



Interactive comment on “Evaluation of the US DOE’s conceptual model of hydrothermal activity at Yucca Mountain, Nevada” by Y. V. Dublyansky

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Received and published: 22 January 2013

This paper is well written and is a valuable and carefully considered contribution to the interpretation of evidence from secondary minerals and fluid inclusions for the nature of post-depositional heating in volcanic tuffs at Yucca Mountain. Essentially, it promotes the consideration of an alternative model, involving later hydrothermal activity, for the thermal alteration which has been previously attributed to predominantly conductive heating from remote volcanicity of a water-rock system where the water is derived from normal meteoric infiltration (the MICH model). The significance of the proposed model for hydrothermal alteration is that it would post-date, and be independent of, cooling of Miocene intrusions and therefore would support the argument that future hydrothermal activity should be considered as a plausible scenario in the safety case for a repository

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at Yucca Mountain. In such cases, it is important that one or more alternative conceptual model is fully considered, and this paper provides such an alternative model and critiques the acceptance of the original model reported by DOE. The argument put forward is that the fluid inclusion data and the spatial distribution of inferred temperatures does not match the modelling results as well as DOE have suggested. That, in my opinion, is a valuable and carefully argued case against the acceptability of the model. In some parts of the text, I feel that the author's critique is too fine-detailed (e.g. in sections 4.3 & 4.4) and fails to take into account the real uncertainties in both the model outputs and the fluid inclusion data. That underestimation of uncertainties would be an error in both DOE's work (e.g. the fluid inclusion temperatures almost certainly have higher uncertainties especially at low values; for example the scatter in Figure 3 probably indicates the real scale of uncertainties) and in the author's interpretation. Figure 4 is a compelling comparison of modelled T versus fluid inclusion data but probably does not adequately represent the data uncertainties. Also, there are not really sufficient 'benchmark' data to allow a comprehensive evaluation. I cannot comment on the relative merits of the modelling with the HEAT and HYDROTHERM codes (and TOUGH2) because I don't have expertise in that area. The basic question is about the genesis of reported secondary minerals and associated fluid inclusions with elevated temperatures (with respect to ambient) in the unsaturated zone. The problem that I have in this respect is that the present author does not present the alternative 'hydrothermal' model in any useful degree of detail. The alternative model is not supported by a detailed re-interpretation of the secondary minerals in the tuffs and the inference of hydrothermal fluid flow paths. In my opinion, I would like to see an interpretation of the geological and mineralogical evidence for hydrothermal source and flow, coupled with a hydrodynamic model that simulates the hydrogeological processes. Dr Dublyansky would suggest that his previous publications, cited in this paper, provide that degree of re-interpretation and modelling, but in my opinion (provided in previous communications with him) there is still insufficient detail in that alternative model.

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Interactive comment on Geosci. Model Dev. Discuss., 5, 3853, 2012.

GMDD

5, C1319–C1321, 2013

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