

## ***Interactive comment on “Arrested development – a comparative analysis of multilayer corona textures in metamorphic rocks” by Paula P. Ogilvie and Roger Lawrence R. L. Gibson***

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The manuscript is a very nice piece of work summarizing the occurrence and modelling of corona texture in granulite facies rocks. However, although the title of the paper promises to talk about coronas in general, the manuscript itself limits itself to high T, granulite facies corona formation. There are many more examples, for lower T, including various experimental work in particular from the Putnis group (e.g. Niedermayer 2009) or others (e.g., Jonas et. al, 2015) describing mineral replacement that produces corona-like structures. I would like to see an experimental section included in this review. This goes alongside a missing description of reaction fronts (e.g. work of Bastian Joachim or Vanessa Helpa recently done at GFZ). The authors touch this

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towards the end displaying (surprisingly) the only equation in the full manuscript.

The manuscript is very wordy and would largely benefit from overall shortening that eliminates multiple repetitions in order to make it more concise. Although very well written, I do have the feeling that the manuscript would also benefit from some language polishing. There are many weird expressions frequently used that do not sound adequate (probably a job for the AE??). I have attached an annotated pdf that will hopefully help the authors to improve the paper and indicate potential sections (e.g. the discussion that is rather a summary and repetition) that could be deleted.

Description of chemical potentials is often unclear. It is the chemical potential of a component within a phase and not just the chemical potential of Si, Ca or Al. This makes it hard to follow several parts in the manuscript. Figures should illustrate the gradients in chemical potentials more clearly as they currently do.

It appears to me that the authors are somewhat unfamiliar with the underlying concepts of element transport. For example, diffusivities (which refer to diffusion coefficients) are substantially different from diffusive fluxes. Yet, the manuscript often uses them interchangeably, but it makes a huge difference. To the same end, I am really surprised to read a review describing corona formation with mentioning Onsager coefficients just a single time (and even not correctly as Onsager coefficients are not equal to diffusion coefficients)! It is correct that these can be ultimately coupled to diffusive fluxes (thanks to the work of Rainer Abart and his group – unfortunately not cited in the review), but a complete review should include a full description of the transport properties. To the same end, I wonder why the seminal Dohmen and Chakraborty paper (2003) explaining the different reaction regimes and formation of steady state fluxes is nowhere mentioned.

Taken together, I think the manuscript does a pretty good job in summarizing previous work and it contains serious issues when it is coming down to explain the underlying concepts, in particular transport processes. I think that this manuscript needs signifi-

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cant revision to be published.

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/se-2016-97/se-2016-97-RC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-97, 2016.

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