

Interactive comment on “Oblique rifting: the rule, not the exception” by Sascha Brune et al.

Anonymous Referee #2

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This paper presents a very interesting global quantitative study of rift obliquity and concludes that much more rift segments are oblique than previously admitted. This study is well documented and well described, with focus given on few well-known oblique rift examples. The introduction and discussion are more focused on the geodynamic implications and the mechanical reason that could explain their observations. I have only a few minor comments, the first one concerns the choice of the author to consider the COB as the rift trend to define obliquity which should be discuss a little bit more in view of existing analogue and numerical model and the second concerns the mechanical discussion at the end of the paper, which is too light and could be improved a lot.

First point : At line 5 page 5 or line 13 page 26 The authors state that the COB is an indicator of rift trend and use it to define obliquity by comparing with the direction of plate. It is a choice, which is not better nor worse than another choice but it is arguable, and I would like to see a bit more argumentation in the text than it is a better choice

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than the coast line. The COB is an indicative of the trend of the rift in the lithospheric mantle that is in the very late stage of rifting after the mantle necking. 3D models show that the COB might have a different obliquity then the initial trend and that with time obliquity of rift structure might increase as the lithosphere become weaker and weaker or decrease.

Second point: Based on a previous paper by the first author, this contribution explains the predominance of obliquity is that oblique rift requires less forces than orthogonal rifting. I think this analysis is biased and not complete because 1/ oblique rifts only emerges when transtension is imposed either by the BC's or the initial weak zones (thermal or mechanical) imposed on models. If their statement was indeed true, models of propagating rift segment would all produce oblique rift and none of them do (Van Vijk and blackmann 2005, Allken et al. 2012, Mondy et al 2017, Le Pourhiet et al. 2018) actually do unless a oblique weak zone is imposed (Molnar et al. 2017 2018, Balazs et al. 2018) or propagation stagnates (Van Vijk and blackmann 2005, Allken et al. 2012, Le Pourhiet et al. 2018)

2/ the authors completely dismiss mechanical or thermo-mechanical models that simulates oblique rifting using the standard set up of Le Calvez, and Vendeville, (2002), that is: two offset weak zones embedded in a rectangular box with imposed cylindrical boundary conditions (now, renamed as transtensional by Zwaan et al. 2016). In these set up, the linkage zone forming between the weak zone in Allken et al 2012, Liao and Gerya 2016, Le Pourhiet et al. 2017 , Amman et al. 2017, or Balazs et al. 2018, are also an oblique rift segment in your definition (COB vs boundary condition). Yet, if ones consider, the surface area and the number of faults as a proxi, the work required to rift this oblique segment is much larger than to rift the orthogonal segment. The main difference between these set-ups is that in one case (Brune et al 2012) the there is a pre-existing linear weak zone in the mantle lithosphere (i.e. the necking zone is pre-imposed) while in the other, it forms self consistently when crust and mantle deformation couple (i.e. in the late stage of rifting).

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I think, that given the expertise of the first author, a more thorough discussion of the role of heritage, rift propagation or lack of it, and linkage as possible explanation of the omnipresence of oblique margin could really improve the paper a lot without lengthening it by much.

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