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Solid Earth
Discussions

Interactive comment on "Rift zone-parallel extension during segmented fault growth: application to the evolution of the NE Atlantic" *by* Alodie Bubeck et al.

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Thanks for the opportunity to review this paper. The topic of the paper is both timely and of very broad interest in the structural geology and tectonics community, but also for geophysicists, volcanologists, basin analysts etc. The paper appears scientifically sound, although certain aspects of the paper need to be clarified to confirm that. The paper presents finds and ideas that are indeed interesting and worthwhile exploring, but with room for improvement in better delineating the relationship between ideas presented herein and previous work by other authors. A very positive aspect of the paper is the fact that it draws on data from different fault systems around the world,

and also draws comparisons to global examples. With this the authors have placed their paper in a class of highly generic and applicable papers that would be of interest across a range of disciplines. As such the paper is well within the scope of Solid Earth. The paper is mostly well written and well illustrated, but there is also room for improvement in terms of content presentation. More about that later.

In my view what is presented in this manuscript could ultimately form the basis for a fine publication in Solid Earth, but that there are important issues that the authors need clarify/address first. My comments include discussion points/questions (in the spirit of the open review process some of these may be discussed in the paper, although some may be too broad or out of scope but could form the basis for an interesting discussion in the forum) as well as remarks/comments/suggestions/concerns relating to the science, or being of a more technical or editorial nature, or relating to presentation. Please consider all of the below friendly suggestions for discussion and, hopefully, improvement.

The main issues break down into five main categories. For elaboration on these issues, please scroll further down to the bulk of my comments, or refer to the annotated PDF.

1. There is confusion related to the authors' use of the term 'coherency' (and several other terms), and how they relate to processes fault growth. This creates confusion throughout.

2. Methods are not adequately described. Very importantly, it is not clear how heave or throw were recorded.

3. The results sections are mixed with background material and interpretations.

4. Some of the interpretations (e.g. those presented in Figures 2c and 4c) are hard to reconcile with the evidence presented.

5. As a result of the points 2, 3 and 4 above, I find it difficult to fully grasp the discussion and how it relates to the results/data presented in the paper. I suspect it will be much

easier to critically assess the discussion when the other points have been addressed – and I'd be happy to comment further on the discussion once this has been done.

In the following I go into depth about the issues above – and there are also many comments in the annotated manuscript PDF.

Abstract:

- at some point in this paper, it would be useful to clarify what you mean by 'mechanical interaction' since you use that term quite a lot. The term is defined in Peacock et al. (2016, JSG 92, 12-29), is this what you mean? In terms of interaction, linkage and the presence or absence of kinematic/geometric coherency, I think many would agree with me when I say that there are loads of ambiguous terms, so perhaps a short terminology section would be in order where you define what you mean by various terms (I will list other terms that need clarification as we go along).

Introduction:

- P1 L25: "Studies using natural examples and numerical or scaled-analogue modelling techniques have shown that normal faults grow through stages in which initially offset segments propagate towards each other and link to form composite structures (e.g. Trudgill and Cartwright, 1994 Gupta and Scholz, 2000; Peacock, 2002)" – true, but there are also now several studies that suggest otherwise, namely that fault growth may not not be dominated by a tip-propagation behavior, but that faults and fault systems (even if segmented) appear to establish their full length very early on (e.g. Walsh et al. 2002; Giba et al. 2012; Jackson & Rotevatn 2013; Nicol et al. 2016; Childs et al. 2017; Jackson et al. 2017). This is known as the 'coherent fault model', which has later been rebranded as the 'constant-length model' and derivations thereof. The fault growth model that you invoke in the first sentence is that of a sympathetic increase in displacement and length over time during fault growth, which some refer to as 'isolated fault growth' (I personally think this name is misleading), and which has dominated the structural geology literature for decades. It is very important to note that neither of the

models precludes linkage, interaction, relays and whatnot, and therefore I think both are equally a sound foundation for the topics discussed in this paper. But, the reason for bringing this up is this: you must make a decision about how to deal with this – at present you seem to ignore the 'coherent' view on fault growth in favor of the 'isolated' fault growth model. But then later in the introduction you state that "displacement (throw) gradients on adjacent coherent normal faults are commonly accommodated by relay structures". The key here is your use of the term 'coherent' (you need to define this term; I am not sure what you mean by it), which leads me to ask whether i) you are now buying into the coherent fault growth model instead, or ii) if you think both models are equally valid, or iii) if you are just using these terms without really having a clear position on what your position is on the matter. I think the intro needs to mention both of the two fault models, or at least provide some rationale as to why you prefer one over the other. Or, if you think it is of less importance and beside the point of the paper, rather focus the intro on the fact that faults are universally segmented, and that the faults/segments interact (regardless of whether they grow according to the 'isolated' or 'coherent' view on fault growth), avoiding this problem altogether. If you do this, I would get rid of the first sentence, and lead in with the sentence in L28-29 (starting with "Segmentation is a feature. . .". Having said all of this, I cannot help myself but wonder if (you think that) it matters whether faults grow according to one model, or the other, or a combination of the two. Does it make a difference in terms of what your findings mean? Or how applicable your findings are? Or, turning it around, can your findings shed any light on models for fault growth? Again, you may feel that it is beside the point of this paper. Nevertheless interesting as a general topic for discussion in the forum, paper or elsewhere.

- define 'mechanical interaction'

- This is perhaps pedantic (P1 L36), but heave gradients are not horizontal displacement gradients, since heave is not horizontal displacement per se. Heave is simply the horizontal component of the displacement, so in my opinion it would be more correct

to talk only of heave gradients.

- define ‘coherent normal fault’ – this becomes especially important since you in the first sentence appear to ignore coherent fault growth as an alternative to the more popular isolated fault model.

- what do you mean by the term ’growth fault’ here? Do you mean it as in syn-sedimentary growth faulting? If yes adding using the term ‘syn-sedimentary growth fault’ would add clarification, at least for me. Others might passionately disagree. This does not become more clear later in the paper, so there is a need to address this.

- I do not feel that the intro fully lands the aims of the paper – I am still not fully aware of what the main mission of the paper is at the end of the intro. I would recommend this be discretized somewhat.

Background

- it breaks the flow up a little to have a new intro chapter here, called ‘Background’ – consider combining Intro and Background chapters to streamline the introduction of key concepts, identify key problems/knowledge gaps and state discrete aims and objectives. At present (as pointed out in the last comment to the Intro chapter) the paper does not in my opinion express very clearly what the key mission of the paper is.

- P2 L21: here you are back talking about a ‘coherent system’ – you need to precisely define what you mean. This also relates back to the first major point about the ‘coherent fault model’. What is a (kinematically?) coherent system? To you?

- You are leaning quite heavily on Tentler & Acocella (2010). Is it possible to broaden this by looking at what other studies have said about deformation/strain and the degree of underlap/overlap, separation, etc? Names like Long and Imber come to mind.

- P2 L36 onwards: “In nature, however, the process should graduate through some or all of these stages, as the bounding structures propagate toward each other and link, subjecting the relay zone to distortions as a function of the changing fault cut-off line

lengths associated with slip accumulation (Fig. 1c,d).” Back to the first point again. We would like to think it propagates and links (I have done the same in many of my own papers, but looking back I think this view may be too simplistic) but is it also possible that the linkage and rotation of the relays is a process of subsidence (displacement accrual), relay rotation and breaching (but no or limited tip propagation of the main faults), rather than propagation and linkage?

P2 L43 – again you talk about coherency. It is becoming increasingly important that you address this and clearly define what you mean.

[Methodology]

- The paper is weak on describing the methodologies used. I was left wondering for a long time if the paper actually was based on outcrop work, subsurface (the mention of growth faults pushed me in that direction, but that’s a personal bias), or on imagery/remote sensing – or a combination. Stating in brief early in the paper what methods are used, and adding a methodology section would help more clearly explain the nature of the work. I am still not sure if the authors have visited all of the field sites, or if some of this was entirely based on mapping aerial/satellite imagery. This is of course very important for the resolution of the dataset, and has implication for what the throw/heave/extension plots represent. The only places where fieldwork or field mapping is mentioned is in the abstract and the acknowledgements. And there is no detailed description of what was actually done. There is some very general information under each field site, but not specifically what kind of data was collected, and how it was recorded. And what type of imagery has been used to compliment outcrop work? I assume part of the mapping was done based on the imagery in the figures, but the text does not specify. All of this could be added with ease and would make the paper much easier to follow.

- Very importantly: it is not clear how heave measurements were recorded, or how throw measurements were recorded (or estimated? – see comments to Figure 3). This

needs to be crystal clear, or the results will not be reproducible.

[Terminology]

- Some terminology is not clearly defined (see previous comments – coherency is one important term here). I would ask the authors to consider introducing a short terminology section that removes doubt and confusion related to key terms used in the paper.

Field study areas

- P3 L 24: What do you mean by 'growth faults', and what do you mean by 'interpreted to be growth faults'? On what basis? Please clarify. What does forceful emplacement of dikes have to do with whether they are growth faults or not? Whose interpretation is this? Yours or that of the cited papers? It is quite confusing that this section is a mixture of background, results and interpretations (see several comments in annotated PDF).

- Figure 2 confuses me. Part C lacks a proper legend and requires deep concentration to reconcile with part B. Hard to follow. Better legend/annotation would help (e.g. it is not immediately obvious in part C which of the structures correspond with sets a,b, c, d in part B). More importantly – the proposed evolution concerns me because the yellow structures in C bear no resemblance to the yellow structures in B. In C (the proposed evolution) they are almost perpendicular to the main rift strike, wheras in B (the actual studied system) the yellow structures strike at c. 45 degrees relative to the main rift strike. Another point about Figure 2 is the arrows that indicate dip directions: Do all faults dip to the N-NE? Are there no antithetic faults here that dip to the S-SW?

- what is your evidence for the relative ages of the different sets? For example for set D post-dating set B as you seem to suggest in your proposed evolution in Fig 2c

- Figure 3 is also confusing to me. Part B: The legend vertical axis records 'heave', but the legend talks about 'total displacement' and 'extension'. This is somewhat confusing. You can address this by naming the axis 'heave/extension' and changing 'total

displacement' to 'total heave'. It is also quite difficult to see the difference between the different line weights/dashing/colors – you might want to make it clearer. Part C: the caption states this is 'extimated vertical displacement' – what do you mean? How was it estimated? Was it not measured? Also you state that "no evidence for throw was identified along NW-SE striking fractures of set B". What do you mean by no evidence for throw? What constitutes evidence of throw? Seems like a peculiar way to get at this. If there is no throw they should be opening mode fractures or vertical strike-slip faults – you should be able to describe this in clearer terms.

- I have similar reservations with Figures 4 and 5 so please apply the above suggestions/comments to these figures as well. Again I find it hard to reconcile the orientations of the faults in the evolutionary models with the real data.

- P4 L6-14: since you are not specifying how heave, throw, extension direction (? Yes, you mention this as a measured parameter) etc were measured, it is difficult to understand what these numbers mean and their significance.

- P4 L15 onwards: "With no evidence for the relative timings of the bounding and linking fracture sets, simultaneous orthogonal extension directions have produced an area of inherently 3D strain within the relay zone. A kinematically and geometrically coherent fault array (e.g. Walsh et al., 2003) should exhibit an approximately centralized displacement maxima; this is not the case for horizontal (heave) displacement where we find a prominent extensional strain deficit in the centre of the array (Fig. 3b,c)." I have a hard time understanding where you are going with this. Are you saying that you expect all fault arrays to follow an idealized bell-shaped displacement distribution? Is folding etc accounted for in your plots? And here you are talking about a kinematically and geometrically coherent fault array – back to the first comment again. Please clarify what you mean and how this is significant. Why does the array have to be kimenatically and/or geometrically coherent? What if the array is not kinematically or geometrically coherent? Is that an option, and what is the consequence of that? Not that it belongs here, it belongs in the discussion.

- the Krafla chapter struggles generally with the same issues as those pointed out above for the Hawaii dataset (mixing of description with background material, interpretations and discussion plus the other issues listed above). Please revisit this section too, using the Hawaii comments as a guideline.

- Section 3.3 – what do you mean by first-order and second-order faults – do you mean in terms of chronology (you said earlier there were no evidence for age-relations between the Hawaii sets)? Orientation? What sort of hierarchy is being introduced here, what is it based on? This statement: "Fracture sets that strike at a low angle to the main rift zone (<45°) show extensional-shear opening (e.g. Krafla: Figure 4)," – relates back to section 3.2 – what do you mean by extensional-shear opening? Are we talking about an opening-mode dominated fracture with a minor component of shear displacement? Or an oblique-slip dominated shear fracture (or oblique-slip fault if you like) with a component of opening? Better description and documentation of the studied structural features would help a lot. For example, it would help tremendously if you provided a picture of what you call extensional-shear opening fractures – and if you would explain how you measure the displacement across such a structure.

Discussion:

- since the methodology and results sections have some unclarities related to i) how key was recorded, ii) the mismatch between data and proposed evolutionary models, and iii) the mixing of data and interpretations/discussion, I had a hard time grasping the discussion properly. The discussion raises a series of interesting ideas – but the way the data in the paper is presented (i.e. lacking proper explanation of how key data was recorded, and the mixing of results and interpretations) it is not helping me to see how the discussion is (or isn't) backed by what the authors present to us. I think therefore, that the best course of action would be if the authors addressed the issues with the manuscript leading up to the start of the discussion – and that the discussion/conclusions can only be fairly evaluated once that has happened. I have indicated to the editor that I would be very, very interested to look at the discussion

once more when the mentioned issues have been addressed.

Nevertheless I still have some general remarks to the discussions/conclusions at this point:

- how is what you are proposing different from what has already been proposed by previous authors in terms of local perturbation of regional stresses (e.g. Kattenhorn et al. 2000) – what makes the findings of this paper unique compared to previous studies? In other words, can you try to better delineate the relationship between your work, as presented here, and that of previous authors?

- is there a risk that, perhaps merely as a function of your choice of words, you are exaggerating the significance of rift-parallel extension? Localized extension on structures forming at high angles to general rift strike is hardly attributable to regional rift-parallel extension – I am not saying you are suggesting it, but in my opinion your choice of words/terms to describe this makes it sound like a regional, rift-wide effect rather than something that occurs localized to relays or steps in the rift axis. I am not sure I fully understand exactly what you are suggesting here. It would be very interesting to revisit this when the rest of the paper is more clear.

Conclusions:

- the conclusions embody some of the confusion I experienced when reading the discussion (I would completely rephrase the conclusions), and also underscores some of the problems related to terminology. I am not repeatedly addressing terminology to be pedantic – but unclear use of terms introduces confusion as to what the authors really mean to say and therefore it must be addressed to improve the clarity of the paper.

"Discontinuous normal faults in the Koa'e and Krafla fault systems accommodate regional horizontal extensional strains via a combination of fault throw and heave on first-order rift faults, and by obliquely oriented second-order deformation, driven by heave displacement gradients and vertical axis block rotation within the intervening

relay zones."

- Accommodating "accommodate regional horizontal extensional strains via a combination of fault throw and heave" is in my opinion a quite peculiar choice of words. Strain is accommodated by means of faulting, the displacement of which can be broken into throw and heave, but throw and heave are not separate 'mechanisms' that accommodate strain. - "Obliquely oriented second-order deformation" I also find to be fairly vague, and "heave displacement" (an imprecise term) is not a driver of anything – it is just the horizontal component of the total displacement.

"Second-order deformation within the two studied relay zones accommodate components of the regional extension, but locally accommodate components of extension in a direction parallel to- and oblique to the rift zone."

- Again, as pointed out earlier (in the PDF comments), I do not understand what you mean by introducing a hierarchy of first order and second order deformation/structures – particularly when you have no chronology in at least one of the study areas. Unless you clearly define what first- and second-order means, such terms have limited meaning. - "but locally accommodate components of extension in a direction parallel to. . .." and so on. . . to me, talking about extension directions like this implies something more regional – in my opinion it would be more helpful to discuss this in terms of local stress reorientation and opening directions of specific structures/trends, rather than talking about extension directions which has a mega-scale flavour to it. And that is not the intention, is it? If it is, I will be even more confused.

"Locally heterogeneous fault populations within relay zones are attributed to locally non co-axial stress states associated with mechanical interaction and resulting fault displacement gradients rather than polyphase tectonic episodes."

- What is a heterogeneous fault population? A fault population with more than one set of different orientations? Mechanical interaction and resulting fault displacement gradients also feels very vague and general, and would better be replaced with something

more specific about the effectcs of such interaction, and the specific of what happens to the displacement gradient (those gradients could be quantified).

Final remarks

As I said initially, this paper represents a scientifically valuable and interesting study – but as is probably clear from my comments I did scratch my head a fair bit during reading. I think the authors can, relatively easily, clarify most of the issues I have pointed out in the text and figures leading up to the discussion. That leaves us with the discussion – and I think the authors must actually do the changes to the rest of the paper before we can see full breadth of the connection between results/data and the discussions/conclusions of this paper. It is the editor who must decide whether (parts of) the paper needs to be re-reviewed, but I would certainly be happy to have a second look at the discussion in a revised manuscript.

I thank you again for an interesting read and for the opportunity to evaluate this interesting piece of work. I look forward with great interest and anticipation to the next steps of this exciting and fully open review process. Do not hesitate to contact me if you would like to discuss my review comments.

Dunedin, September 19th, 2017. Atle Rotevatn University of Bergen (presently on sabbatical at the University of Otago, New Zealand)

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Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2017-94/se-2017-94-RC1-supplement.pdf

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