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Discussions

# *Interactive comment on* "Kinematics of the South Atlantic rift" *by* C. Heine et al.

**Anonymous Referee #1**

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Review of 'Kinematics of the South Atlantic Rift' by C. Heine, J. Zoethout and R.D. Müller, submitted to Solid Earth, a new online journal published by the European Geophysical Union.

The geometric fit between South America and Africa has been generally known since the earliest days of continental drift. A detailed fit, however, has remained elusive. One reason is that both Africa and South America did not behave as large rigid plates during rifting. This deformation has to be taken into account to create a fit between the plates that does not include unacceptable gaps or overlaps. Again, this has been known since the earliest reconstruction attempts, but quantifying this deformation has remained elusive. Another reason for the ongoing reconstruction debate is that the amount of extension along the conjugate margins is poorly known, resulting in large uncertainties in how the margins should be restored to pre-rift geometries. This paper presents the authors' contributions to this debate, with restorations of both internal



deformation and extensional margins. The paper falls far short of settling the debate, however, because very little of the background data for derivations of the quantitative restorations are shown. This makes it very difficult to compare these results with those from other similar papers.

Some general comments first: The paper contains several grammatical errors (I assume there will an editorial step in the publication process so have not gone into details) and some glaring typos, the worst being the capital 'M' in several words that start with the letter 'm'. This must be the result of some global change, but Makes reading the paper Most annoying. Another issue is the format of the publication. Given that this is an online journal, it is presumably optimized for reading on a reasonably-sized computer. In this, the text works well. It displays with a large menu banner on the right-hand side of the screen, with buttons for navigation to references, figures and so on. There is also an option for a printer-friendly version. This may be printer friendly but it is not tree-friendly, with large amounts of unnecessary white space and a distracting vertical banner on the right repeatedly reminding us that this is a 'Discussion Paper'. The references (and there a lot of them) print with the same large font as the main text, also an unnecessary waste of space/paper. The figures, on the other hand, are very difficult to work with on a computer and the 'printer friendly' version is no better, with 2 figures crammed on each page. This is really where a breakthrough is needed in online publications. There must be a better way to show figures than by simply including them as static images in a pdf file. The figures in this paper would really benefit from being made available in an environment like Google Earth, with the ability to have overlays that can be switched on and off and queried. Figure 4, for instance, is incredibly busy. With all of Africa at only 6.5x5 cm on the printed version, it is almost impossible to decipher. It has background color representing topography, gravity and sediment fill and patterns representing SDRs and volcanics, Proterozoic basement and salt basins. There are also symbols for about two dozen basins (some of which barely have space for a pixel of the sediment fill grid) and other features. This large amount of information can be very easily displayed on Google Earth and could be done by making the figures available in Google Earth's kmz format (or some other useful format, I'm no expert). The figures were made using GMT; I don't know whether GMT can make kmz files, but there are several GIS packages that can. It would be very useful to have figures available in a digital form, much better than simply a static image; EGU should investigate this further and encourage inclusion of some more interactive file format in online papers. Ask Reader's Digest what happens when the online world is assumed to simply be display of printer-like content on a screen.

Back to the paper: Page 42 line 18 (hereafter denoted as P42:18, using the regular, i.e. non-printer, version) - another global capitalization typo: Pre-salt should be pre-salt like pre-breakup.

P47:26 to p48 - This is the section describing restoration of passive margin extension. A first comment is that the methodology used needs a lot more clarification. The authors use crustal-scale cross sections which are restored using a constant-area technique to determine the pre-rift location of the edge of oceanic crust (their LaLOC). The technique is fine, but what is lacking here is any discussion of how their 10 cross sections (Fig. 3) were used to constrain the restorations along the entire 14,000 km or so of the South Atlantic margins. The 10 cross sections are not located on a map, they are simply denoted by general area. There are two that may be from the same area, one in the Kwanza Basin and another labeled as 'GS Grid South'. Although these would seem to be the same geographic area, the cross sections are very different. I would also expect the Kwanza section to look similar to the section in Unternehr et al. (2011), but it is very different. The Unternehr section has a large landward-facing scarp near the LaLOC, whereas the one in Fig. 3 has a large seaward-facing scarp near the middle of the line. Unternehr also postulates mantle exhumation, not shown in Fig. 3. Why the difference? With apparently significant variations within this small area, it would seem that only 10 sections for the entire South Atlantic is very inadequate. More explanation and location of sections is very much needed. Some sections are restored to 36 km Moho depth, others to 32 km, with no explanation for the difference. A glaring



omission is a cross section across the Santos Basin. The LaLOC in this area is more than 500 km from the coast (800 km if measured along the rifting direction, see Fig. 11), so restoration of extension here is key to fully understanding the South Atlantic. Some discussion of how this area was handled is essential. The authors do assure us (p48:28) that their interpretation for the Brazilian margin matches that of Chang et al. (1992), but no visual confirmation is presented. Chang et al. (1992) did their restorations based on estimated extension factors (beta) and did not take rifting direction into account; their Santos line is at about 45° to the rifting direction, so some assumptions about structural evolution that are not discussed in this paper must be made. The Chang paper was also written before recognition of mantle exhumation processes, so this needs to be taken into account – see Zalan et al. (2011). Another omission is discussion of the influence of volcanic margins. Sections are shown for Pelotas, Walvis, Orange and Colorado, all of which cross volcanic margins with SDRs. How was the extent of continental crust across these margins determined? On the reconstructions, Fig. 17 onward, the LaLOC is sometimes inboard of the SDRs, sometimes within and sometimes outboard. Explanation needed. A final point about margin extension: the LaLOC is only shown on reconstructions with oceanic crust. It should also be shown on older reconstructions as lines on each plate that track the authors' estimates of evolving extension on the conjugate margins.

A general point about the references in this paper: This section 2.3 (p47:26) starts by referencing 13 papers that have apparently presented crustal-scale cross sections. But according to the limited discussion of Fig. 3, only one paper was actually used, that of Blaich et al. (2011). This results in many references that are simply there to acknowledge existence of the paper, which helps explain why there are nearly 200 total references in the bibliography. This number could be greatly reduced by just citing work that was actually used.

P49:13 – data are plural, refer to 'these data'.

P49:18 – What does 'M7 has been identified on both conjugate abyssal plains closed to the COB' mean? I don't quite see why, if M7 is identified on both sides, M4n is used as the oldest isochron. What has breakup-related volcanism got to do with it? If these chrons are on the abyssal plain, would there necessarily be associated volcanism? Also, another confusion – LaLOC is used earlier, now we're back to COB?

P50:1 – this introduction to non-rigid continental plates is clumsily written. Again, a long list of references going back to 1982 (which is hardly 'recent') is inserted as proxy for a critical evaluation of previous work, with no breakdown as to which were useful. How about this:

Burke and Dewey (1974) pointed out that Africa did not behave as a single rigid plate during Cretaceous rifting of the South Atlantic. They divided Africa into two plates separated along the Benue Trough-Termit Graben (WARS in fig. 1). Subsequent work identified another rift system trending to the east from the Benoue area (Fairhead, 1988; Genik, 1992; CARS in Fig.1). There is less evidence for major deformation in South America, although several continent-scale strike slip zones have been postulated (see Moulin et al. 2009 and references therein). In this paper we define four major plate boundary zones and extensional domains: the Central African (CARS), West African (WARS), South Atlantic (SARS), and Equatorial Atlantic Rift Systems (EqRS). We here include the newly-identified "Patagonian extensional domain" and related smaller rift structures in southern South America in our definition of the SARS (Fig 1). From these four extensional domains, only the SARS and EqRS transitioned from rifting to breakup, creating the Equatorial and South Atlantic Ocean basins. In the next sections we review timing, kinematics, type and amount of deformation for each of these domains.

This drops 6 references and I think looks less clumsy.

P50:18 – Africa. Again, far too many references. Do you really need 13, all the way back to 1974, to refer to WARS/East Niger? A more technical description is called for, describing what these authors saw to make you want to put in a plate boundary here. This same critique applies to the rest of this Africa section – a long list of references in



each sub-section but very little to show why all the sub-plates are needed in the model. It is not the reader's job to burrow through, in the case of CARS (p51:4-6), 8 references to find out what persuaded the authors to include CARS.

A comment on Figs. 4 and 7, the continental base maps: I have already mentioned the problem with putting so much information on figures that really don't show up well on a computer screen. Another is the projection used. Oblique Mercator projections are pretty obscure and without specifying the projection parameters used (pole, spheroid, datum), border on the useless. Why were these projections used? If the authors do keep them, the full specifics of the projections need to be given.

Paragraph starting on p52:26 – The original interpretations of strike slip in the Doba-Dosea-Salamat basins have been questioned, see http://www.searchanddiscovery.com/documents/abstracts/2004hedberg_baku/extended/reynolds/reynolds.htm I think this interpretation of less strike slip actually fits better with the interpretations in this paper (see Fig. 5) but do raise other issues as discussed below.

Another mapping comment – I found myself wishing that the faults mentioned in the text were shown on a map early on. The Borogop fault is not shown on Fig. 4, the Africa base map, but does appear in the reconstructions starting with fig.12. The Central African Shear Zone is not shown on any map yet is apparently an important province boundary. I almost hate to ask for more information to be on the maps, but perhaps a Fig. 4b with major structures labeled would work.

P53:16 – With published estimates of extension ranging from 15 to 56 km in the Muglad basin, a summary of reasoning for the choice of 35 km is needed, otherwise it just seems like an arbitrary choice.

P53:19 to 21 – Reeves et al. (1987) did not observe 'subsurface reverse faulting of Early Cretaceous age'. They observed isolated outcrops of Jurassic or possibly Triassic rocks co-located with a linear positive gravity anomaly. They suggested that the structure formed during a compressive event associated with the Miocene East African

Rift. Heine et al. need to explain why they interpret the structure to be Cretaceous in age.

The rotation pole and flowlines for NEA-SAF motion are shown in Fig. 5. Having the pole located so close to the tectonic boundary creates some problems. It implies a large gradient of motion across the area. At the far west end of the NEA-SAF boundary in the Bongor Basin at 11°N, 15°E extension calculated from the pole in Table 1 is 57 km. This would seem too much extension as the basin itself is only about 80 km wide (Fig. 5). At the far east end of the NEA-SAF boundary in the Lamu Basin at 1°S, 42°E (east of the Anza compression mentioned above and off Fig. 5) implied compression is 44 km. This too would seem to be far too much deformation, especially compression in an area where only extension is reported in the literature. Clearly this choice of rotation pole needs some more justification.

Paragraph starting on p54:27 – Discussion on extension in the Termit region. The chosen extension amount of 70 km in the Termit Graben would seem to be too much. The basin itself is only about 200 km wide (Genik Fig. 6), implying 54% extension [using the version of the calculation where % extension = (final-initial length)/initial)]. This is a Basin and Range type value which would appear to be inconsistent with the general high-angle bounding fault system in Genik (1992). The authors don't show their calculation that is apparently based on Genik's Fig. 9, but one thing to check would be the vertical scale. Genik's figure is based on a time section and has the depth scale converted from time, resulting in a non-linear depth scale. Make sure that the section was correctly converted to depth for the extension calculation. Show the calculation in a style similar to Fig. 3 in any case.

P56:18 in, not until

Section 3.2.2 starting on p60 – this provides details on extension assumptions in southern South America, the Patagonian extensional domain (although this section doesn't use the term, first introduced on p50). There is a disconnect between Figs. 1 and 7.



There are several basins shown on Fig.1 but not on Fig.7, CaGN, CaG, PdE and Vald. Leb changes symbol to Lev and MaN changes to NF. This region is pretty complicated but having different interpretations of basin geometry and names in the same paper doesn't help.

Similar comments about extension amounts used in Africa can be made for those used in this region – they seem to be consistently on the large size. For instance, in the Colorado Basin (p61:9) a value of 80 km is used, nearly double the value of 45 km given by Pangaro and Ramos (2012) referenced in the previous sentence. Why? More justification for values chosen is needed.

The rest of the paper shows the reconstructions and follows along logically. There are some points that should be explained, such as the geometry of the salt basins when salt was deposited. But I have pretty much used up the time I allotted to this review. I spent it on what I consider to be the important parts, the unraveling of internal deformation in Africa and South America, and these sections need more work and better justification of the models used. The actual reconstructions are totally dependent on choices made for deformation internal to the continents and rift margin deformation, so these need to be firmly established first.

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