

## ***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 authors greatly appreciate and thank the referees for their considered comments and suggestions. In all instances, the authors have addressed the reviewers concerns and amended the manuscript accordingly. An annotated manuscript tracks the extent of these changes (attached as supplement) Major issues of concern to the referees and corrective action taken by the authors are summarised below:

1. Omission of a detailed discussion of reaction kinetic (diffusion control on reaction), experimental work and low-temperature coronas. The authors have amended the introduction so as to acknowledge the occurrence of low temperature corona/replacement textures. We believe that an exhaustive review of the latter is beyond the scope the

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review. To provide clarity, the paper now expressly states that the focus is on high-temperature, granulite facies coronas.

A section is now dedicated to an overview of reaction kinetics and experimental work which has constrained our understanding of interface/diffusion controlled reaction rates. This includes a rigorous treatment of theory and equations underpinning corona modelling. Key references to experimental work and the application to corona modelling are now cited and acknowledged. Referee 1 also stresses that phase equilibria modelling in Thermocalc is only able to model fragments of the P-T-X evolution of the corona, i.e., it is unable to fully model the vagaries of diffusion controlled phase equilibria as a function of time. We are thus restricted to ‘snap-shots’ in the P-T-X path. The authors acknowledge this limitation and propose that future corona investigations should consider both.

2. Repetition and shortening of the manuscript In all instances where both referees flagged concerns regarding repetition/tautology, the authors have removed text. However, we propose that a ‘Summary and Conclusions’ section is still useful in consolidating the main findings of the review. To this end, we have attempted to keep this section as brief as possible to avoid any redundant statements and remain as clear and concise as possible.

3. Inconsistent and/or erroneous application of kinetic theory and terminology The authors have ensured that throughout the text, the terms ‘diffusion, diffusivities, chemical potential and component flux’ are now clearly defined (with reference to the newly added section summarising Kinetic theory) and utilised appropriately. In all instances where chemical potentials are discussed in the text, the authors have ensured that it is clear that the potential refer to those of the component in a phase in the local equilibrium considered. This is now clearly stated in the relevant figure captions as well.

4. Limiting model of corona formation to end-member processes, i.e., single-stage vs. sequential models With regard to corona reaction mechanism models, i.e., single-stage

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vs. sequential models, both referees stress the importance of interpreting a corona as a combination of the two end-member mechanisms. To this end, the authors more clearly define the two corona formation mechanism models as end-member scenarios, each characterised by distinct textural and chemical potential signatures with very different petrogenetic implications. It is stressed that an understanding of these inherent limitations is required if an appropriate interpretation of P-T evolution is to be inferred from a corona.

5. Misleading assertion that coronas form in mafic granulites owing to an intrinsic anhydrous bulk composition. The manuscript now expressly states that coronas reviewed in mafic bulk rock compositions are associated with igneous mafic precursors with a low  $a_{H_2O}$ .

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/se-2016-97/se-2016-97-AC1-supplement.pdf>

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