We thank Valentin Rimes for his detailed review. We have found the review very constructive and helpful for improving our paper. We have responded to all comments and suggestions and our responses are shown below.

Review 1 – by Valentin Rimes

Dear authors, dear editor,

Thank you for this nice manuscript!

Overall, the manuscript is novel, of clear scientific significance, useful for the community and well argued. The aim, the focus of the paper and the conclusions are clear. It is overall well written, and the figures are clear (and look nice!).

Nevertheless, there is still room for some improvement to improve the readability and strengthen the argumentation. Precisions or corrections are definitely needed for the 4s vs 6s TWTT of first volcanics and for the generation of the results of the model in time domain (see details in the PDF). Some interpretations might be restructured. Some precisions seem to be needed for some methods. There are several potential small improvements to the figures and the text. Finally, I also propose a series of comments that are more proposition than corrections and that the authors might elect not to follow.

After these improvements/precisions/corrections, I'm sure that the manuscript will be a great contribution to the understanding of passive margins and a great addition to Solid Earth.

You'll find my detailed review in the supplementary PDF. Do not hesitate to contact me if something I wrote is unclear or if you have questions about my review.

Best regards,

Valentin Rime

General Comments

In this manuscript, Cassel et al. present seismic data from the South Atlantic and discuss the alongstrike variability of SDR and its correlation and influence on accommodation space. It is then shown that these results are consistent with variation in decompression melting during breakup.

Overall, the manuscript is novel, of clear scientific significance, useful for the community and well argued. The aim, the focus of the paper and the conclusions are clear. It is overall well written, and the figures are clear (and look nice!).

Nevertheless, there is still room for some improvement to improve the readability and strengthen the argumentation. Precisions or corrections are definitely needed for the 4s vs 6s TWTT of first volcanics and for the generation of the results of the model in time domain (see details below). Some interpretations might be restructured. Some precisions seem to be needed for some methods. There are several potential small improvements to the figures and the text. Finally, I also propose a series of comments that are more proposition than corrections and that the authors might elect not to follow.

After these improvements/precisions/corrections, I'm sure that the manuscript will be a great contribution to the understanding of passive margins and a great addition to Solid Earth.

Specific Comments

Lines 134-135: I'd say that these velocity values (for sediments) should be justified. A simple reference or a small sentence might be sufficient but at the moment they come out of nowhere. Or are they also coming from McDermott et al. 2019 like for the SDR?

Regarding post-breakup sediments, in line 135 we state that we use a k value of 0.4 km/s/km for interval seismic velocity dependence on depth. Figure McDermott et al. 2019 shows a k value nearer 0.5 km/s/s, however this value is likely to be too large for the thicker post-breakup sediment of the profiles to the south. We will also test using k=0.5km/s/s/ in the depth conversion and the follow-on flexural backstripping to produce water loaded accommodation space and present the results as a sensitivity test either on a figure in the main text or in a supplement.

Lines 136-137: "The SDRs of the Torres High profile are very thick and are most likely composed of basaltic flows. As a consequence, we use a higher velocity of 6.5 km/s for depth conversion." Did you use 6.5 km/s for all SDR of the study or only for the Torres High profile? From the phrasing, it is not clear to me. If other velocities where used, this should be stated. If the same velocity was used, you might also add a sentence to justify it as your observation "The SDRs of the Torres High profile are very thick" is only valid for the S1 profile. I agree that these changes have no influence on the conclusions of this paper but it would be scientifically nice to justify your choices (simplicity and comparability might be valid reasons on this one :-)).

For simplicity we used 6.5 km/s interval seismic velocity for depth converting SDRs for all profiles (S1, S2, S3, S4). McDermott et al. (2019) show a laterally variable "skin" of lower interval seismic velocity about 2 km thick above deeper SDRs with 6.5 km/s. The average interval velocity for the whole SDR pile is likely to be slightly less than 6.5 km/s. We will test using a lower SDR interval velocity of 6.0 km/s in the depth conversion and the follow-on flexural backstripping to produce water-loaded accommodation space and present the results as a sensitivity test either on a figure in the main text or in a supplement. Because we only backstrip the post-breakup sediments (not the SDRS) this will have minimal influence on the determined water-loaded post-breakup accommodation space. This aspect of the discussion highlights why we focus in figures 4 and 8 on measurements in TTWT – the primary seismic reflection observation is in TWTT while a depth-conversion is a model with often substantial uncertainty.

Lines 234 ff: I find that this part on the Rio Grande Cone is not the strongest of your manuscript. I'd say that the Rio Grande Cone is mainly there because a large river brought sediments there. And then the high accommodation space caused by the not-so-magmatic margin allowed it to deposit on top of the passive margin. But let's make a thought experiment. What if this river brought sediments over S1 (or S2) profile? The same delta would probably also have deposited, but just further offshore, on top of oceanic crust or the very distal passive margin. So I'd say that the main control on the presence of this delta is mainly the presence of the river and the high accommodation space allowed it to deposit not too far offshore. The structure of the margin (magma very rich or not so rich) does control the accommodation space on the margin but not further oceanwards where there is anyway plenty of space (except exactly on the Walwis ridge but that's not a simple SDR). Maybe you could rephrase it as "the high accommodation space allowed the Rio Grande cone to deposit large thicknesses of sediments on top of the margin". Anyway, I feel that this discussion on the Rio Grande Cone is not super strong or the most interesting of your paper and you might want to remove it from the abstract and/or summary (but not from the main text). What do you think?

In line 130 we remind readers that margin sediment thickness is dependent not just on accommodation space but also on sediment supply. Sediment supply is of course controlled by many external factors and varies substantially along margin (as shown for the Pelotas margin example). The purpose of flexurally backstripping the post-breakup sediments to give water-loaded accommodation space is to remove the consequences of laterally varying sediment supply. This enables the water-loaded accommodation space of the Torres High line (S1) to be directly compared with the Rio Grande Cone lline (S3). Perhaps we need to explain more clearly the purpose of flexural backstripping.

Line 243: "for the magma-normal margin profiles in the south, first proximal SDRs occur at 6s or deeper". That is for me the main problem of the manuscript. Your figure 3 and 8 show that it occurs around 4s and not 6s. This is correctly mentioned in the Abstract "first volcanics are observed at 4.2s TWTT or deeper" but not here. This also has implications for the consistency of your model (section 5.3). And for your conclusion ("In the time domain, a magma-rich margin, with sub-aerial SDR flows, shows first volcanics at ~2s TWTT while a "normal" magmatic margin has first volcanics at 6 -7s TWTT."). It's not a huge deal as it doesn't change the conclusions of your paper, it's still deeper than 2s. But it's not 6s, that's incorrect.

We agree totally with this comment and we need to correct this. In figure 8b we show the TWTT for first proximal SDRs with uncertainty. The solid circle should be in the centre of the range – and perhaps the horizontal arrows should identify both maximum and minimum values (not just the minimum value). The median value for line S3 and S4 then becomes slightly greater than 5s (not 4.2s). Closer inspection of figure 9e shows that the very first volcanics occurs not at 6s but at about 5.5 s., more comparable with the observation.

We would not expect the model prediction and observation to agree exactly; the model is a very simple one. We should also mention that the model prediction assumes a fully thermally equilibrated lithosphere while the Early Cretaceous Pelotas margin is not fully equilibrated and is still thermally subsiding which will slightly decrease the TWTT of first volcanics.

Section 5.3 (lines 241-281): I'm not 100% comfortable with this section. Maybe some things have to be restructured/rewritten or better explained. I see 4 problems/improvement potentials with this section (and maybe with the structure of the whole discussion). Here they are and I'll go more in details on each after.

1. Methodology: I find strange to find methodological description here, I don't understand one part of the method and I'd argue that you need to include sediments to compare it with a real margin.
2. The model shows 6-7s TWTT while the seismic 4s.
3. I don't understand why the results of the modelling come only to support the TWTT of first volcanics and not to support also sections 5.1 and 5.2
4. I don't understand why you focus only on TWTT of first volcanics to identify magmarich/magma-normal margins.

See detailed replies to comments 1-4 below.

Ok, let's go in the details of each comment.

Comment 1:

Lines 246 – 263 and 275-278: I have the feeling that this is more methods and should not be placed in the discussion chapter (5). I would make a new section before the discussion to present the methods. As a lazy reader, I often want to read the discussion chapter without the details of the method and I thrust the (lazy) reviewers to have checked the methods :-) . At the moment, I think it “dilutes” a bit your discussion points. What do you think? Described in

Our paper is primarily an observational paper. Its purpose is not to test a model – it is to make observations from data.

We use the simple model (line 246 and onwards) only for the purpose of trying to understand our observations. We therefore place the simple model in the discussion – indeed we place it at the end of the discussion.

Line 275 ff.: Here I don’t get how you calculated your sections in time. Ok, you assumed that the Moho is at 10s (BTW on the figure it’s a bit deeper than 10s) but how did you calculate the other reflectors? Did you use the velocities mentioned in section 4 and you used this 10s-rule just to compensate for the lack of good constrains on the velocity of the crustal basement or…? This needs to be better explained.

We need to explain more clearly the use of Warner’s 10 second rule for Moho TWTT. Wie will revise the text to do this. Warner (1987) observes and explains why the Moho TWTT for thermally equilibrated lithosphere is always close to 10s irrespective of crustal basement thickness and sediment thickness above. It is an approximation but a useful rule. It means that if the Moho is at approximately 10s and we know basement thickness, then the TWTT to top basement can be calculated (we assume a basement seismic velocity of 6.5km/s). This bottom approach means, for thermally equilibrated lithosphere,that the TWTT of top basement to first order is independent of sediment thickness. The reasons for this (explained by Warner 1987) are a combination of isostasy and the relationship between density and seismic velocity.

Also, how can you model a section in time and compare it with your seismic if you don’t model post-rift sediments? I understand that the post-breakup thermally equilibrated sections (fig. 9 c and d) are just a concept and not used to compare with reality 1:1 and thus it’s not a big problem not to model sediments. But you compare the time-converted sections (fig.9e, f) 1:1 with real-world examples. And I’d argue that the presence of sediments instead of water would have a big influence as the seismic velocities are completely different. Without a change, I’d say that the comparison is invalid. But as I did not fully understand your methodology, maybe I missed something.

See earlier comment on the use of Warner’s 10s rule..

As they are four problems with this time-converted section (two highlighted here, one in comment 2 and one in comment 4), you might elect, in an extreme case if you can’t correct it, to just remove this time comparison and only use the modelling to show the change in volume of volcanics and changes in accommodation space (see also comment 3 and 4). The paper would still be strong, relevant and useful without this. But of course, it’s nice to have.

The paper is primarily an observational paper so, yes, the simple model at the end of the discussion could be omitted. However we believe that the simple model forms a useful part of the discussion of the observations.

Comment 2:

The model shows first volcanic material at 6-7s TWTT while the seismic shows it at 4s (as already discussed above). Maybe you have to re-run your model with decompression melting starting a bit earlier to match with your observations at 4s. Or what if you include sediments in your time-migrated model (as discussed in comment 1)? As they have velocities higher than water, that might pull up the appearance of the first volcanics and better fit your observations.

See earlier comments. We will revise the text to address this.

Comment 3:

I don't understand why your modelling results only come at this point of the manuscript.

See response earlier. The aim of our paper is to make observations – the model is only used to try to understand the observations – the purpose of the paper is not to test the model.

In the main text, you only use it to justify your 4/6s TWTT but in the abstract and the summary, you also use it to justify the difference in SDR thickness and accommodation space ("*The observed inverse relationship between post-breakup accommodation space and SDR thickness is consistent with predictions by a simple isostatic model of continental lithosphere thinning and decompression melting during breakup*" and "*The observed inverse relationship between postbreakup accommodation space and SDR thickness is predicted by a simple isostatic model of continental lithosphere thinning and decompression melting during breakup*").

I think you have to discuss it a little bit more in the text. You need to justify it in the text before presenting them as "conclusions". At the moment you only have one sentence "lost" in section 5.3 about accommodation space (line 271-273) but nothing about volume of volcanics. Of course, I agree with you, your model show it, but you should mention it clearly in the text. Why not use them in the text to justify your point 5.1 and 5.2? Your model indeed does not only show the difference in TWTT of first volcanics, but also that the thickness of volcanic material and accommodation space changes with the different parameters. Why only use it for 5.3, right at the end?

We understand this comment and we will revise the text to explain this more clearly.

Your order of argumentation at the moment also makes less sense as you already concluded in section 5.1 that the margin is more magma-rich in the N. Why would you then test the same hypothesis with a model in section 5.3 if you already came to this conclusion? I see two options to circumvent this problem: either you present your modelling first (as already proposed on comment 1 and see further comments) and then can also use these results to support your point 5.1 and 5.2. Or you say that you don't need to prove this with a model and that the thickness is the volcanic sequences is enough to prove we are more magmatic (and I would agree with this). If so, you wouldn't need the modelling at all and you can discuss the TWTT of first volcanics based on real-world data alone (but I'd say that your model is still a good addition to the discussion). Maybe you see another solution to circumvent the problem?

See earlier replies about what we see the role of the modelling to be in the paper. We see this paper a an observational paper, not a modelling paper. We include the modelling in the discussion (at the end) only to help understand the observations.

Comment 4

I see another problem in the summary linked to section 5.3: "Our study shows that SDRs are not synonymous of magma-rich margins; the TWTT of first volcanics may provide a better approach to distinguishing magma-rich margins from margins with normal magmatic addition". Again, this has not clearly been discussed before coming to the summary. Your whole study (observation + model) showed that the thickness of volcanic material [and accommodation space as a consequence] also distinguishes magma-rich and magma-normal margins (and not only TWTT of first volcanics). Why do you only focus on the TWTT of first volcanics to determine whether it's a magma-rich or magma-normal margin in the summary and abstract? Is it because it's easier to measure than thickness of volcanics and accommodation space? That would be a good reason but should be discussed in the text.

We understand reviewer 1's comment and you identify an important point. We will increase the explanation and discussion of the observation of TWTT of first volcanics and its correlation with volcanic volumes. This is perhaps the most important result of the paper and at present it is perhaps slightly hidden.

Also, the age of the margin might also play a role here. Do you think that this boundary would also be at 4s TWTT on a very recent margin with almost no post-rift sediments yet and less thermal subsidence? I'd say no (although I do agree that after a while, thermal subsidence is close to 0 and the accommodation space above the first volcanics is anyway filled (as on your example)). In the case of a very young margin, the thickness of magmatic material would be the best parameter to determine between magma-rich and magma-normal margins. This brings us back to the previous paragraph: why is the TWTT of first volcanics a better method that other methods? This limitation should probably be discussed in the text.

Reviewer 1 raises an important point. The age of a margin will affect the TWTT of first volcanics because a young margin will not be thermally equilibrated. We will explain this effect in the revise text.

Suggestion of improvement for the structure of the discussion:

As seen from the comments above, the reasoning path of the discussion section could be changed to something like:

We see several differences across the strike of the margin: thickness of volcanics, accommodation space and TWTT of first volcanism (2 vs. 4/6s) ➐ We relate this to a change in the timing and volumes of magmatic production ➐ Our model allow to test this rather we think the model allows us to explain the observation ➐ Thickness of volcanics, accommodation space and TWTT of first volcanics of the model fit real-world observations (story of 6 vs. 4s and inclusion of sediments in the model apart) This is shown in the Chenin et al 2023 paper – we need to expand the text to explain this in more detail ➐ These differences in thickness, accommodation space and TWTT of first volcanics can be explained by a different volume and appearance time (or beta factor) of decompression melting (along with the other arguments you already give in section 5.1) as is shown in the Chenin et al 2023 paper ➐ Both thickness of volcanics, accommodation time and TWTT of first volcanics provides a way to determine magma-rich margin more reliably than the presence of SDR. Yes! ➐ Because TWTT of first volcanics is easier to measure, it probably provides the simplest way to determine magma-rich margin (or another argument as to why this parameter is important) Yes! ➐ then a last section of the discussion with current section 5.2 about accommodation space (including that it is also confirmed by the model). We will increase the discussion of this in the text

That seems like a big change but basically you can just copy and paste most of your existing text. But that's only just a suggestion to use your modelling to support all your points and make your discussion clearer, more relevant and more impactful. You might elect to not follow it at all, that's not a problem.

We understand reviewer 1's comments above and will modify the text.

Summary (lines 282-298): As discussed before, you might consider removing the Rio Grande Cone story as it not the most important outcome of the study. Anyway, I'd maybe put the point "The observed inverse relationship between post-breakup accommodation…" right after the point "Post-breakup accommodation space correlates inversely with SDR thickness…" as those are linked.

See earlier response to this – the flexural backstripping corrects for the variable sediment supply along the margin

Line 296, this 6-7s has to be clarified. 6-7s is only from the model at the moment, not from seismic data.

See earlier responses to this point. Reviewer 1 is correct and we need to amend the text and figure to be consistent.

Line 297-298: In light of the discussion above, the sentence could be slightly changed to "may provide the simplest approach to distinguishing …"

Good point – we agree and will amend.

Abstract:

Maybe linked with comment 3 above, I'd also maybe reshuffle the order of the sentences of the abstract to put your model upfront [but this is just a suggestion]:

As discussed earlier, we see the paper as an observational paper rather than a modelling paper. We use the model only to try to explain the observations.

"We show that post-breakup accommodation space correlates inversely with SDR thickness, being less for magma-rich margins and more for magma normal/intermediate margins. *The observed inverse relationship between post-breakup accommodation space and SDR thickness is consistent with predictions by a simple isostatic model of continental lithosphere thinning and decompression melting during breakup.* [The Rio Grande Cone, with large sediment thickness, is underlain by small SDR thicknesses allowing large post-breakup accommodation space.] A relationship is observed between the amount of volcanic material and the TWTT of first volcanics; first volcanics are observed at 1.25s TWTT for the highly magmatic Torres High profile while, in contrast, for the normally magmatic profiles in the south, first volcanics are observed at 4.2s TWTT or deeper."

Technical Corrections

Figures

Figure 1 : The figure overall looks nice but some improvements are possible. For me there is a confusion between the legend and the caption and it is not clear what are the SDR (should be grey but I am confused with the colour of the belts), the Belts, the Basement from Stica (is it the same as the cratons?). For clarity, you might think of removing the Belts which are barely discussed in the paper. I'd also remove the Rio Grande Arch and the Torres Syncline which are not discussed at all in the paper. Also, it would be nice to indicate the Perolas margin (trivial for you but maybe not for everybody).

We agree - reviewer 1 makes several good suggestions here – we will revise the figure following that advice to make it clearer.

Figure 2 : The caption for a) and b) seems to not match the figure (maybe not the correct version). a) only show profile S1 and b) only S3. Both a) and b) show surfaces + units.

Agreed – we will correct the caption text

Figure 4: Panel b): I found a little confusing to have this black triangle on the graph. Wouldn't it be better to have it on the axis (e.g. "Max vertical SDR TWTT thickness (s)" and "Post-rift sediment thickness TWTT at max SDR thickness (s)")? Just a suggestion. Captions: maybe "at the same horizontal distance on the profile" sounds better than "at the same location". I had to scratch my head to figure out what it meant. As you want.

Good point – we agree. We will amend the figure.

Figure 7: "Figure 7" is written twice (Nice figure BTW).

Error noted – thanks – we will correct

Figure 8: What is this vertical line, small horizontal line and small circle on panel b? It seems that the black dot represents the TWTT of the first proximal SDR but what are these other symbols? This is nowhere explained it seems and I cannot figure it out.

We agree – this is not adequately explained and is confusing – the vertical lines show the range of uncertainty. We will amend figure 8b and improve text.

On panel c) and d) it would be nice to add a vertical axis with TWTT as this is the core of what you want to show with this figure. (Ok, you have a scale, but it's not easy just with this scale to know where is 4s or 6s.

Understood – but the owner of the seismic requires this presentation format of TWTT scale.

Text

Lines 53-56: Could be nice to have at least one reference for these Feliciano Belt and pre-rift geology if any reader want to know more on this topic.

Understood – will add reference

Line 88: "top basement remains parallel" parallel to what? To Moho I imagine.

Understood – we will explain this more clearly

Line 104: "at approximately 30 km": maybe good to say 30km from what (from eastern part of the line? From coastline?) or to remove it altogether as it is not the point of the sentence.

Understood – we will amend text

Line 130-131: You might add a "mainly" (sediment supply being *mainly* controlled by factors external to margin formation) as margin formation can also influence drainage system and thus also sediment supply. (Sorry, I'm picky but I like the topic :-)

Understood – point taken – will amend text

Lines 145 ff.: I was confused with the lithospheric thermal re-equilibration. I struggled a long moment to understand how you integrated it until I realized you probably did not include it as you want to know "the bathymetry that would exist at present if no post-rift sedimentation had occurred." And post-rift sedimentation barely has an influence on thermal re-equilibration. Maybe it's not bad for ignorant people like me to mention somewhere how you handled it. What do you think? Or did I misunderstand something?

Reviewer 1's understanding is correct – we are not rewinding post-breakup thermal subsidence. We will add text to make this clearer.

Line 167: "subduction dynamic subduction": One subduction too much and a missing subsidence.

Well spotted – we will correct

Line 178: " The Austral segment of the South Atlantic margin of South American" a word missing

Understood – should read "The Austral segment of the South Atlantic margin of South America" – we will correct

Lines 204 and 206: Where is Rio Grand do Sul? It seems to be nowhere on your maps. Probably good to include it on fig. 1.

Ah – Rio Grande do Sul is the most southern state of Brazil (and where the first author comes from) We will amend the text to make this clearer..

Line 247: Chennin with one "n"

Thanks - we will correct

References:

- Rossetti et al. not in alphabetical order.
- Warner (1987) missing.
- Maybe not bad to check for other mistakes in the references.

We will add and correct references