General Comments

In this manuscript, Cassel et al. present seismic data from the South Atlantic and discuss the along-strike 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?

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 :-)).

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?

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.

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 magma-rich/magma-normal margins.

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?

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.

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.

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.

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.

Comment 3:

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

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?

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.

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.

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) → We relate this to a change of magmatic production → Our model allow to test this → 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) → 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) → 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. → 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) → then a last section of the discussion with current section 5.2 about accommodation space (including that it is also confirmed by the model).

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.

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.

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

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

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]:

"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).

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.

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.

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

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. 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.

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.

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

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.

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 :-)

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?

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

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

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.

Line 247: Chennin with one "n"

References:

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