General Author Response

We thank both reviewers for their constructive feedback – we appreciate the significant amount of time required to review such a lengthy manuscript. We have revised our manuscript, figures and Supplementary Material (including animations and GPlates-files). Sections of text were restructured, streamlined and re-written in some cases to address reviewer feedback.

Although our work builds on many previous studies, we believe that our work reconciles some key controversies – including, 1) that the Southwest Borneo Core was autochthonous to Sunda since at least the mid Jurassic, 2) the Philippine Archipelago (including Halmahera, Obi and parts of Luzon) formed on intra-oceanic subduction since the latest Jurassic, 3) the Proto South China Sea opens as a back-arc along the east Asian margin, 4) that continental fragments are detached from east Asia and transported onto northern Borneo (Semitau) and Luzon (e.g. Mindoro), and 5) that the tectonic evolution of Sundaland is intricately linked to the convergence of the Indo-Australian, Eurasian and Pacific plates. Other key findings are presented in the main text.

We present our regional model, embedded in a self-consistent global plate motion model, in digital format so that it can be tested and expanded in future studies. In addition, we generate seafloor spreading histories, continuous plate boundary evolution and plate velocity fields in order to ensure that our model is consistent with relative plate motions and geodynamic driving mechanisms of plate kinematics. Below, indented and in blue text, we offer our responses to each point raised by the reviewers.

Reviewer #2
Jonathan Pownall

Review of the manuscript ‘The Cretaceous and Cenozoic tectonic evolution of Southeast Asia’

by S. Zahirovic, M. Seton, and R. D. Müller

Zahirovic et al. present a new plate reconstruction of SE Asia and Australia from 155 Ma to the present day based on their reassessment of previously-published data and tectonic models. The authors should be congratulated in synthesising such a vast amount of information from the literature regarding the geology, geochronology, geochemistry, palaeontology, sedimentology, palaeomagnetic history, and present-day geophysics of this tectonically-complex region. This new reconstruction differs in sev- eral ways from previously-published plate reconstructions for the region (i.e. Metcalfe, 2011; Hall, 2012; Morley, 2012), the main differences being the accommodation of northward Indian movement purely by rifting as opposed to invoking displacement along the I-A transform, and a New Guinean origin for W Sulawesi, E Java, Mangkali- hat, SW Borneo, and E Borneo as opposed them being derived from the NW Australian margin.

There is no doubt that this task has been a serious undertaking, and the authors have demonstrated that they have been thorough in their approach, especially when consid- ering the vast and geologically-diverse region involved. It is therefore frustrating that often the message is obscured by the poor clarity of the text. The discussion is def- initely the better written part of the paper, in which complex ideas are carefully and often clearly

explained, for instance in section 4.2. It would be of great benefit to the readability of the paper as a whole if the earlier sections, especially the introduction and methodology section, were written to the same standard. It is a shame that some- times ambiguous and overly-long sentences detract from the intended message. Also, the tense often confusingly switches between present and past, e.g. on page 1367 – past tense ('experienced extension'; line 3) switches to present tense ('Extension also propagates'; line 13). I found it more readable when the past tense was used to de- scribe proposed past events, such as was used in the conclusions. Regarding these issues, I've made a list below of parts that I felt were ambiguous or awkward to read. I hope I'm not being too picky, but, for me and maybe others, this would greatly im- prove the readability of the text. Aside from these points, the paper still provides a very interesting discussion of tectonic models for SE Asia.

My main question about this new plate reconstruction is if there is any geological evi- dence for W Sulawesi, E Java, Mangkalihat, and E Borneo having been derived from New Guinea and not NW Australia. In the discussion, no convincing justification is presented for this decision, and an alternative suggestion is given in figure 12 and mentioned briefly in the text for them indeed having a NW Australian origin. Following on from this, I'd ask the authors to explain why Seram and eastern Sulawesi, compris- ing the Sula Spur, are shown to appear from nowhere at 80 Ma in the reconstructions. If the Argo and Banda blocks were rifted from the NW Australian margin, as proposed solve this problem. Another concern is that on a few occasions little justification is given for preferring one previously-published model over another. It would also benefit the paper if more new ideas were proposed and/or discussed in addition to the previous models. And at times, it seemed as though preference was given to geometry rather than geology. It should also be pointed out that no new geological or geophysical data is reported, but I appreciate that it is beyond the scope of the study to have done so, and the authors clearly state that the paper is a synthesis of previous work.

My conclusion is that the new plate model presented by Zahirovic et al. will certainly further stimulate discussion over such a tectonically-complex region, which can only be a good thing, irrespective of some personal disagreements with certain aspects of the reconstruction. The paper also provides a valuable synthesis of such a huge amount of literature. Therefore, I recommend that the paper should be considered for publication, subject to the revisions outlined below.

Specific Comments

1336 9-13 The sentence beginning 'We propose a scenario. . .' is confusingly written – it is not immediately clear what is rifting from Gondwana or which subduction zone is providing the slab pull.

> We have clarified this sentence – the slab pull in our model is derived from north-dipping subduction along southern Eurasia.

1337 5 It is not clear what you mean by 'continuously closing plate boundaries'.

> This is a good point, and we have simplified this jargon. It gets explained in the Methodology, but as you pointed out, is not so clear just in the Abstract. The wording comes from the terminology introduced by Gurnis et al. (2012) who built the algorithm and workflow in GPlates. Essentially, plate boundaries can move individually, but their intersections define the topology (i.e. no gaps or overlaps), and this is calculated on-the-fly by GPlates once the user has specified which

boundaries participate in the topology. We have updated the text to clarify this workflow.

1337 23 Maybe you need to explain what you mean by ‘Tethyan conveyors’

This has been clarified. Here we mean the Tethyan oceanic plates that carried terranes northward to emplace them onto southern Eurasia.

1338 14 It’s not clear what ‘affinity’ is referring to – affinity to continental basement?

This word was deleted – as you noted, it does seem to be superfluous. Thanks for picking it up!

1338 18 It sounds like you are referring to a remnant of spreading (which makes no sense), not the Jurassic oceanic crust produced by spreading.

This has been clarified to refer to oceanic crust that formed during the latest Jurassic seafloor spreading episode, rather than a remnant of spreading (such as an extinct ridge).

1338 21 ‘results in uncertain origins’ sounds a bit strange

This has been modified to refer to uncertain placements and plate reconstructions.

1338 24 It’s not clear what you mean by ‘pre-breakup fits’

This is a good point – we have tried to simplify this terminology. Here we refer to the full fit reconstruction between the two conjugate continental margins.

1339 1 ‘submerged and remote’: I assume you’re implying that much of Sundaland is either below sea level, poorly-exposed, or difficult to access.

Thanks for the suggestion, we have updated the text.

1339 18 wrong tense: how about ‘in order that our model may be tested and improved’?

The text has been modified to correct this.

1339 20 Methodology Section: As the reader, I was not made absolutely clear of the criteria you used to construct your model. It is often ambiguous what you intend to say. For example, the sentence ‘Proxies of convergence. . .and orogenies.’ requires the reader to make too many assumptions to arrive at the message you intend. I would recommend that you work on improving the clarity of this section, especially as it should provide the fundamental basis for your plate reconstruction.

The methodology has been re-structured to stress that geological constraints are the fundamental and most important, followed by additional constraints applied from the rules of plate tectonics and plate boundary closure. The text has been clarified.

1339 21 ‘geological data’ sounds a bit vague. . . maybe worth elaborating. . .?

As with the previous point, our modification should address this point as well. We have re-structured the methodology and made the text more precise. We also point the readers to Table 2, which summarizes the main geological constraints we applied.

1339 22 Why do you equate the time of ophiolite emplacement to its crystallisation age? Also, how can the timing of ophiolite emplacement be used to infer rifting events? Are you referring strictly to obducted ophiolites (Oman-type), or do you also refer to newly-generated oceanic crust and/or exhumed subcontinental mantle rocks?

> In the text we tried to make a distinction between the age of ophiolite formation (emplacement and generation of new oceanic crust) and the obduction of the ophiolite through collisional processes. These two events are normally separated in time, and some dating techniques can differentiate between the two. Usually, K-Ar mineral ages, assuming a closed system, represent the time of mineral crystallization within the igneous rock. That is why we equate this mineral crystallization age, assuming a closed system, to the age of the oceanic crust forming the ophiolite. Of course, this has problems where the rocks undergo metamorphism, hydrothermal alteration and/or interaction with seawater, resulting in an open system. This seems to be the case with some of the samples from Sulawesi that we use in our interpretations, where the whole-rock ages seem to represent a minimum age, rather than the age of formation. Therefore, we use the oldest age reported, ~158 Ma on Sulawesi (Polvé et al., 1997), to represent the age of the oceanic crust. For us, the emplacement age is indicative of active seafloor spreading or generation of new oceanic crust (or exhumation of mantle rocks through extension), and we use this age as a proxy for the onset of seafloor spreading along northern Gondwana (in the Australia and New Guinea segment). This correlates well with the Late Jurassic shallow marine sediments, and the drilled sites in the Argo Abyssal Plain that have dated oceanic basement representative of the onset of the latest Jurassic-Early Cretaceous seafloor spreading (Gibbons et al., 2012; Gibbons et al., 2013b). More generally, your comments are helpful because the literature does sometimes interchangeably use ophiolite “emplacement” and “obduction”, which in reality are two very different things. The term “tectonic emplacement” is also often used to represent obduction. We have changed our terminology to “ophiolite generation” and “ophiolite obduction” to discriminate the two ages.

1339 25 ‘...collisional events are inferred from...’: You imply that high-T metamor- phism is diagnostic of collision, yet it may also be achieved by extension.

> This is a very good point, thank you for pointing this out. We meant ultra high pressure metamorphism, often also associated with high temperatures, that resulted in the greenschists, blueschists and eclogites seen in the suture zones (Parkinson et al., 1998). We have updated the text to reflect this.

1339 27 What style of ‘orogenies’ do you intend?

> Here we refer to orogenesis resulting from the shortening of continental crust or orogenesis resulting from terrane collisions leading to crustal shortening. The text has been corrected to clarify this.

1340 9 You define certain ‘rules’ for you model, but what if the Earth did not abide by them all of the time? Is there any scope within your model to test these rules? Would these rules be rigidly upheld if unsupported by field evidence?

> This is a fantastic point and question, and often addressed only implicitly, such as in the previous version of our text. We base our understanding of plate tectonics from studies of present-day plate boundaries (e.g. Bird (2003)), but also from preserved

seafloor that records (at least partially) the tectonic configuration and evolution through time (e.g. Seton et al. (2012)). We apply the principle of uniformitarianism, and assume that the rules of present-day tectonics persist throughout at least the Phanerozoic. Since the model is provided in digital form, it can be tested. We will test it in the future using geodynamic numerical models, similar to the methodology applied in Zahirovic et al. (2012). As computational time becomes more readily available, it would be interesting to apply our kinematic boundary conditions as an initial condition in 4D global numerical models with adaptively refined meshing, realistic rheological parameters for lithospheric and crustal processes capable of modeling partial melting and other complex smaller scale geological processes (see Stadler et al. (2010)) that are not entirely captured by global numerical models using CitcomS. Digital plate motion models in GPlates can currently be linked to CitcomS and Terra mantle convection codes, and work is underway to link it also with Underworld. This means that any plate motion model that works with GPlates can be tested in regional or global models, to help discriminate between alternative (end-member) scenarios. However, the priority is still placed on definitive geological evidence. As more geological evidence becomes available, the plate motion model will evolve to be consistent with the additional constraints. Where geological evidence is ambiguous, additional criteria must be applied to plate motion models, including plausible plate boundary configurations and invoking plausible plate driving mechanisms that are testable with numerical models. An interesting point though is our assumption that plate speeds do not exceed ~25 cm/yr. We base this on our understanding of India's northward trajectory towards Eurasia as a precedent. However, as you point out, plates may have moved faster in the past, but we lack preserved seafloor to demonstrate this. Numerical models that resolve the plate boundary forces implied by our plate motion models can be used to test the speed of plates by exporting the plate boundary configurations from GPlates (see Quevedo et al. (2013)), which may form the basis of future work.

1340 11 It's maybe worth defining 'RRR' as 'ridge-ridge-ridge' for those who don't know.

Agreed, we have accepted your suggestion.

1340 22 Sentence beginning 'Lastly, plate boundary...': What if there was localised extension within an overall convergent setting (or vice versa?).

This is a good point, but we are not sure if it is relevant to the regional and global scale rigid plate reconstructions that we are proposing in this manuscript. Of course the region is very complex and includes internal deformation of continental crust. Our rigid block rotations imply such deformation, similar to those modeled by Hall (2002, 2012). Future work would involve creating a deforming plate model for the region, and GPlates has the functionality to accommodate such deformation, as currently exists in our model of the Australia-Antarctica rifting (Williams et al., 2011) and South Atlantic opening (Heine et al., 2013). However, the rigid plate model needs to be built first before the deformation can be incorporated, and so our work is a first step towards having this goal.

More directly related to your question, localized extension is only incorporated in cases where oceanic basins are being created in back-arcs, or where well-studied rifting is occurring (such as the extension on the South China margin, whilst convergence is occurring along northern Borneo). Any scale finer than this is not currently incorporated in our model, but can be introduced at a later stage. We have tried to clarify the sentence, but please let us know if it needs more clarification.

1341 5 Again, why does an ophiolite crystallisation age necessarily date it's emplacement?

> See point above regarding Page 1339 Line 22.

1341 6 Sentence beginning 'Accretion events..': You imply that UHP metamorphism will have always occurred at the onset of collision, why may not be true.

> Although we infer that the UHP metamorphic rocks found in East Java record an arc-continent collision at ~115 Ma, we realize that UHP rocks can form in alternative scenarios, and our text has been revised to discuss this. We interpret that the background UHP signal from ~140 to 90 Ma is a result of ongoing subduction, while the peak at ~115 Ma represents the arrival of a continental fragments. We address the possibility of subduction erosion followed by exhumation (possibly through changes in the coupling between the overriding and downgoing plates), changes in convergence or the approach of a continental fragment (see Section 4.3).

1341 20 Not sure of the use of the word 'interactively'

> This has been clarified. We used GPlates to derive and/or modify the rotations (i.e. finite Euler pole and rotation) of blocks (such as East Borneo, East Java, etc.) using the user interface (hence the word "interactively"). More information on how this is done can be found in (Boyden et al., 2011) and in the GPlates documentation (see Tutorial 2.1 and 2.3 at https://sites.google.com/site/gplatestutorials/tutorials2013/). These blocks were placed along the Gondwana continental margin, with gaps and overlaps minimized visually. Full fit reconstructions were not possible due to the lack of well-defined conjugate passive margins.

1342 7 Not sure of the use of the word 'interactively'

> See above.

1342 17 What are 'block-out lines'? Maybe worth defining.

> This is another good question. For the sake of brevity, it was not explained, but we have happily added an explanation in the main text.

1343 7 Sentence beginning 'In addition, the. . .': I found this to be confusing. How can 3-D mantle structure be observed when it's been modelled?

> Apologies – this was an error. We have updated the text to represent that we used the seismic tomography models to help us interpret the distribution of subducted slabs.

1343 13 'resulting from subducted slabs following Van der Voo et al. . .': I'm guessing you don't mean this literally!

> You are right – subducted slabs are unlikely following van der Voo et al. (1999)! We have restructured this sentence to clarify this, as it refers to the assumptions van der Voo et al. (1999) made when selecting the seismic velocity anomaly isovalue to represent 'colder' mantle resulting from past subduction.

1344 26 Why is this your preferred scenario? 1345 20 'As the strike. . .': Is there any field evidence to support this? 1345 29 '. . .we invoke the separation. . .': Why? 1346 35 I could

not find the Wharton Ridge labelled in figure 1.

> The tectonic fabric (normal faulting) of the continental shelf is parallel to the magnetic anomalies interpreted in the Argo Abyssal Plain (Gibbons et al., 2013a; Heine and Müller, 2005), highlighting the northeast – southwest strike of the margin. We lack the seafloor spreading record on New Guinea, however, the outline of the stable Australian continent is limited by the New Guinea Mobile Belt, and the strike of this interface is approximately 120 degrees to the strike of the isochrons in the Argo Abyssal Plain. We propose that therefore simultaneous rifting along the New Guinea continental margin and the NW Australian shelf is kinematically incompatible without requiring a triple junction. An additional figure will be included in the supplementary material demonstrating this.
>
> Our preferred scenario invokes rifting propagating from east to west along the northern Gondwana margin from the latest Jurassic. This is documented by the Late Jurassic shallow marine sandstones on the New Guinea margin that indicate rifting onset as early as 187 Ma (Cullen and Pigott, 1989), and the latest Jurassic preserved seafloor spreading history in the Argo Abyssal Plain (Gibbons et al., 2013b; Gradstein and Ludden, 1992).
>
> Apologies – the Wharton Ridge does not appear in Fig 1, and only appears in the reconstructions of Fig 13B. Text has been updated.

1346 27 'Instead, the model of Hall (2012) requires that. . .': I'm not sure this statement is true. Hall's reconstruction shows only right-lateral slip along the hypothesised I-A transform.

> See Section 6.6 in Hall (2012), part of which reads "Between 90 and 75 Ma (Figs. 20 to 22) the boundary was a leaky transform with very slight extension of the order of a few tens of kilometres. From 75 to 55 Ma (Figs. 23 to 26) the boundary was con- vergent, with the amount of convergence increasing northwards from about 10°S. This implies either subduction of the Indian Plate beneath the Australian Plate or vice-versa. The amount is small and would have been approximately 500 km at the northern end of the trans- form boundary."
>
> This paragraph from Hall (2012) indicates that ~500 km of convergence is implied across the I-A Transform between 90 and 75 Ma. The transform initiates and cross-cuts pre-existing oceanic lithosphere, and most importantly, cuts across pre-existing seafloor fabric (i.e. weaknesses) that would have preferentially be exploited in the model. Although this portion of the I-A Transform exists on oceanic lithosphere that is now subducted, it is still important to propose a scenario that is geodynamically feasible. Our model is simpler and consistent with the seafloor spreading record obtained from much more recent data, and does not require this complexity. In addition, there is no need to invoke a hiatus in subduction between 90 and 45 Ma – in fact, the geological evidence of arc volcanism on Sumatra only suggests a hiatus in subduction between 75 and 65 Ma (See our Fig. 14).

1348 29 'This collision timing': I'm not sure when you are referring to.

> Here we refer to the collision between East Java-West Sulawesi and an intra-oceanic arc, following the arguments and model proposed by Wakita (2000). The text has been clarified.

1349 2 Sentence beginning ‘Rather than invoking. . .’: Your reconstruction shows collision at 80 Ma, not 120-110 Ma. I don’t quite see how this ties in with the K-Ar dates you refer to.

> This was an error in our text, thanks for picking it up! Yes indeed, we prefer a scenario where East Borneo-West Sulawesi terrane collides with an intra-oceanic island arc at ~115 Ma, and suturing that consumes the Barito Sea by 80 Ma. The text has been corrected.

1349 3 ‘this time’: which time?

> We have clarified the time to be 110 Ma, as argued by Hall (2012).

1351 8 ‘has been disputed to instead suggest’: Do you mean ‘reinterpreted to instead suggest’?

> Yes – thanks for the suggestion, the text has been changed.

1351 16 ‘we prefer that it accreted to Eurasia in the Triassic’: Why do you prefer this model?

> West Burma remains an enigmatic terrane, with controversial interpretations for its origin and accretion timing. We tentatively follow the interpretations of Metcalfe (2011), Morley (2012) and Hall (2012) to place West Burma on the Eurasian margin before the latest Jurassic rifting event along northern Gondwana. We have simplified our text in relation to this. West Burma also lacks the Jurassic subduction-related volcanism we would expect from north-dipping subduction of Tethyan crust. The reported Paleozoic Cathaysian fusulinids would place West Burma on the Eurasian margin much earlier, excluding it from being Argoland. The problem here is that the fusulinid samples are from one locality, east of the Sagaing Fault, which is very close to the suture zone (Shan Boundary from Metcalfe (2011)).

1352 9 ‘The synthesis...’: I found your summary of the geochronology results to be slightly ambiguous: Is the 151 Ma age an Ar-Ar result or a K-Ar result? What was dated by Ar-Ar in the Calaguas Ophiolite? Was the Dibut Bay ophiolite dated with Ar-Ar or K-Ar and what do you mean by ‘plateau age of amphibolite metamorphism’? What minerals were dated, and how was this related to a metamorphic age?

> The 151 Ma result is an Ar-Ar result, and the text was updated to clarify this. The ~100 Ma age from the Calaguas Ophiolite was derived from an amphibolite. The Dibut Bay age is also $^{40}Ar/^{39}Ar$, and derived from an amphibolite – the reference to metamorphism has been removed. The ~92 Ma age of the Dibut Bay ophiolite was derived from an amphibole in an amphibolite. The text has been updated.

1352 26 ‘comparable’: in what respect?

> Here we refer to the ophiolites being correlatable – both in age, but also spatially, following the interpretations of Billedo et al. (1996) and Encarnación (2004). The text has been updated.

1353 7 ‘We follow the interpretation of Encarnacion. . .’: Why? I’m sure you have good reasons for adopting certain models and rejecting others, but sometimes it’s not particularly clear what evidence you’ve based your decision on. It’s maybe worth quickly stating why your preference lies with a certain model to prove that your decision isn’t

arbitrary.

> The synthesis of Encarnacion (2004) follows much earlier works (Geary et al., 1988; Geary and Kay, 1989; Karig, 1983) that demonstrate the varying ages of ophiolitic belts and arcs in the Philippine Archipelago, and the tectonic mechanisms responsible for their formation. This cyclical back-arc opening, often followed by back-arc subduction, ophiolite obduction and accretion has been a process invoked for the long-term growth of Southeast Asia (Pubellier and Meresse, 2013). Therefore, we believe that the mechanisms and interpretations in Encarnacion (2004) are consistent with other studies on the growth of the Philippine Archipelago – with episodes of back-arc formation, magmatic arc overprinting on existing crust/basement and successive periods of ophiolite obduction.

1354 2 What do you specifically mean by 'developed on'?

> This is a really good question. In the 1990s/early 2000s, two end-member reconstructions for Luzon largely circulated the literature. One end-member scenario implies that Luzon either forms on the Eurasia margin, in the vicinity of Sundaland (Hall, 1996, 2002). The other end-member advocated/implied Luzon formation and evolution with the rest of the "allochthonous" Philippine Archipelago (Lee and Lawver, 1995). However, it has since become clear that western accreted blocks on Luzon have an east Asia origin, whilst the central and eastern portions of Luzon are allochthonous and likely formed with the latest Jurassic-Early Cretaceous proto-Philippine Arc, which is consistent with much older ideas on the formation of this composite terrane (Faure et al., 1989; Rangin et al., 1985; Yumul Jr et al., 2003).

1356 9 'In fine tuning ...': I found it difficult to grasp what you mean by this sentence.

> We have added more text to clarify this workflow, which had been previously employed in Hafkenscheid et al. (2006), Zahirovic et al. (2012), Van Der Meer et al. (2010) and other studies. This entire process is an estimation that will need to be tested with geodynamic modeling. We use GPlates to interactively modify the Euler rotation of the Philippine Sea Plate as a whole, while maintaining the relative plate motions within the West Philippine Basin and the back-arc opening related to Izu-Bonin-Mariana rollback.

1357 2 I'm not sure what you mean by the use of the word 'embryonic', also on page 1359, line 14; page 1367, line 12.

> Here we mean "incipient" or early-stage. We have replaced embryonic with incipient throughout the document for clarity.

1357 11 '. . .invokes Caroline Plate origin from rollback induced back-arc formation. . .': I think you could reword this sentence to make it clearer.

> The sentence has been reworded and clarified.

1358 4 'We model the origin of. . .' Why? It would probably be worth providing evidence for this decision.

> We follow the model of Hall (2002), as stated earlier in the paragraph. This is because the southern boundary of the Caroline Plate is the best candidate for the origin of the Torricelli-Finisterre arc. The sentence has been clarified to include a

citation for Hall (2002).

1358 14 Do you mean ‘no seafloor history is preserved’ or do you mean that ‘no seafloor spreading occurred’?

Here we mean that ‘no seafloor history is preserved’. We have clarified the text.

1360 23 The Banda Embayment is the term given to the pre-existing D-shaped embayment, bound by the Sula Spur to the north, that once enclosed Jurassic oceanic crust (the Proto Banda Sea). Therefore, the Banda Embayment did not develop by slab rollback as you describe; rather it was rolled back into by the Banda slab.

Thanks for the clarification, this is very helpful. We have updated our text and model to reflect this.

1361 8 But is there any paleomagnetic evidence to support a northern hemisphere origin? I’m not sure that your criticism is valid.

The Southwest Borneo core remains controversial – with unclear ties to either Indochina or Gondwana. Previous models, such as Stampfli et al. (2002) imply a Gondwana origin with Borneo and Indochina rifting from northern Australia in the Paleozoic. Alternatively, as proposed by Metcalfe (2011) and Hall (2012), the Southwest Borneo Core originates in the NW Australian shelf in the latest Jurassic-Early Cretaceous, and collides/docks with Sundaland along the Billiton “suture” by 110 Ma. As a result, the Southwest Borneo Core likely originates in the southern hemisphere – but we dispute the latest Jurassic origin from the NW Australian shelf advocated by Metcalfe (2011) and Hall (2012). The evidence for the Billiton Depression being a suture is also scarce, to say the least. The Billiton Depression, part of the East Natuna Basin, is likely faulted – but no evidence has ever been presented that it represents a major suture.

In essence, we are not proposing that Borneo originated in the Northern Hemisphere, but that there is no evidence that the Southwest Borneo Core rifted from northern Gondwana in the latest Jurassic-Early Cretaceous. We have updated the text to clarify this.

Instead, the continuity of the east Asian volcanic arc from Jurassic to Late Cretaceous times (Charvet et al., 1994) into Southwest Borneo (including the Cretaceous-age Schwaner granitoids) has been invoked many times before (Haile and Bignell, 1971; Honza and Fujioka, 2004; Katili, 1975, 1981), and therefore excludes the possibility that the Southwest Borneo Core accreted to Sundaland in the mid Cretaceous, as proposed by Metcalfe (2011) and Hall (2012). As the Southwest Borneo Core was already being intruded by subduction-related volcanism in the Jurassic and Cretaceous that was a continuation of the east Asian Andean-style volcanic arc. This means that the Billiton Depression could not have been a suture in the Cretaceous, and it is uncertain whether it is a suture at all.

We have clarified the text to convey this. The paleo-biogeographic study of Metcalfe (1988) clearly shows that Southwest Borneo shows Tethyan affinities in the Triassic, but not in the Jurassic. This may mean that SW Borneo was always part of Indochina, considering it has Paleozoic metasedimentary basement, or it had rifted from Gondwana and collided with Southeast Asia. However, many authors separate SW Borneo from the rest of Sundaland using the Billiton “suture”. Yet no evidence

has been presented that it is a suture, and it seems to stem from a suggestion by (Ben-Avraham, 1978) that it is a large transform.

1364 13 '..and suturing occurred. . .' Suturing of what?

Suturing of the Dangerous Grounds-Reed Bank to Borneo. Text has been clarified.

1366 25 Maybe elaborate on what you mean by 'The dominant tectonic regime was age-coded for present-day basin geometries'.

We have clarified this portion of the text.

1367 23 'These compressional regimes continue...': do you mean collision of the Eurasian and Australian plates, not Indian?

Yes, this was a typo – thanks for picking it up! We meant the Australian continent colliding with Asia (including Eurasia).

1368 19 'but prefer that. . .': Maybe it's worth reiterating why you prefer this model.

We have briefly re-iterated the main point, and suggested readers look at Section 3.7 in order to reduce repetition.

1369 6 '. . .but we believe it. . .': Again, what's the evidence for this?

The evidence is laid out in Section 3.7, and the text has been modified to point readers to review the other section to reduce duplication.

1371 3 'The model proposed by Hall (2011). . .': How does your model account for the creation of the Sula Spur? Hall (2011) proposed that the Banda Embayment and Sula Spur were created by the rifting of the Argo and Banda Blocks from the NW Australian margin at 160 Ma, but in your reconstruction, Seram and Northern Sulawesi appear from nowhere at 80 Ma. What's your justification for this?

After careful consideration and further research, including the synthesis of Milsom (2000), we have decided that the model of Hall (2011, 2012) better accounts for the geology of the Sula Spur and associated blocks. We have modified our model and re-created Fig. 13b and animations in the Supplement to address this.

1374 4 If W Sulawesi, E Java, Mangkalihat, and E Borneo were 'equally likely' to have originated in the Argo Abyssal Plain than northern New Guinea, then why have you chosen to model one origin over the other? 'Equally likely' implies you don't have enough evidence to make a meaningful decision and you're therefore undermining your own argument, especially as this is one of the most significant differences between your plate reconstructions and those previously proposed by Hall (2012), Metcalfe (2011), and Morley (2012).

There are many differences between our model and that of Hall (2012) and Metcalfe (2011). However, due to the ambiguities in the data and interpretations presented in these models (such as the problems with Southwest Borneo), and the lack of more constraints on placing East Java in the Argo Abyssal Plain – we choose a more diplomatic approach, and stress that these fragments could have originated from northern Gondwana (including from Greater India, Argo Abyssal Plain or New Guinea). We have clarified the text slightly.

In reality, much more work needs to be carried out in order to 1) definitively rule out (portions of) West Burma as Argoland and identify the required Jurassic magmatic arc resulting from northward Tethyan subduction along this terrane, 2) resolve the controversy of the nature of all Sumatran accreted blocks, and 3) identify the best pre-rift location for East Java, West Sulawesi and East Borneo. For point 3, we would suggest a comparative study of detrital zircons from Greater India (perhaps preserved in the Tethyan Himalaya), the NW Australian shelf and New Guinea, similar to the recent statistical study of Burrett et al. (2013).

1375 14 You give reasons why SW Borneo likely was not derived from NW Australia, but cannot do the same for W Sulawesi or E Java; so why have you chosen to model them having been rifted from New Guinea? What's wrong with NW Australia? If there are good reasons for choosing a New Guinea origin in your reconstruction, the paper does not make them clear. Alternatively, if you are intending to present two possible scenarios, this could also be more clearly stated.

This is a genuinely interesting point, and we hope that further work will help better constrain the pre-rift position of East Java and West Sulawesi. Hall (2012) reports that Archean ages in detrital zircons are useful to link these blocks to the Argo Abyssal Plain. We think this makes sense – however, it is necessary to consider alternative pre-rift positions too. Considering Greater India and New Guinea are also plausible, and nearby locations, then they may be shedding detrital zircons of similar age, considering the age of these continental blocks. An objective and statistical method of ranking the better source for the detrital zircons will be necessary, and would be a very interesting case study. We have clarified our text and acknowledge that the best we can do now is place these fragments on northern Gondwana, when taking into account the less-than-ideal geological (spatial) constraints that exist. In addition, we were vary of placing these fragments on the NW Australian shelf as it would require a very large clockwise motion at the western periphery of the MesoTethyan plate north of India and close to Africa – because it would be close to the Euler equator, since a large clockwise rotation of the blocks is needed to transfer them to Sundaland. Thus we chose the simplest kinematic scenario that could account for the geological constraints and result in plausible seafloor spreading rates in the Meso- and Neo- Tethys.

Table 2 'Sarawak Orogeny' and 'Sabah Orogeny' do not make sense as dating methods – how were these orogenies dated?

This is a good point. These events were dated stratigraphically using the punctuated events of regional unconformities, accompanied with periods of rapid exhumation, regional basin inversion events and other stratigraphic constraints (Hutchison, 1996, 2010; Hutchison et al., 2000; Fyhn et al., 2010).

Figure 4 It looks nice, but I'm not sure how useful this figure is and it's not referred to by the text.

Figure 4 is referred to in text, in the Methodology (Page 1340, Line 9). It is useful to demonstrate our methodology and stress that it can be tested using forward numerical models of subduction.

Figure 12 There are no orientations marked on the cross-sections, i.e. NE and SW labels.

NE and SW labels have been added. Thanks for the suggestion!

Figure 13b I'm sure you've also noticed that this figure has been squashed! I'd suggest you make it as big as possible in the final version of the paper (maybe cut it in half and take up two pages?) as it's very important to what you're proposing and has lots of detail. Why not in the caption refer the reader to your animated plate reconstruction video contained in the supplementary files?

> Yes this is super important. We designed that the figure is split into two, each panel taking up an entire A4 portrait page. We thought this would be the best way forward, especially as it is an online-only journal. However, it seems that it was not possible to arrange in the reviewing PDF format, as it is landscape only. We will consult the proofing team to get this fixed. We also now refer the reader to the animations, thanks for the suggestion! You can download the full size figures from here (download link here).

Figure 16 Again, there's no orientation or scale on the cross-sections. Also, it might be worth mentioning how you propose the Proto Molucca slab was over-ridden by the rifting of the Sepik block from New Guinea, as depicted.

> Orientations have been added. Since both Fig. 12 and 16 are schematic, they are not to scale. This warning has been added into the figure caption – thanks for the suggestion! As the figure is a schematic, it is difficult to determine whether the slab was overridden, or if it sank into the lower mantle. We depicted it as being overridden due to the buoyancy of the young Sepik oceanic basin. However, numerical models of subduction will be helpful in testing this scenario. A clarifying comment has been added to the figure caption.

Technical Corrections

> **All technical corrections have been accepted as is, unless stated otherwise. Thanks for all the great suggestions!**

Page Line 1337 15 References seem to be listed in a strange order

> This is a good point – not sure why this is the case. We used EndNote to manage the citations using the Copernicus template. We have manually changed the order to be chronological – from older to more recent.

1337 19 References seem to be listed in a strange order

> This has been fixed.

1337 26 'comprised of' would be more correctly written 'composed of' or 'comprises'?

> Composed fits much better – thanks for the suggestion!

1338 17 'gives' should be 'give'

1338 27 'Much' should be 'Many'?

1339 24 'proxies of' should be 'proxies for'?

1340 5 first 'of' should be an 'and': 'the development and evolution of Sundaland'?

1343 8 There's a problem with the reference: should be 'Simmons et al. (2009)'

1343 18 ‘Gondwana’ should be ‘Gondwanan’?

1344 1 ‘originating from’ should be ‘by’?

This has been re-worded for clarification.

1345 10 ‘Gondwana’ should be ‘Gondwanan’?

1348 28 ‘collide’ should be ‘collided’

1350 22 ‘indicate’ should be ‘indicated’

1351 26 ‘by a’ should be ‘by the’?

1352 17 ‘as a back-arc’ should be ‘within a back-arc’?

1352 19 ‘wile’ should be ‘while’

Apologies – this is a very obvious typo, not sure why MS Word did not flag it.

1354 2 ‘Whether’ maybe should be ‘Whether or not’

1354 27 ‘with the Eurasian’ should be ‘to the Eurasian’

1354 29 ‘on the margins’ should be ‘beneath the margins’?

Here we chose to use the word “along” the margins – please let us know if this is still problematic.

1355 3 ‘can indicate’ should be ‘may indicate’

Yes, absolutely – apologies.

1355 11 ‘data is’ should be ‘data are’

Another obvious one, apologies for the typo and thanks for the suggestion!

1355 15 would be better: ‘evidence by Bird (2003) indicates the presence of active subduction zones. . .’?

We compromised here and used “evidence interpreted by Bird (2003)”. We hope that this is sufficient.

1356 7 ‘3 and 1.2’ should be ‘3.0 and 1.2’?

1356 17 ‘which is differs’ should just be ‘which differs’

1356 17 ‘from Hall’ should be ‘by Hall’

1356 18 ‘histories of’ should be ‘histories for’?

1360 5 ‘linked to a collision and the emplacement’ could be just written as ‘linked to collision and emplacement’

1361 3 a comma needed after ‘Borneo’

1363 27 ‘subsidence on’ should be ‘subsidence of’?

1364 15 ‘based on collisional unconformity’ should either be ‘based on collisional unconformities’ or ‘based on a collisional unconformity’. In any case, what exactly is a collisional unconformity? Maybe this needs a short explanation or a re-wording.

> Here we refer to a collision-related unconformity. In this case, collision results in crustal shortening and orogenesis, ceasing sedimentation and resulting in an unconformity. We have clarified the text to include a brief explanation.

1364 17 ‘represents the south-dipping’ would be better written as ‘results from south-dipping’?

1367 16 ‘resulting with’ should be ‘resulting in’

1368 16 delete the word ‘block’ or change it to ‘blocks’?

> Changed to ‘blocks’

1368 27 change ‘which the southwest Borneo’ to ‘which southwest Borneo’

1372 5 It might be clearer to say ‘unsubducted’ rather than ‘remnant preserved’

> The text has been simplified. Please let us know if it needs more clarification.

1377 16 ’85 Ma as’ should maybe be ’85 Ma by’

References

Ben-Avraham, Z.: The evolution of marginal basins and adjacent shelves in east and southeast Asia, Tectonophysics, 45, 4, 269-288, 1978.
Bird, P.: An updated digital model of plate boundaries, Geochemistry, Geophysics, Geosystems, 4, 3, 102710.1029/2001GC000252, 2003.
Boyden, J. A., Müller, R. D., Gurnis, M., Torsvik, T. H., Clark, J. A., Turner, M., Ivey-Law, H., Watson, R. J., and Cannon, J. S.: Next-generation plate-tectonic reconstructions using GPlates, in: Geoinformatics: Cyberinfrastructure for the Solid Earth Sciences, edited by: Keller, G., and Baru, C., Cambridge University Press, Cambridge, UK, 95-114, 2011.
Burrett, C., Zaw, K., Meffre, S., Lai, C. K., Khositanont, S., Chaodumrong, P., Udchachon, M., Ekins, S., and Halpin, J.: The configuration of Greater Gondwana—Evidence from LA ICPMS, U–Pb geochronology of detrital zircons from the Palaeozoic and Mesozoic of Southeast Asia and China, Gondwana Research, 2013.
Charvet, J., Lapierre, H., and Yu, Y.: Geodynamic significance of the Mesozoic volcanism of southeastern China, Journal of Southeast Asian Earth Sciences, 9, 4, 387-396, 1994.
Cullen, A. B., and Pigott, J. D.: Post-jurassic tectonic evolution of Papua New Guinea, Tectonophysics, 162, 3, 291-302, 1989.
Faure, M., Marchadier, Y., and Rangin, C.: Pre-Eocene synmetamorphic structure in the Mindoro-Romblon-Palawan area, West Philippines, and implications for the history of Southeast Asia, Tectonics, 8, 5, 963-979, 1989.
Fyhn, M. B., Pedersen, S. A., Boldreel, L. O., Nielsen, L. H., Green, P. F., Dien, P. T., Huyen, L. T., and Frei, D.: Palaeocene–early Eocene inversion of the Phuquoc–Kampot Som Basin: SE Asian deformation associated with the suturing of Luconia, Journal of the Geological Society, 167, 2, 281-295, 2010.

Geary, E., Harrison, T. M., and Heizler, M.: Diverse ages and origins of basement complexes, Luzon, Philippines, Geology, 16, 4, 341-344, 1988.
Geary, E., and Kay, R.: Identification of an Early Cretaceous ophiolite in the Camarines Norte-Calaguas Islands basement complex, eastern Luzon, Philippines, Tectonophysics, 168, 1, 109-126, 1989.
Gibbons, A., Barckhausen, U., van den Bogaard, P., Hoernle, K., Werner, R., Whittaker, J., and Müller, R.: Constraining the Jurassic extent of Greater India: Tectonic evolution of the West Australian margin, Geochemistry Geophysics Geosystems, 13, 5, 25, Q05W13, doi:10.1029/2011GC003919, 2012.
Gibbons, A. D., Whittaker, J. M., and Dietmar Müller, R.: The breakup of East Gondwana: assimilating constraints from Cretaceous ocean basins around India into a best-fit tectonic model, Journal of Geophysical Research: Solid Earth, 2013a.
Gibbons, A. D., Whittaker, J. M., and Müller, R. D.: The breakup of East Gondwana: Assimilating constraints from Cretaceous ocean basins around India into a best-fit tectonic model, Journal of Geophysical Research: Solid Earth, 13, 5, Q05W13, 10.1002/jgrb.50079, 2013b.
Gradstein, F., and Ludden, J.: Radiometric age determinations for basement from Sites 765 and 766, Argo Abyssal Plain and northwestern Australian margin, Proceedings of the ocean drilling program, Scientific Results, 1992, 557-559,
Gurnis, M., Turner, M., Zahirovic, S., DiCaprio, L., Spasojevic, S., Müller, R., Boyden, J., Seton, M., Manea, V., and Bower, D.: Plate Tectonic Reconstructions with Continuously Closing Plates, Computers & Geosciences, 38, 1, 35-42doi:10.1016/j.cageo.2011.04.014, 2012.
Hafkenscheid, E., Wortel, M., and Spakman, W.: Subduction history of the Tethyan region derived from seismic tomography and tectonic reconstructions, Journal of Geophysical Research-Solid Earth, 111, B8, B0840110.1029/2005JB003791, 2006.
Haile, N., and Bignell, J.: Late Cretaceous age based on K/Ar dates of granitic rock from the Tambelan and Bunguran Islands, Sunda Shelf, Indonesia, Netherlands Journal of Geosciences (Geologie en Mijnbouw), 50, 5, 687-690, 1971.
Hall, R.: Reconstructing Cenozoic SE Asia, Geological Society, London, Special Publications, 106, 1, 153-18410.1144/GSL.SP.1996.106.01.11, 1996.
Hall, R.: Cenozoic geological and plate tectonic evolution of SE Asia and the SW Pacific: computer-based reconstructions, model and animations, Journal of Asian Earth Sciences, 20, 4, 353-43110.1016/S0012-821X(04)00070-6, 2002.
Heine, C., and Müller, R.: Late Jurassic rifting along the Australian North West Shelf: margin geometry and spreading ridge configuration, Australian Journal of Earth Sciences, 52, 1, 27-39, 2005.
Heine, C., Zoethout, J., and Müller, R. D.: Kinematics of the South Atlantic rift, Solid Earth, 4, 2, 215-25310.5194/se-4-215-2013, 2013.
Honza, E., and Fujioka, K.: Formation of arcs and backarc basins inferred from the tectonic evolution of Southeast Asia since the Late Cretaceous, Tectonophysics, 384, 1, 23-53, 2004.
Hutchison, C. S.: The ‘Rajang accretionary prism’and ‘Lupar Line’problem of Borneo, Geological Society, London, Special Publications, 106, 1, 247-261, 1996.
Hutchison, C. S., Bergman, S. C., Swauger, D. A., and Graves, J. E.: A Miocene collisional belt in north Borneo: uplift mechanism and isostatic adjustment quantified by thermochronology, Journal of the Geological Society, 157, 4, 783-793, 2000.
Hutchison, C. S.: Oroclines and paleomagnetism in Borneo and South-East Asia, Tectonophysics, 496, 1, 53-67, 2010.
Karig, D. E.: Accreted terranes in the northern part of the Philippine archipelago, Tectonics, 2, 2, 211-236, 1983.

Katili, J.: Volcanism and plate tectonics in the Indonesian island arcs, Tectonophysics, 26, 3, 165-188, 1975.
Katili, J.: Geology of Southeast Asia with particular reference to the South China Sea, Energy, 6, 11, 1075, 1077-1091, 1981.
Lee, T.-Y., and Lawver, L. A.: Cenozoic plate reconstruction of Southeast Asia, Tectonophysics, 251, 1-4, 85-138, 1995.
Metcalfe, I.: Origin and assembly of south-east Asian continental terranes, Geological Society, London, Special Publications, 37, 1, 101-11810.1144/GSL.SP.1988.037.01.08 1988.
Milsom, J.: Stratigraphic constraints on suture models for eastern Indonesia, Journal of Asian Earth Sciences, 18, 6, 761-77910.1016/S1367-9120(00)00035-3, 2000.
Parkinson, C., Miyazaki, K., Wakita, K., Barber, A., and Carswell, D.: An overview and tectonic synthesis of the pre-Tertiary very-high-pressure metamorphic and associated rocks of Java, Sulawesi and Kalimantan, Indonesia, Island Arc, 7, 1-2, 184-200, 1998.
Polvé, M., Maury, R., Bellon, H., Rangin, C., Priadi, B., Yuwono, S., Joron, J., and Atmadja, R. S.: Magmatic evolution of Sulawesi (Indonesia): constraints on the Cenozoic geodynamic history of the Sundaland active margin, Tectonophysics, 272, 1, 69-92, 1997.
Pubellier, M., and Meresse, F.: Phanerozoic growth of Asia: Geodynamic processes and evolution, Journal of Asian Earth Sciences, 72118-12810.1016/j.jseaes.2012.06.013, 2013.
Quevedo, L., Hansra, B., Morra, G., Butterworth, N., and Müller, R.: Oblique mid ocean ridge subduction modelling with the parallel fast multipole boundary element method, Computational Mechanics1-9, 2013.
Rangin, C., Stephan, J., and Müller, C.: Middle Oligocene oceanic crust of South China Sea jammed into Mindoro collision zone (Philippines), Geology, 13425-428, 1985.
Seton, M., Müller, R., Zahirovic, S., Gaina, C., Torsvik, T., Shephard, G., Talsma, A., Gurnis, M., Turner, M., Maus, S., and Chandler, M.: Global continental and ocean basin reconstructions since 200 Ma, Earth-Science Reviews, 113, 3-4, 212-27010.1016/j.earscirev.2012.03.002, 2012.
Stadler, G., Gurnis, M., Burstedde, C., Wilcox, L. C., Alisic, L., and Ghattas, O.: The dynamics of plate tectonics and mantle flow: From local to global scales, science, 329, 5995, 1033-1038 10.1126/science.1191223 2010.
Stampfli, G. M., von Raumer, J. F., and Borel, G. D.: Paleozoic evolution of pre-Variscan terranes: from Gondwana to the Variscan collision, in: Variscan-Appalachian Dynamics: The Building of the Late Paleozoic Basement, edited by: Catalan, J., Hatcher, R., Arenas, R., and Garcia, F., Geological Society of America Special Paper 364, Boulder, Colorado, 263-280, 2002.
Van Der Meer, D., Spakman, W., van Hinsbergen, D., Amaru, M., and Torsvik, T.: Towards absolute plate motions constrained by lower-mantle slab remnants, Nature Geoscience, 336-4010.1038/ngeo708, 2010.
Williams, S., Whittaker, J., and Müller, R.: Full-fit, palinspastic reconstruction of the conjugate Australian-Antarctic margins, Tectonics, 30, 6, 21, TC6012, 10.1029/2011TC002912, 2011.
Yumul Jr, G. P., Dimalanta, C. B., Tamayo Jr, R. A., and Maury, R. C.: Collision, subduction and accretion events in the Philippines: A synthesis, Island Arc, 12, 2, 77-91, 2003.
Zahirovic, S., Müller, R. D., Seton, M., Flament, N., Gurnis, M., and Whittaker, J.: Insights on the kinematics of the India-Eurasia collision from global geodynamic models, Geochemistry Geophysics Geosystems, 13, Q04W11, 10.1029/2011gc003883, 2012.