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Interactive comment on "Future accreted terranes: a compilation of island arcs, oceanic plateaus, submarine ridges, seamounts, and continental fragments" *by* J. L. Tetreault and S. J. H. Buiter

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Response to Review C750: W. Collins

I thank W. Collins for his insightful review and suggestions; I believe they will improve the manuscript a lot. I will respond to each point below:

Abstract: The reviewer writes:

I would have added backarc basins to this collection of accreted materials. OK, some of this will subduct or obduct to form ophiolites, but commonly the overlying sedimentary pile is not included in this menagerie, but it might be the most abundant material to

accrete, because it is usually too buoyant to subduct.

Because of the thin basin crusts (ignoring the sedimentary package), back-arc basins were not considered for this compilation. And often back-arc basins become autochthonous terranes, due to their location close to the subduction zone. But I do recognize, as the reviewer points out, that depending on the polarity of the subduction zone these regions can also become accreted allochthonous terranes (which is the focus of this paper), therefore I have added more discussion of back-arc basins throughout the entire paper.

P 1457 line 17. The reviewer writes:

Interesting that the reference for an explanation of "flake tectonics" is Oxburgh 1972. Personally, I think it is almost mechanically impossible. What is a "strong wedge" anyway? Accretion of Quesnellia, Stikinia, and Cache Creek terranes in the Canadian cordillera are better explained by backarc closure following slab flipping (eg Colpron and Nelson, 2007 GSA Today 17).

While I myself may be also sceptical of flake-tectonics, I felt that I should include the theory of flake tectonics because this is a review paper and because the term is still used (see Snyder et al., 2009 GSA Bulletin; Gogus et al., 2011 G-cubed) to describe large-scale lateral displacement of thin-skin terranes or even the tectonic wedge in the Alps (Schmid et al., 1996 Tectonics, Moore and Wiltschko, 2004 Tectonics). I have included a paragraph on back-arc basin closure and tectonic flipping in the section titled "Accretionary processes" as another method of terrane accretion and an alternative to the flake tectonic model. Because Colpron and Nelson (2007 GSA Today) do not explicitly propose accretion by back-arc basin closure, I did not include it as an example. In addition, back-arc closure does not explain the several hundreds of kilometers of the inland translation and decoupling of the terranes' upper crust from the rest of the crust and mantle. However, several other references have been inserted to address back-arc basin closure as a possible alternative.

P 1473 line 15. The reviewer comments:

I would suggest that the most common way to form microcontinental ribbons, at least for the SW Pacific, is by extension and rifting from the active continental margin during subduction retreat (read Schellart papers). This is the way most backarc basins form, with the retreating arc and forearc becoming isolated as the continental ribbon. However, this process does not explain the continental fragments embedded in the Atlantic Ocean floor, and other processes such as heterogeneous hyper-extension of passive margins along pre-existing structural discontinuities, are required to form them.

This is a good point and an embarrassing oversight on my part, especially since I include many continental fragments from the eastern coast of Australia, which have formed from back-arc spreading! I have corrected this and thank the reviewer for catching this omission.

P 1476 line 1. The reviewer writes:

It is stated that "FATs will combine before accreting onto a continent". Note that the microcontinental ribbons in the SW Pacific are on the upper plate, inboard from the active subduction zone, with the Pacific plate as the primary subduction driver. I would argue that FAT combination is a necessary condition for accretion, such as in the SW Pacific. If the oceanic plateau is on the primary subducting plate, the delay in its conversion to eclogite is the reason why it remains buoyant and thus "collides" with the continental ribbon. Both can then be accreted onto the continent as a composite terrane.

I definitely agree with the reviewer on the point that composite FATs will most likely lead to accretion. And as the study of Johnston (2008 Annual Reviews of Earth Planet Sci) showed, the preconception that many of the small terranes forming the North American Cordilleran were joined to North America by separate and numerous subduction and accretion events was replaced by the concept of terranes combining offboard the continent before the final accretionary event to North America. However, I would not use as strong as a term as "necessary condition" to describe terrane accretion. Obviously,

small submarine ridges, island arcs, oceanic plateaus, etc can accrete to a continent without first colliding and combining with another FAT (and there are many examples in my manuscript of accreted which are now supplemented with modern examples of terrane accretion). I believe my wording in the section "composite terranes" was strong enough to suggest that composite FATs will indeed collide and accrete rather than subduct. So I will leave the section as it is.

P 1478 line 18. The reviewer comments:

The statement: "The ability of continental crust to subduct has been documented in the coesite found in exhumed ultrahigh pressure terranes" might be correct, but it is a corollary that these units came back to the Earth's surface. Hyper- extended crustal fragments in subducting oceanic crust will obviously be dragged into the mantle, but then it becomes an issue of fragment size if it returns, when buoyancy contrast becomes sufficient to induce slab breakoff. Clearly, breakoff is required to get such fragments back to the surface, largely by a process of isostatic rebound.

After re-reading my paragraph, I can see why the reviewer was confused and responded to that statement as such. I think that paragraph is supposed to be in the next section ("From FAT to accreted terrane") and that sentence itself is awkwardly placed. I have rewritten that paragraph to better suit the section it is in and moved the statement about continental crust subductability to the following section. I have addressed exhumation in the new paragraph.

P 1480 line 13. The reviewer writes:

To understand the fate of lower crustal material along active plate mar- gins, one needs to go no further than the study by Hyndman et al 2005. GSA Today 15. They provide compelling evidence that the wide backarc (orogenic) regions are invariably hot because of shallow asthenosphere convection, facilitated by water derived by dehydration of the underlying subducting slab. See their subsequent papers as well. Thus, delamination is not nearly as important as convective removal of arc-type lower crust, which is *a simple consequence of subduction and an elegant explanation for modern high heat flow around the circum-Pacific. Inferences made from seismic or crystal fractionation modelling are largely unnecessary if this global-scale phenomenon is considered.*

The reviewer comments that lower crustal removal during terrane accretion of island arcs will occur by small-scale convective removal during back-arc extension (Hyndman et al., 2005) and that other processes, such as foundering of dense cumulates or crustal shearing during accretion, are not the primary mechanisms. This is a very interesting theory, and I now include it in the section "FAT to accreted terrane." And, I would even go further to suggest that that small-scale convection in the back-arc could work in concert with the negative buoyancy of the arc cumulates and convection to aid lower crustal foundering.

However, I must remark that the reviewer is using convective removal in the foreland (continental back-arc) to explain lower crustal removal of a terrane on the opposite side of the subduction zone. I am discussing the removal of lower crust from a FAT during accretion/collision. Once a FAT is accreted and becomes part of the back-arc mobile belt, then it is possible that post-accretion removal of crust and lithosphere is possible by small-scale convection. But to reiterate, I think this is an interesting processes that should be discussed, and have added it to the paper.

P 1481 line 5. The reviewer states:

I disagree with the comment that "The nature of the accretionary prism region can be either erosive or accretionary depending on the sedimentary and erosive fluxes (Clift and Vannucchi, 2004; Scholl and von Huene, 2010). It is much more likely to relate to the tectonic driving forces associated with subduction. Ultimately, whether a mountain range like the Andes exists to supply sediment via erosion, or whether it is an island arc with no erosive or sedimentation capacity, is not determined by sedimentary and erosive fluxes, but by the fundamental drivers of plate tectonics themselves.

I understand the reviewer's point, and I agree that the convergence rate affects the

type of margin that a subduction zone has. In my original sentence I was assuming sedimentary fluxes related to tectonic uplift, but that is not clear. I believe that relating the type of margin to the convergence rate is a better statement. And, the relation of convergence rate to accretionary vs erosive margins is quantified in Clift and Vanucci (2004), supporting the reviewer's point. I have corrected the statement in the paper.

My coauthor and I would like to once again thank W. Collins for his considerate review. I felt the addition of back-arc basin closure was a very insightful addition to this paper. And I am glad W. Collins caught my missing formation mechanism for continental fragments (back-arc spreading), so that I could remedy it before the final version. I hope that I have sufficiently addressed all of the concerns and suggestions of the reviewer in my revised manuscript.

Sincerely, Joya Tetreault

Interactive comment on Solid Earth Discuss., 6, 1451, 2014.