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Discussions

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

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The manuscript "Kinematics of the South Atlantic rift" by C. Heine, J. Zoethout, and R. D. Müller presents high-resolution reconstructions of the South Atlantic rift and oceanic basin formation based on a comprehensive data compilation and supported by structural restorations. The main outcome is that the South Atlantic can be reconstructed without major intracontinental shear zones, structures for which geological evidence is partly missing, particularly in South America. A causal link between extension direction and velocity and marginal structures including the hyper-extended Brazilian-Angola margin is suggested. An initial phase of slow E–W extension in the South Atlantic rift basin from the Base Cretaceous to Upper Hauterivian caused distributed extension in W–E direction. The second phase from Upper Hauterivian to Base Aptian is characterized by increased extensional velocities and a change in extension direction to SW–NE. The last phase, culminating in diachronous breakup along the South Atlantic

rift and formation of the South Atlantic Ocean basin, commences at 120 Ma and is characterized by a further increase in plate velocities with a minor change in extension direction.

I think this paper is really worthy for publication with moderate corrections. Hereafter are a few comments that could improve the final paper. I am concentrating on the southern South Atlantic because I am not an expert on the central Atlantic and the rifts in Africa.

The main remarks are the preferred time-scale that in my view needs to be underpinned by a more extensive discussion (or needs to be modified), the formation of the volcanic seaward-dipping reflectors - the along-margin LIP, and the evolution of the Falkland Islands area.

Time-Scale

To tie stratigraphic ages to the magnetic polarity timescale predominantly used for global plate kinematic models, the authors have converted the estimates given by Gradstein et al. (1994) and Gradstein et al. (2004) to polarity chron ages which places base Aptian (Base M0r old) at 121 Ma. The difference of about 5 Ma is substantial. The most recent time-scale places base Aptian at 126 Ma. The base of the magnetic polarity interval M0r has been tied to the basal age of the Aptian stage in marine sequences with excellent biostratigraphic and magnetostratigraphic control (Channell et al., 2000). In the ICS (2010) report it is stated: "A wealth of data have been collected and published on the Aptian stage in the last few years by our French collegues on the stratotype sections of Bedoulian and Gargasian substages including revised biostratigraphies, δ13C curve and cyclostratigraphy. Sixteen papers, previously published on-line in Notebooks on Geology, have been assembled by Moullade et al. in volume 24/1 (2009) of Annales du Muséum d'Histoire Naturelle de Nice. Although magnetic signature in the French stratotype sections cannot be detected, carbon isotope data allowed a precise correlation between the base of magnetic chron M0, recommended at the 1995 Brussels Meeting for identifying the base of the Aptian, and the Aptian basal ammonite Deshayesites oglanlensis Zone." Walker et al. (2013) presented a recent review of the GSA time-scale that agrees for the Cretaceous period with the ages from the ICS.

The choice of a different tie here is based on “an extensive review of global spreading velocities, in which an older age of M0 of 125.0 Ma (Gradstein et al., 2004) would result in excessive spreading velocities (Seton et al., 2009; Torsvik et al., 2009)”. This is all what has been stated in the present manuscript. In the two cited papers there is at best some limited discussion on the choice of the time-scale. Only He et al., 2008 (not cited) provided age determinations for supposedly M0 anomalies in Yixian, China, indicating younger ages. Maybe this is absolutely clear for a plate modeler, personally I would like to see much more written on the “extensive review of global spreading velocities” in order to underpin the choice of the younger anomalies and to learn and understand which time-scale shall be used today, particularly for the Cretaceous.

Formation of the volcanic seaward-dipping reflectors (SDRs)

I do not see how the formation of oceanic crust could precede the formation of SDRs (as presented) given the fact that those are symmetrically emplaced at both margins. All interpretations and models on the formation of these volcanic flow units to my knowledge assume that these came in before that formation of the earliest oceanic crust. This needs to be explained or modified. In the proposed model (Suppl. 134 Ma) the SDRs came in after the formation of the seaward oceanic crust and were emplaced only along the western continental margin? In contrast to the figures - where absent in the east (southernmost African margin) the SDRs are also absent in the west (see Becker et al., 2012, and Franke et al., 2010). The only information on the SDRs in the S-Atlantic are to my knowledge from the Kudu wells at the border between Namibia and S-Africa. Sediments directly overlying the drilled basalts are dated as (?Late) Barremian (in the time-scale used here ∼122-123 Ma). Sparse microfauna in the lowermost interval interbedded with volcanics may indicate an age no older than Valanginian.

In your figures the SDRs are emplaced, given your time-scale, a bit early at 134 Ma (E Valanginian). Typically only few (means about 3) million years are supposed for the formation of these features, leaving a gap of about 8-10 Ma? I would in addition suggest that the SDRs are time-transgressive – but I may be biased here. Anyway, there is no need to correlate the SDRs with the Parana-Etendecka basalts as the Kudu basalts are “not offshore equivalents of the Etendeka basalts” (Erlank et al., 1990). Rather the Kudu SDRs basalts appear to be most similar to within-plate basalts of asthenospheric origin. Offshore the Tristan da Cunha hot-spot (if it is a hot-spot at all) did emplace much more material to the African plate. This typically explains the “plume tail”, the Walvis Ridge. However, in the models it is placed consistently below the S-American plate.

Falklands

There are a numbers of studies available on the evolution of the Falkland Islands area and if those did rotate or not and where these islands have to been located before breakup: e.g. Mitchell C, Taylor GK, Cox KG, Shaw J (1986) Are the Falkland Islands a rotated microplate? Nature 319 (6049):131-134. Thomson K (1998) When did the Falklands rotate? Marine and Petroleum Geology 15 (8):723-736. A brief discussion would improve the paper.

4.1 Kinematic reconstructions

I suggest a reorganization of this chapter. The main issue should be to convince the reader about the consistency of your preferred model PM1. This can be followed by a discussion about previous models (e.g. NT91) and your tested alternatives without introducing additional models. In the corresponding figures only the applicable models (and times) should be presented to avoid confusion and considerable overlays in the figures. “All our models (PM1-PM6)” – should be PM1-PM5, anyway.

4.2 Fit reconstruction and the influence of Antarctic plate motions

“The overall NE–SW extension causes a clockwise rotation of the Patagonian blocks away from SAf and SAm commencing at ∼150 Ma, and initiates rifting in the North Falkland, Colorado and Salado basins which preceeds relative motions between the African and South American plates (Fig. 12).” Nice idea – but how to explain that the N Falkland Basin is N-S while the other two are E-W directed? In my view we need to attribute the N Falkland Basin to the latest E-W extension like the Orange Basin. If so we have to assume older ages for the first oceanic crust and the seafloor spreading anomalies?

4.3 Phase I: Initial opening – Base Cretaceous to upper Hauterivian (143–126.57 Ma)

“Extensional deformation along the WARS, CARS and SARS is documented to start in the Earliest Cretaceous (Berriasian) by the formation of intracontinental rift basins”. Extensional deformation started much earlier, admittedly it is not well understood. A summary of known ages of the onset of rifting around South Africa is given by Jackson et al. (2000). Estimations for the southern African basins are: Cape Basin, 220-200 Ma; Orange Basin, 160 Ma to 144 Ma; Lüderitz-Walvis Basins, 126 Ma . The ages should be handled with caution, because the earliest rift fill was rarely drilled and these estimations may vary by as much as 20 Ma for any particular basin (Jackson et al., 2000). However, at least two phases of rifting, as suggested by Keeley and Light (1993), occurred in the Late Triassic-Early Jurassic and in the Mid-Jurassic - before the major Late Jurassic to Early Cretaceous rift phase that subsequently resulted in seafloor spreading. There is discussion about Triassic rifting, however, as you state above evidence for Jurassic rifting is widespread. (“This is recorded by Oxfordian-aged syn-rift sediments in the Outeniqua Basin in South Africa as well as subsidence and crustal stretching in the North Falkland Basin and the Maurice Ewing Bank region”)

“Extension in all Major (!sic) rift basins occurs at slow rates during the initial phase, with separation velocities between SAm and SAf around 2mma−1 in the Potiguar/Rio Muni segment and up to 15mma−1 in the southernmost SARS segment, closer to the stage pole equator.” All this depends on your proposition that extension starts in ?Late

Jurassic times. If there was a Triassic rift phase these numbers were wrong. I suggest writing this more carefully. I did not get the arguments for the proposed E-W direction from 143 Ma to 127 Ma. Please explain in a bit more detail what this proposition is based on. What is the relation to the opening of the Weddel Sea (starting at 155 Ma), which maybe count for more N-S extension?

“Around 138Ma (Mid-Berriasian), break up and seafloor spreading starts in isolated compartments between the Rawson Block and the continental margin south of the Orange Basin, and by 132 Ma, all but the conjugate Orange Basin/Salado Block segment in the southern SARS have broken up and seafloor spreading commenced south of the Walvis Ridge/Florianopolis High (better Rio Grande Rise). “ I agree with those absolute ages because these fit nicely with reported ages for the breakup unconformity in the basins around the S Atlantic (see Franke, 2012 MPG). However it became not fully clear to me how you constrain such ages.

Further remarks

Lots of abbreviations make the manuscript difficult to follow: e.g. P65: “This indicates that rifting in the WARS and CARS has most likely started at the same time when rifting in the SARS commenced. Flowlines produced by PM2 and PM3 deviate between 126 and 120Ma from our preferred model, indicating more transpression during this time interval.” CNPS, PM NT, and so on. I suggest minimizing the use of abbreviations. Model NT91 is NM91 in the figure captions. While WARS and the CARS are discussed under a corresponding headline this is not done for the SARS.

Given the uncertainties in the timing I suggest avoiding ages like 126.57 Ma. 127 Ma is fully sufficient.

P49: L22ff: “we use anomaly M4n old as our oldest oceanic isochron to constrain the motion of South America relative to Africa.” Please explain the resulting effect of this limitation.

“Salado and Colorado Basins in Argentina, two Early Cretaceous-aged basins which strike nearly orthogonal to the Main South Atlantic rift” I suggest assuming an earlier age for the initial formation of these basins. Please see also Pángaro and Ramos (2012).

Case insensitivity is widespread : e.g. P55 L20: Makes; P47 L20: Main ->main P66 L19: . . .outlines. margins, P68 L6 Magmatism

Now there are more than ten plate reconstructions at hand for the South Atlantic. It is typically left to the reader to evaluate the limitations or to the follow-up authors to point to the problematic parts. Wouldn’t it be a good idea to write a few lines about the regions, structures and times where you do not feel too well with? Just a thought.

Figures

Many figures are out of order or are lacking a citation in the text. Magnetic spreading anomalies should be annotated consistently (as e.g. M0 not as CM0)

Fig. 1 I suggest sorting the description and abbreviations in alphabetic order Fig. 9: Please explain the two lines in the NW corner. Typo: compresion Fig. 13 It would be nice seeing the full extent of the South Atlantic region here Fig 14 lower panel could be limited to the western hemisphere only

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Once again, a nice paper that I enjoyed reading Dieter Franke

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