. Ok I have moved the sentence to above the sediment sentence. We have included an additional supplementary figure showing the distribution of misfits both between OIB and ANTOSTRAT, and between ROSETTA and OIB.

• Line 149: You already stated that you did this in the Methods section. Some words could be saved by merging with the next sentence, e.g.: 'Inferred active sub-RIS faults (Figures 4a & S1) correspond to narrow, linear basement basins with high gradient gravity anomalies, prevalent on the West Antarctic side (Figure S1a).'

Fixed

• Line 177: a gravity anomaly 'low' or 'high'? Free-air or Bouguer?

Changed to "... coincides with alternating high and low free-air gravity anomalies."

• Fig. 3: This figure is looking great, and really emphasises the power of your results! Would you be able to include outlines of some of the major features you mention in the Results / Discussion? In particular, I'm thinking about the MSH and the linear ridge within the Western Ross Basin. If you used dashed lines to outline them in panel a, it might make life easier for the reader.

Thanks! Good idea, I've added dashed outlines around the MSH and the Western Ross Basin.

• Line 202-204: this seems a little speculative and somewhat out on a limb (the foundering isn't mentioned at any other point in the manuscript). I wonder whether, if words need saving, this sentence could be removed?

Yes, I agree this is a bit speculative, but I think if we're proposing active rifting under the Siple Coast, then it's important to provide a hypothesis for the source of this extension. We've updated the sentence to include another ref, as follows: The fault-controlled tectonic basins may reflect a crustal response to the lithospheric foundering hypothesized beneath the South Pole region (Shen et al. 2018) or be a broader regional expression of Neogene extension that formed the Bentley Subglacial Trench (Lloyd et al. 2015).

• Section 4.2: The majority of this section reads like a literature review and the links to the study at hand are made only in a broad sense. For example, the final few lines of this section (uplift driven-readvance) aren't linked back to this study, which seems to render these lines extraneous. Also, it isn't clear how this relates to the section title ('cryosphere-groundwater implications')? I wonder if this section could be tightened into a single paragraph (like section 4.4), where the emphasis is shifted onto how the findings of this study can specifically contribute to our understanding within the broad themes of fault/cryosphere/hydrological interactions.

We have rewritten this section, shortening in from 296 words to 216 words, and combining it into 1 paragraph. The material should now be slightly more linked to our results, and some of the lit review material is moved to the introduction. We have also rewritten the MSH-CH discussion section, to also read less like a lit review.

• Line 206: 'fault-induced/controlled'?

Changed to Fault-induced.

• Fig. 4: do you need to say 'with sediments removed' in the second line of the caption? Also, you can probably remove 'with implications for subglacial hydrology', unless you can be more specific here.

Yes good suggestions, they are removed

• Line 222: are you suggesting that these faults could accommodate the isostatic response to changes in ice mass along the Siple Coast? If so, Philips & Läufer (2009, Tectonophysics) would be an appropriate reference.

Thanks for pointing out this paper. I've added a reference to it in both sections 4.2 and 4.3.

• Line 237: might be clearer to say that you are inferring that the MSH is contiguous with the CH?

I think the main point of this sentence is not to show that the MSH and CH are contiguous (this is already stated on line 161), but to show that both are aligned with the geologic divide, as defined by Tinto et al 2019 and the Ross Sea coring.

• Line 259: I think 'mountain range' might be a little hyperbolic. Perhaps 'a long, narrow, elevated ridge emergent about sea level'?

Changed to 'long, broad, elevate ridge', since the MSH is probably better described as broad than narrow.

• Line 265: how much sediment?

Changed to "...and was covered in 100's of meters of sediment (~400m at DSDP 270, De Santis et al. 1995)."

• Line 291: 'magnetic' not 'magnetics'.

Fixed

• Line 293: do you mean subtracting the basement from the bathymetry?

Yes, you're right, but at a closer look 'subtracting' feels unnecessary in the conclusion, changed to "Using a bathymetry model,"

• Line 301: suggest breaking the paragraph before 'Newly...'.

Fixed

• Line 309: 'basin extents'?

Fixed

Supplementary Information comments

Please note earlier comment about ensuring the order of the supplementary text and figures matches the order in which they are referred to in the main manuscript.

Fixed

• Line 18: typo with the placement of the OIB parentheses.

Fixed

• Line 42: could you indicate how much of the line data were removed for this reason?

It was ~10% of the data. This was for over the center of Roosevelt Island or for flights with turbulence where they needed to fly above the weather. I'll add the percentage to the supplements.

• Line 70-79: this is very helpful, and I do wonder if it could be 'promoted' to the main manuscript (section 2)?

Okay, I have re-written the beginning of the methods section to include this material, which has added 70 words.

• Line 81: which method is 'this method'? Please explain for the reader (e.g. 'to validate the method described in Text S1).

Fixed

• Line 202: check whether the Gustafson et al. (2022) paper is accepted prior to publication of this paper.

Not currently published, but according to one of the authors will be published in the first week of May.

• Fig. S5: suggest changing the colourbar legends to 'upper/lower limit' rather than 'upper/lower uncert.'.

Fixed

Round 1 reviews:

Reviewer 1: Katharina Hochmuth

Basement topography and sediment thickness beneath Antarctica's Ross Ice Shelf imaged with airborne magnetic data

Tankersley et al.

The manuscript "Basement topography and sediment thickness beneath Antarctica's Ross Ice Shelf imaged with airborne magnetic data" by Tankersley et al. tackles an important questions about the how the underlying geology influences the cryosphere on various levels. The ROSETTA Ice Project is such an amazing effort and it is great to see such important and exciting science emerging from it. The manuscript is well written and presents important novel data. With the various topics included such as tectonics, subglacial and ground waters, thermal subsidence, sedimentation, ice sheet development etc., certain topics are not as deeply developed as needed to fully explore the new dataset. Some information now located in the supplementary should be moved to the main text, to better support the discussion. This is especially true for some of the figures presented.

Please see below for my questions and concerns with the presented manuscript.

I hope you find my comments helpful and instructive,

Best regards, Katharina Hochmuth

General comments:

Use of acronyms: The authors use many acronyms in their manuscript. Some are commonly used in the community (e.g. GIA, RIS etc.), others especially place names are quite overwhelming and difficult to keep track of (e.g. MIS is normally associated with marine isotope stage, but here is used as MacAyeal Ice Stream). I like to encourage the authors to reduce their use of acronyms for better readability and accessibility of the manuscript and include all acronyms used in a figure in the figure captions.

Agreed. I have updated figure captions to spell out acronyms. For place names, I have removed acronyms for Siple Dome Basin, Marie Byrd Land, Ross Sea, and the Siple Coast glaciers (WIS, KIS,

BIS, and MIS). I have retained RIS (Ross Ice Shelf), MSH (Mid-Shelf High), TAM (Transantarctic Mountains), and CH (Central High), as I refer to them often and need to reduce word count.

Key points:

Key points are partly misaligned with the conclusion given later in the manuscript. Bullet 3 might need some rewording to complement the conclusions given later.

Okay key points have been re-written.

Abstract:

l. 26 The term basement can easily refer to different things in this context. Specify if you are referring to crystalline, acoustic, magnetic etc. basement.

changed to magnetic basement, and we have tried to clarify throughout the text.

l. 27 basement-cover contact What contact are you referring here to?

Abstract has been re-written.

Plain language summary

Well written plain language summary. This paragraph conveys the main points of this study well without retorting to using specific jargon.

l. 45 Earth's

looks like the version I have has "Earth's" included already.

l. 49 maybe better to refer here to the solid earth? Or since it is the plain language summary ground?

This sentence has been rewritten.

Introduction:

The introduction outlines the questions of the manuscript well. The reader can gather the information needed to decide, why it is important to study the basement topography and sediment thickness, but I would suggest to also include a brief overview on Ross Sea geology and development through time, which can be tied to the discussion.

The intro has been rewritten and now has a strong section on geologic history of the region.

l. 59 past times specify the time frame you are referring to

This sentence has been removed.

l. 62 drilling specify which campaign, site and hole number

added (DSDP Site 270, Ford & Barrett, 1975)

l. 62 basement Which basement are you referring to here? Please specify your use of the term basement once here to avoid any confusion down the line.

changed to 'crystalline basement'

l. 73 - 81 Why is it so important to delimitate those differences? How can a better constraint of these parameters help us to better understand the Ross Ice Shelf, or ice shelves in general?

We have re-written the intro to have the focus be on implications more relevant to the scale of our results. The original intro was focused on cm-scale sediment properties, while our results are more important to km-scale findings, such as locations of faults, aquifers, and basement margins.

Data and Methods:

l. 91 Since the differentiation between sediment, sedimentary rock and crystalline basement is the basis of this manuscript, a very brief overview on the local geology of the Ross Sea embayment will help the reader to gather more context. This could certainly be added to the introduction preparing the reader what kind of strata can be expected.

We have added a geologic background section to the introductions, to prep readers for the presence of rift basins and sedimentary infill.

l. 104 ANTROSTRAT is certainly a great source to use to in cooperate the seismic data collected in the region. Have you checked the accuracy of the data used with newer, updated mapping efforts?

No, I haven't but it would be interesting. I was aware of updated mapping at certain locations in the Ross Sea, but not at locations coincident with the OIB flight paths.

l. 122 Regarding the intrusions, give the reader a general overview how much of a concern this is in the Ross Sea and in which regions this is a common feature.

We have added some material to the supplement Text S6 regarding the intrusions, included references to geologic maps and the expectation of low magmatism in the region.

l. 126 How are you defining a characteristic wavelength of the Ross Sea?

We applied several Gaussian filters (20, 40, 60, 80, 100, 120 km etc.) to the ANTOSTRAT basement surface (derived from Lindeque sediment thickness and Bedmap2 bathymetry) and compared the results with the unfiltered basement. Filters less than 80km didn't alter the regional basement features, while filters greater than 80km excessively smoothed the basement. This is now included in Text S4.

137 Citation should be Mueller et al. 2007

Possibly a misunderstanding of the author and reference? My version shows Müller et al. 2007 (author identity and spelling can be found here: https://www.sydney.edu.au/science/about/our-people/academic-staff/dietmar-muller.html)

Results:

The results are well described, but would profit from using some decluttered figures as presented in the supplement to become more accessible to the reader.

General comment: A lot of new place names are introduced, since these features have not been described before just refer to them as labelled on the map and omit using phrases like "here termed", "here referred to"... It leads the reader to wonder, if there are other names for it and overcomplicates matters.

Good point, I have altered these phrases.

l. 146 Relate the mean difference to the error in the methods. Almost a kilometre does sound a lot, but can easily explained by seismic data uncertainties etc. Same for your other statistical values in this paragraph.

We have updated our uncertainty estimates, and included a new supplementary text section, Text S6. Analysing the misfits between our modelled basement and the predicted basement (OIB to ANTOSTRAT, and ROSETTA to OIB) showed that there is not a strong depth dependence on the misfit. This suggested that a uniform value of uncertainty would be more appropriate than a percentage. Since the misfits don't follow a normal distribution, the median value of the misfits is more representative of the values, compared to a mean value. The median value of misfits between OIB and ANTOSTRAT is 480m, and the median value of misfits between ROSETTA and OIB is 400m, so we adopted +/-480m as our representative uncertainty for the basement model. This equates to 22% of average basement depths for the sub-RIS.

l. 183 omit "With criteria outlined in the Methods", the reader has just read the methods and if not and they are wondering, they will know where to look for the information.

Fixed

Discussion:

The discussion briefly runs through a lot of interesting different topics with could be explored more deeply. To "live-up" to the statements made in the abstract and keypoints, especially the discussion of geothermal heat, GIA and groundwater needs to be further explored.

Yes, we have re-written lots of the discussion section to expand, especially on the groundwater / cryosphere section.

General comment: Given the vicinity of the Ross Sea to the pole, pointing out East and West is not always intuitive. Ensure clear markings on Figure 3, where east and west is and maybe guide the reader in the discussion with additional directions pointing to e.g. TAM or the Siple Coast.

Ok I have made the East West distinction clearer in the inset of Figure 1 and try to referred to the locations more.

211 "common tectonic origin" Could you elaborate what event, mechanism you are referring here to? This could be related to short introduction into Ross Sea geology in the introduction.

Updated to "a common tectonic origin as fault-controlled basins (Figures 3a & 4a) formed through Cretaceous distributed continental extension across the WARS (Jordan et al. 2020)."

l. 215 This paragraph is making nice connections to the plate tectonic history of the Ross Sea, which I would strongly suggest should be strengthened throughout the manuscript. The geographical locations are not being presented in earlier figures, which makes is hard to follow for readers not completely familiar with the Ross Sea.

I've added Adare Trough and Terror Rift to the sediment thickness figure.

l. 237 Good to point out the change in β-factor is due to a different database and should not be interpreted!

To avoid confusion, we have changed the figure to only show beta factors where we have sediment thickness estimates.

l. 241 A large portion of this paragraph talks about ground water, is this really the right heading?

Good point, we have changed the heading to Cryosphere-groundwater implications.

l. 250 onwards The interaction between groundwater, subglacial water and the basement is certainly an interesting topic and of massive importance to our understanding of ice sheet dynamics. As it is presented at the moment, there is no real linkage between the presented data and the ice sheet behaviour. What does the new data show and what connections can be drawn? In the abstract and conclusion, groundwater plays an important role and should therefore be discussed in more depth in the discussion section.

We have had some developments in this section, which are now included. We have also added some material to the intro about groundwater. You're correct that this is a fascinating and important topic. We hope this paper, and Figure 4b, will pique people's interest in this material. Unfortunately, there wasn't room to expand it significantly, but we have added some mentions to other works, including a paper in review for Science, Gustafson et al. 2022, which will hopefully be published by the time this is accepted. Their work shows deep groundwater (up to 2km) beneath Whillans Ice Stream and is thought to provide water to the ice-bed interface. We suggest these basinal aquifers are likely in the Siple Coast basins we identify, and that they influence ice dynamics through 1) controlling ice-bed hydrology and pore pressure, and 2) localizing GHF as water flows along basement margins and faults.

l. 279 "previous workers", maybe change to authors, or in previous literature

Changed to 'Previous authors"

l. 284-287 This is an important finding. What does this mean for plate kinematic reconstructions, ice sheets etc.?

Not sure of the implications for plate kinematics, but if this MSH-CH feature separates styles of rifting on the EANT vs WANT sides, it could explain the tectonic control on bathymetry, as discussed in Tinto et al. 2019. Taking this further may explain the disparate deglaciation patterns of the eastern vs western Ross Embayment. Unfortunately, there is not space in this manuscript to expand on this further.

l. 288 Paxman et al. 2019's oldest time slice is the Eocene/Oligocene boundary

changed to early Oligocene

l. 294 maybe also see Kulhanek et al. 2019 as well as Paxman et al. 2019

added Kulhanek et al. 2019

l. 304 What about deglaciation patterns? See Halberstadt et al. 2016

Good question. The presence of shallow crystalline rock along the MSH could be providing the geologic control on deglaciation discussed in Halberstadt et al. 2016. Additionally, the main control on deglaciation identified by Halberstadt et al. 2016, sea floor physiography, was likely controlled by the basement topography, even after burial, since the rift basins might continue subsided faster than the intervening basement highs. I'm not sure if there's room in this short-format paper to expand on this, but I've added the Halberstadt reference at the end of this sentence.

l. 306 - 326 This is very interesting! Could you compare your assessment of the height of Siple Dome to Paxman et al. 2019's maximum reconstruction? In previous papers (e.g. Coenen et al. 2020, Uenzelmann-Neben et al. (accepted Nature Communications)) argue for the minimum topography to be closer to observed values. The comparison to previous sediment volume assumptions should be expanded. Is the increase in sediment similar across the entire basin, or have certain basins been dramatically underestimated? Given the age uncertainty of the sediment basins, can it be ruled out that the sediments are coming from deeper inland via preglacial rivers and therefore not directly relate to the height of the surrounding area?

To make space for other sections, and since this was the least well-constrained of our discussion points, we have shortened this section. Also, we had a mistake in the original version where we state that thicker sediment we image would result in higher paleotopography. We think we were incorrect about this since increased sediment would result in more subsidence, but once you remove the thickness of sediments itself the paleoelevation of the basin bottom would likely be lower. We have left our discussion of sediment thickness comparisons with the model used in the reconstructions but have removed the implications for paleotopography. We have also clarified the section on thermal subsidence.

Conclusions:

l. 345 and onwards The prominence of the groundwater results in the Conclusions are not supported by the discussion.

We have altered the conclusions to try and match the prevalence of each section in the discussion.

The last point of the discussion regarding Thermal Subsidence and sedimentation is omitted from the conclusion, but should definitely be included.

I have added this into the conclusion now.

Figures:

General comments:

The figures are quite packed and try to convey a lot of information. Overall figure quality is good, but with the density of information and the potential small printing size, I would recommend to consider to move figures a and b to individual figures.

Unfortunately, I don't think there is space to separate a and b, as it will reduce the allowable word count by 500 words. As an alternative, since I have added a new figure 1 at your request, I have moved much of the information to the new figure 1. This includes the inset map, many of the location labels, and the OIB flight lines. I have also changed to colour scale to be discrete so that I could remove some of the contour lines and have made the lat/long gridlines more discrete. This makes the results figure (Fig 3) much less cluttered.

Readers with vision impairment might find it difficult to differentiate the colour scales on some of your figures (especially Fig. 1b). Maybe consider different colour scales and omit the use of e.g. red arrows close to green colours (e.g. Fig. 3b).

I've checked the figures with multiple color-blind simulators to check that they are all better suited for vision impairment and changed the parts which were no accessible.

Annotations are small and hard to read. Please check, if all locations etc. are actually mentioned in the text (and vice versa) and consider larger fonts for the annotations

I have increased font sizes and removed some location labels.

Fig. 1

Insert map should be enlarged it is very difficult to see the various regions and differentiate between the different dashed fields.

I have enlarged the text and the inset map.

In Fig a and b, move the legend and histograms completely below the figure, which allows you to enlarge them. Clarify that a black lines is separating at the MSH. The histograms only depict the data underneath the ice shelf and not the entire map shown, right? Please clarify this also regarding the choice in colorscale (min-max). The dotted-dashed line is hardly visible and not all flight lines mentioned here are shown in the text. Include a separate flight location map in Figure 3. Previous basement recordings are incredible useful to display here, but need to be enlarged to be actually visible.

I have moved/enlarged the histograms, moved the OIB lines to the new figure 1 and made them more pronounced, enlarged the previous seismic point surveys. I have clarified in the caption for the results figure that the colorscale is ranged for the RIS data, and included contours for the new figure 1. I have also included an inset map showing the EANT/WANT divide used for the histograms.

Fig.2

Add inlet map depicting the location of the flight line. In the methods section you describe using Bedmachine2 bathymetry. Why did you choose to show Bedmap2 bathymetry here? In the figure caption repeat which parameter S is, so the reader does not need to refer back to the text. The grey lines depicting the distance should be in the same distances in both figures.

I have made the flight path more obvious in the location figure (Figure 1) and added a reference to Text S1-2 for the symbol description. We had to be very stringent with our word count to add in the new figure 1. Bedmap2 was used to remove Werner solutions shallower than bathymetry. We chose not to use bedmachine for this, and also not to use it for these figures, since it is also a product of geophysical inversion from ROSETTA data and we didn't want to introduce unnecessary uncertainties. The grey lines in the upper panel are variable since they show the width of the bins (parameter b) used in the binning.

Fig. 3:

For easier comparability between all figures, align Fig. 3a with Fig. 1, detailing, that the focus is now on the RIS. GHF measurements need to be increased in size to be actually visible. Fig. b should be its own figure, in this small setting, it is incredible hard to see all the different features. As mentioned above, the section on groundwater and subglacial water needs to be expanded to be able to fully grasps the mechanisms presented in the figure.

Unfortunately, I don't think it is possible to make space in this paper to split this figure into 2 separate figures. I have tried to declutter the figure with the following adjustments:

- moved location labels to other figures

- removed all GHF points except Lake Whillans

- moved the beta factor colour bar below the figure

- removed onshore beta factors

- removed some pre-existing faults

- increased font and symbol sizes and placed colorbars in more relevant locations

Supplementary Material:

l. 58 give an error estimate for the basement depth

Our new representative uncertainty for the basement model is +/- 480m. See the response to your comment about line 146. There are additional figures and text in the supplement section that discuss our new uncertainty analysis.

l. 110 "8km cell" Is that the resolution you achieved? This should also be mentioned in the main text.

We have clarified this, as below, in Text S4.

"The above processes were performed on all ROSETTA-ice flight lines (white lines in Figure S4), including the N-S tie lines at ~55 km spacing. Where the tie lines crossed over the E-W flights lines, some resulting basement solutions (black dots in Figure S4) are nearby those from the crossing line. Since we are interested in the shallowest magnetic signals, we have retained only the shallowest solution within 8km cells across our region. Since bin widths (parameter B) were set to 36 km, the nearest solutions along individual lines were further than the 8km cell. The closest spacing of E-W flight lines was 10 km, so this processes only affected solutions at N-S/E-W line crossovers. These points were then gridded with a 5 km cell size and a minimum curvature spline with a tension factor of 0.35 (Smith & Wessel, 1990) (Figure S4)."

l. 114 Lindeque et al. 2016 is citing Wilson and Luyendyk 2009 as the source of their Ross Sea dataset. Maybe add that citation.

Added

l. 115 Bathymetry taken here is Bedmap2, whereas in the main text Bedmachine is referred to. Can you clarify?

This was a mistake, now it is using Bedmachine bathymetry

Fig. S1

This figure is very important to the entire manuscript and should, if possible moved at least in parts to the main text.

I've moved panels b) and e) (bathymetry and magnetics) to be a new Figure 1.

Fig. S2

Add a small location map, to declutter Fig. 1. Could the difference in seismic basement also stem from what can be seen in seismic data as basement and what can be seen in magnetic data as basement? It would be worth briefly discussing this to explain these differences. Especially e.g. intrusions could mask all underlying sediments in the seismic, but with your method, you are excluding them from the dataset.

The new figure 1 shows the location of the OIB lines now. I have included a new Text S6 "Uncertainty and assumptions" with some clarifications surrounding this.

Fig. S3

Same comments as on similar figures in the main text. It is definitely worth showing multiple different lines to showcase the technique.

Figure and caption have been updated, and line locations are clearer now in Figure S4.

Fig. S4

This figure is very helpful to understand data coverage and main features of the resulting grid. A similar plain version as Fig. 1 would be ideal to ease the reader into the study region.

A new figure 1 has been added, but unfortunately, there is not space to include this figure in the main text.

Reviewer 2: anonymous

This paper - "Basement topography and sediment thickness beneath Antarctica's Ross Ice Shelf imaged with airborne magnetic data" by Tankersley et al. - uses magnetic anomaly data acquired from the ROSETTA-Ice and Operation IceBridge airborne geophysical surveys to determine the depth to the magnetic basement below the Ross Ice Shelf, and (when combined with the BedMachine bathymetry compilation) the thickness of the sedimentary succession. The authors then use their results to: (a) infer the regional structure of fault-bounded basins dating from West Antarctic Rift System extension, (b) identify locations where basement topography and structure (and heat flux) may influence ice dynamics, for example through providing nucleation points for ice during early glaciation or localising subglacial meltwater flow, and (c) comment on the likely regional palaeotopography prior to and during early glaciation.

Overall, I found the manuscript to be interesting, well-written, and well-illustrated. It makes an important advance in our understanding of the subglacial and submarine geological boundary conditions of the Ross Sea sector. The authors also link their findings to a number of outstanding and pressing issues, such as regional heat flow, crustal structure, palaeotopography, and ice dynamics. Although there is scope for improvement, the data and methods are robust and clearly described, with careful consideration and justification given to the limitations and assumptions of the methodology, and the interpretations and conclusions are generally justified. My comments and suggestions for improvement largely pertain to restructuring certain parts of the manuscript and figures to allow for a clearer flow for the reader, and some suggestions for additional points of discussion or clarification that may also help broaden the manuscript's appeal. Subject to revision to address these largely minor issues, I believe this paper will be suitable for publication in Geophysical Research Letters.

Manuscript comments

• Line 25: I wondered whether describing the crust as 'extended' here rather than 'subsided' would be better?

Yes I agree that is better.

• Line 28: specifically, a 'segmented *basement* high' and '*non-magnetic* sedimentary cover'?

Correct, I've added those descriptions in.

• Line 29: indicate an approximate timing of this subsidence below sea level?

Added 'in the Oligocene'

• Line 31: '*sedimentary* basins'?

Added

• Line 58-61: please give an indication of the specific dates / geological periods of the 'past times' for these reconstructions you refer to.

The intro has been re-written, but reference such as ‘past times’ are now more direct.

• Line 63: typo/reference bug - should be Pérez et al. (2021) (see also reference list and acknowledgements).

It must be a glitch, my version has Pérez et al. (2021).

• Line 69: I think you can delete 'by comparison'.

Ok

• At a number of points through the introduction / methods / results I wondered if the ordering of the figures could be improved. Fig. 1 seems to show the key results (inferred basement depth and sediment thickness maps), but comes early in the manuscript before the methods and results are described. Consequently, the reader has to flip back and forth between the results section and Fig. 1, and furthermore there isn't really a figure that 'sets the scene' other than Fig. S1, which is in the supplement. One suggestion would be: have a new Fig. 1, showing BedMachine bathymetry and the ROSETTA magnetic anomaly that are currently in Fig. S1 panels e and b, respectively. These are the two key 'input' datasets for your analysis. It would also be helpful to show the ROSETTA-Ice flight lines (currently only shown in Fig. S4) in either panel, as well as the four OIB lines in a different colour. You could also add relevant labels from the current Fig. 1. Fig. 2 could stay the same since it nicely illustrates your methodology, and then the current Fig. 1 would become the new Fig. 3, and be located in the Results section. The current Fig. 3 could then become Fig. 4. This would of course increase the figure count, and I am aware space is tight for this journal. However, in my opinion it would help better illustrate the flow of the manuscript from initial data to method to results to discussion. If the word count needed reducing as a result, I would suggest the paragraph between lines 106 and 130 could be shortened, or some discussion points could be abridged also (for example, could section 4.2 be dropped?).

Yes I agree your suggestion here makes more sense. I have moved the current Fig 1 to the results section, where it is now Fig 3. I’ve added a new Fig 1 which is 2 panels, showing BedMachine bathymetry, ROSETTA / OIB magnetics, and offshore ANTOSTRAT basement. These are all the ‘input’ data sets, which I think is nice to have all in the same figure. I’ve shown the ROSETTA mag data with gaps between lines, so that you can make out line spacing, but I’ve left the line labels to be in the supplement since it crowds the figure. To de-clutter current Fig 1, I’ve moved location labels and the inset map to the new Fig 1.

• Having said that, the current Fig. 1 is a very helpful depiction of the region and contains lots of useful detail that illustrates your results well. In addition to perhaps moving this figure to later in the manuscript, I have a few suggestions to make it a bit clearer for the reader. (i) in panel a, please make the 4 OIB profiles a bit clearer (different colour / thicker line?) and label each of the four lines on the figure itself (so we know which is which). (ii) could you make the square marker for DSDP270 a bit larger? (iii) In panel b, it's hard to make out the circles for the seismic survey sites. Maybe change the colour of their outline (white clashes with the contours), and/or increase the size of the symbols. You could also label these in a different colour to differentiate from the geographic locations in the rest of the figure (or better yet, the geographic labels could be moved to the new Fig. 1, where they could be placed on the bathymetry map).

I have moved the OIB lines to a new Fig 1, where they are shown more clearly. I've increase the DSDP marker, and the size of the previous seismic surveys.

• The histograms that separate DTB and sediment thickness for EANT and WANT are informative, but it isn't clear how these two areas are defined. My impression is that you are following the boundary between East and West that is delineated by the magnetic anomalies as mapped by Tinto et al. (2019) that runs along the 'mid-shelf high'. (Note that MSH needs to be defined in the caption). If so, please clarify. Drawing and labelling the boundary would be helpful to the reader.

For the EANT/WANT divide, we picked a line through the Mid Shelf High, which is approximately coincident with the Tinto divide. To clarify, in the new results figure (figure 3) I have included a small diagram showing the WANT/EANT separation for the histograms.

• Line 74: Competent basement vs cover sediments. The DTB defined by the magnetics isn't necessarily the same as the boundary between competent and non-competent rock. Subglacial bed is most likely defined by a continuum from soft sediment through indurated sediment to metasediment to bone fide 'basement'. The basement as defined by the magnetic content of the rock may not equate to any particular boundary of rock competency. While the points made in lines 72-81 are valid, this distinction between the deformability vs magnetic properties of subglacial bed could be more clearly communicated. The main point I would suggest is that you are as clear as possible that what you are defining as the top of the basement in Fig. 1 and your results is a boundary between magnetic basement and non-magnetic cover, which is assumed to be sediment / sedimentary rock.

Yes this is a good point. I have gone through and tried to be much clearer about this distinction.

• Line 90: Could be helpful to refer the reader to a place where the flight lines are illustrated.

Reference to figure added.

• Line 97-105: this is a clear and concise description of the method, and is well augmented by Text S1 as well. I wondered if however, whether either this section or Text S1 would benefit from some additional references (perhaps of studies from Antarctica) that have applied this method before? Does your approach follow that of the earlier studies like that of Karner et al. (2005; EPSL), or are there improvements / modifications you have made? It seems from the description in the following paragraph (starting line 106) that your method for constraining the free parameters in the Werner deconvolution and tying your results to existing seismic data is more sophisticated than what has been done before in this region (or anywhere in Antarctica to my knowledge?). If so, these points of difference (or similarity) to previous work could be more clearly emphasised.

Yes, you are correct that we have developed this method further from what has been previously published around Antarctica. To make space for the additional figure, I have had to reorganize and move a lot of the method material to the supplement. I've cited additional Antarctica depth to magnetic basement studies and have tried to clarify our method advancements. Most of this material is now in Text S1-3

• Line 107: please define ANTOSTRAT when it is first used to help the reader.

"Antarctic Offshore Stratigraphy" added.

• Line 108: I think 'Ice Bridge' should be one word.

Yes, I've changed it.

• Line 114: for reference and to aid potential reproducibility, could you state what the optimised parameter values were? Was it a single set of values for the whole domain? I think this information is provided in the supplement, but it might be helpful to state the values here, or at least clearly direct the reader to Text S2/S3.

I've moved the "Text S2" reference to a better location.

• Line 120-122: it wasn't totally clear to me how you defined where these intrusions were located? It would be interesting to understand if you have explicitly defined / mapped them, or if you are just assuming that they may be there and the filtering will remove them.

We have just assumed that if they were present, then the filtering has removed them, assuming they were small enough. Since we filter based on parameter S (susceptibilty x dike width), we have filtered any small intrusions out. This section is expanded in a new Text 6.

• Line 128: as stated earlier, I would recommend showing BedMachine bathymetry in the main manuscript. It would also be helpful if you could indicate to the reader how the sub-RIS bathymetry in BedMachine was defined. Did they 'transplant' the bathymetry derived from inversion of ROSETTA gravity data in Tinto et al. (2019)?

I've added a new figure 1, which now included the BedMachine bathymetry, and I've included in the caption that the sub-RIS portion is the results from the ROSETTA-Ice gravity inversion (Yes it seems that it was directly included in the BedMachine compilation).

• While on the topic of bathymetry, how much uncertainty is there in the BedMachine2 sub-RIS bathymetry, and how does this translate into sediment thickness uncertainty?

See below response.

• And more broadly, is it possible to quantify uncertainty maps for the basement depth and sediment thickness? Presumably basement uncertainty would come from the magnetic analysis, and sediment thickness uncertainty from a combination of basement depth and bathymetry uncertainty. Thinking ahead, these depth to basement and sediment thickness products should be a valuable community resource, and quantification of their uncertainty (even in relatively basic terms) would augment this nicely.

We have updated our uncertainty estimates, and included a new supplementary text section, Text S6. Analysing the misfits between our modelled basement and the predicted basement (OIB to ANTOSTRAT, and ROSETTA to OIB) showed that there is not a strong depth dependence on the misfit. This suggested that a uniform value of uncertainty would be more appropriate than a percentage. Since the misfits don't follow a normal distribution, the median value of the misfits is more representative of the values, compared to a mean value. The median value of misfits between OIB and ANTOSTRAT is 480m, and the median value of misfits between ROSETTA and OIB is 400m, so we adopted +/-480m as our representative uncertainty for the basement model. This equates to 22% of average basement depths for the sub-RIS. From the methods section of Tinto et al. 2019, they state the uncertainty for the gridded bathymetry data is 68m. Assuming their density assumptions are valid, we can add this uncertainty to our basement uncertainty to achieve an uncertainty of 550m for the sediment thickness results. This will be included in a supplementary figure showing the max and min basement depths, and sediment thicknesses with these uncertainties applied.

• Figure 2: please define the vertical grey lines (bins) in the caption. Also 'Mag (nT)' as an axis label could be somewhat ambiguous. I think it would help to write this out fully as 'Magnetic anomaly (nT)'. Same applies for Figs. S2 and S3.

Due to word count, we have had to remove the descriptions of the individual symbols from the caption, but we have referenced Text S2-3 where they are described. The axes label is fixed.

• Line 133: I am not sure if 'transect' is the best word here...perhaps 'connect' or 'bisect' is what you mean?

Changed to 'cross'.

• Line 137: reference glitch in Müller et al. (2007)?

Must be a glitch, the reference appears fine in my version.

• Line 144: insert 'basement' after 'seismic'. Also, mentions of seismic data / basement in this paragraph need an accompanying reference(s).

This sentence has been removed.

• Line 169: refer to Fig. 1b.

Added

• Line 183: how did you differentiate between active and inactive faults? Is there any independent evidence that your active faults are truly active?

Our active faults border regions with high gradients in basement topography, sediment thickness, and gravity. To have narrow, deep, sediment filled basins there needs to be relative motion upon faults. The sharp structural relief of these features shows they are relatively active/young, since more mature faults and basins would be less pronounced as they are extended more. We have updated the methods section with a better description of this, as well as citations for our justifications. Independent evidence for these faults comes from the ground-based, high resolution geophysical surveys which imaged hundreds of meters of structural relief, interpreted as faults by Muto et al. 2013 and Greischar et al. 1992.

Line 189: suggest writing 'higher' rather than 'high' (it's all relative!).

Yes good suggestion.

• Line 203: I'd also argue your Fig. 1a shows this (another reason I'd suggest moving this figure!).

Yes good point

• Figure 3: in panel a, for the 'land areas' (i.e. those not covered by the RIS), what does the colour scheme represent? Is this still the beta factor? In panel b, is the gravity the free-air anomaly? This could be clarified in the axis label. Also - change 'mGals' to 'mGal'.

To avoid confusion, we are now only showing beta values for areas with sediment thickness estimates. mGal changed, and updated to "Free-air gravity"

• Line 240: how do your calculated beta factors compare to those assumed by Wilson & Luyendyk in their West Antarctic reconstruction (2009; GRL). My impression is that their values are much larger. Is such a comparison appropriate? If so, what might this imply for palaeotopographic reconstructions that aim to correct for post-rift thermal subsidence in the Ross Sea? I don't expect

you to go into great detail on this, but drawing this observation / comparison (if appropriate) would be informative.

I don't think a direct comparison between the 2 beta factor estimates is appropriate, since the range of their values is much larger than ours. Their values range from 2.5-6 for the RIS, while ours are between 1.5 and 2.5. This is probably due to the different methods; it seems as though Wilson and Luyendyk estimated beta factors based on plate-circuit data, while our beta factors are calculated by dividing an assumed initial crustal thickness by current crustal thickness. If we used their beta factors with our assumed initial crustal thickness of 38km, the resulting current crustal thicknesses would be 6.3-15km, significantly thinner than observed today.

I think a relative comparison between the beta factors is interesting and I've added some sentences about this to section 4.4. The main finding here is their 'wedges' of beta factors are uniform from the ice front to the grounding zone, while our results show an increase, at least for the eastern RIS, from the calving front to the Siple Coast. If you included this increase in extension at the coast, especially if the extension is recent, then the paleotopographic models should be higher along the Siple Coast.

• Line 247: suggest inserting 'spatiotemporal' before 'variability'.

This sentence has been removed.

• Line 249: slightly unclear. Do you mean because these features would imply low viscosity?

Yes that was poorly written, we have updated the section now.

• Line 262-274: do you observe any noticeable trend / difference in faulted basin width in the east vs. west halves of the domain? Rift basin width may be correlated with the rigidity of the lithosphere (weaker lithosphere = thinner basins; see Ebinger et al. (1999; Phil. Trans Royal Society London A)). If there is any trend in basin width it might contain some information about patterns of flexural rigidity, which could be compared for example to the findings of gravity studies that infer effective elastic thickness of the lithosphere (Te), e.g. Swain and Kirby (2021; GRL - see their Fig. 2).

This is a very interesting point. There is a noticeable difference in rift widths, with the narrow rifts on the West Antarctic side. However, this may appear to be opposite to what is seen in the Ross Sea with the ANTOSTRAT seismic basement. There, the Eastern Basin (West Antarctic side), is very wide, while the Coulman Trough and Terror Rift on the East Antarctic side are much narrower. It would be good to look into this further and possibly include it, but with the addition of a figure and the strict word limit of GRL I don't think it's possible to include here.

• Line 294: the suggestion in the literature is that (at least at DSDP 270) subsidence was occurring in the Palaeogene as well as the Neogene, and the site may have been submerged in the latest Palaeogene (ca. 25-27 Ma; see Kulhanek et al., 2019; GPC). So maybe just a slight rephrasing needed here.

Thanks for pointing out Kulhanek et al. I have rephrased and include the citation.

• Line 306-307: not a suggestion, but just wanted to wholeheartedly agree with this statement!

Yes, it will certainly be interesting to see how these additional constraints change the reconstructions!

• Line 315: palaeotopography at what time? Eocene-Oligocene boundary? How does this inference compare with the Coenen et al. (2019; GRL) study, which suggested that the palaeotopographic

reconstructions might be too high in the Siple Coast region? Perhaps the palaeotopography might have been characterised by a series of parallel 'lows' and 'highs' (basins and ridges), so in some places the (comparatively smooth) reconstructions are too high, and in other places too low?

Due to tight word limits with the addition of the new figure, we have decided to remove portions of this section. Since we don't have many constraints on sediment age with the basins, it's hard to say how it will affect the reconstructions.

• Line 325: can you comment at all on the age of the sediment? Based on the limited drill core records, is there anything that can be said about how much is post-glacial (34 Ma) and how much is pre-glacial?

Looking at ANTOSTRAT unconformities at the calving front shows most of the total sediment (>80%) is below unconformity RSU5 (18ma). This sediment makes up Package C (Perez et al. 2021) and approximately 50% is between RSU5 and RSU6 (early Miocene and late Oligocene), and the other half between RSU6 and basement (early Oligocene and older). This seems that at the calving front approximately ½ of the sediment is preglacial and the rest is post-glacial, this this is quite unsubstantiated, and it is likely that proportions of post and pre glacial sediment is different for the continental shelf versus the interior of the embayment. Hopefully NZ drilling efforts in the next few seasons through the ice shelf will reveal these answers!

• Line 328: insert 'map' after 'basement'?

Added

• Line 332: *the* sediment thickness distribution.

Added

Supporting information comments

• Line 18: spell out Operation IceBridge here to help the reader.

Added

• Line 25-35: somewhere in this paragraph references for the OIB and ROSETTA-Ice magnetic datasets should be clearly stated.

Added

• Line 26: suggest adding flight speeds in SI units if possible.

Added

• Line 32: should be 'flight' not 'flights'.

Fixed

• Line 42: what is the justification for selecting this particular depth range for valid solutions?

Those ranges were a typo. It is updated to: "Solutions were considered valid between 1200 m and 20 km of upward continued flight elevation (approx. 200 m - 19 km bsl)."

This range was to avoid unnecessarily shallow solutions (200mbsl is within the ice) or deep solutions, outside of the depth we're interested in.

• Line 47: didn't fully understand this...what was the cut-off specifically?

The cut-off was a minimum value of parameter S; the product of estimated magnetic susceptibility and width of the anomaly source. This filter removed sources which were either too thin or had low magnetic susceptibilities. This was the main step in separating the non-magnetic/low-magnetically susceptible sediments from the underlying magnetic basement. I've tried to clarify this throughout the text.

• Line 68: typo - 'bin' not 'bid'.

Fixed

• Line 88: I think you mean 'supports' or 'ensures' rather than 'provides' [the validity...].

Yes, changed to supports.

• Text S2 and S3: I think I mostly followed your approach whereby you optimise the Werner deconvolution parameters to tie OIB magnetic basement to ANTOSTRAT seismic basement in the Ross Sea, and then in turn tune the parameters for ROSETTA-Ice to match the OIB magnetic basement below the Ross Ice Shelf. However, I wondered if the description of this could be simplified or made a little clearer. The word 're-optimisation' is perhaps a little confusing and might be best avoided. I wondered if a schematic flow-chart type figure in the supplement could help you illustrate your method?

We have re-worded and tried to clarify this process, within the main text and the supplement. Hopefully it is clear now, especially since the new Figure 1 clearly shows the input datasets.

• Line 108: I couldn't quite convince myself I understood the meaning of this sentence. What issues? Why 8 km cells specifically?

We have clarified this, as below, in Text S4.

"The above processes were performed on all ROSETTA-ice flight lines (white lines in Figure S4), including the N-S tie lines at ~55 km spacing. Where the tie lines crossed over the E-W flights lines, some resulting basement solutions (black dots in Figure S4) are nearby those from the crossing line. Since we are interested in the shallowest magnetic signals, we have retained only the shallowest solution within 8km cells across our region. Since bin widths (parameter B) were set to 36 km, the nearest solutions along individual lines were further than the 8km cell. The closest spacing of E-W flight lines was 10 km, so this processes only affected solutions at N-S/E-W line crossovers. These points were then gridded with a 5 km cell size and a minimum curvature spline with a tension factor of 0.35 (Smith & Wessel, 1990) (Figure S4)."

• Line 115: Could you explain why the Lindeque et al. (2016) sediment thicknesses were subtracted from Bedmap2 bathymetry, rather than BedMachine (as you did for your basement depths). Is this an issue? If not, why not?

Originally, we subtracted sediment thickness from bathymetry to examine what sub-RIS basement depths would be with the gravity-derived sediment thicknesses of Wilson and Luyendyk 2009. Because those sediment thicknesses were derived from gravity, and the Bedmachine bathymetry for the sub-RIS was from a gravity inversion (Tinto et al. 2019), we used the Bedmap2 bathymetry (no gravity used for sub-RIS) to avoid issues with over-interpretation of the gravity data. Thanks for pointing this out. I have gone back through and used Bedmachine instead of Bedmap for these grids. It looks like it has resulted in minimal differences, approximately +/- 50m for sections of the Ross Sea continental shelf.

• Line 122: Suggest adding '...in extensional settings' at the end of this sentence.

Added

• Line 128: I wonder how much uncertainty there is in the uniform crustal thickness assumption, and in the An et al. (2015) crustal thickness grid (which I believe has been at least regionally refined since). How much might this uncertainty influence your calculated beta factors? Can you comment on this?

Unfortunately, An et al. 2015 didn't provide an uncertainty estimate for their crustal thickness, so it won't be possible to estimate uncertainty for our beta factors. We decided to use the continent-wide Moho model of An et al. 2015 because the Ross Embayment sits in between EANT and WANT and we wanted a model that was well suited for both domains. By far the biggest source of uncertainty in this calculation is the initial crustal thickness, which we assume was uniform and 38km thick. The uncertainty associated with this assumption would be very hard to quantify, but beta factors still provide valuable insight for comparison across basins.

• Line 130: replace 'extent' with 'domain'.

The sentence is now removed.

• Line 132: suggest you say 'unrealistic' rather than 'not justifiable' (if it weren't justifiable, you wouldn't have done it!).

We have decided to only display sub-RIS beta factors to avoid confusing.

• Figure S1: as mentioned earlier, I would recommend taking some of the panels from this figure (specifically b and e) and including them in the main manuscript. This figure could then become primarily a comparison of sediment thicknesses from past studies and your results. If you retain the free-air anomaly panel, change the unit to 'mGal'.

We have added a new figure 1, with bathymetry, magnetic data, and ANTOSTRAT basement. 'mGal' label corrected.

• Line 169: please provide a reference for the black line showing the EANT / WANT divide.

We picked this line and chose it based on the center of the MSH, as shown in Supp. Fig. 4. The new figures 3 and 4 now have inset maps showing the EANT/WANT separation.

• Table S1: this table is useful, but is it referred to at any point in the supporting information? (I know it is in the main manuscript). To make the table more informative for the reader, I'd suggest clearly noting in the caption that the seismic site names are labelled in (the current) Fig. 1. Presumably the magnetic sediment thickness is then sampled from your grid at these eight sites? Also, the final column should be titled 'Absolute difference (m)'.

I have incorporated your changes, and added a new Text S6 about uncertainties, which references the table