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Interactive Comment

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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I was glad to see that the reviewer had no issues with the scientific content of the paper and its conclusions. All concerns were primarily with the form of the paper, and the way the data are presented in it.

Below I provide responses to specific comments.

AB: ". . . 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."

Although the reviewer is correct in stating that this paper critiques the acceptance of the original DOE model, it does NOT provide a model alternative to the DOE one.

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AB: "In some parts of the text, I feel that the author's critique is too fine-detailed and fails to take into account the real uncertainties in both the model outputs and the fluid inclusion data."

It is not clear what the "real" uncertainties mentioned by the reviewer are. The uncertainties of the temperatures (based on fluid inclusions and stable isotopes) used to construct the benchmark are discussed in considerable detail in section 4.1. Actually, on the basis of this discussion, the stable isotope data have been excluded from the benchmark as having too high uncertainties!

For the purpose of this evaluation the U-Pb ages (and the associated uncertainties reported in original publications) were assumed valid. This may not be the case, as discussed in Pashenko and Dublyansky (2006). The validity assumption, however, is necessary, because if the U-Pb ages are not valid, both the USGS-DOE model, and this evaluation become meaningless.

Uncertainties in the modeling outputs of Whelan et al. (2008) are not quantifiable, because of the inadequate presentation (two curves on a graph; very limited discussion of the modeling uncertainties). Actually, I argue that the results presented by Whelan et al. (2008) are likely non-conservative (over-estimates).

In summary, I do not think that criticism regarding the uncertainties is justified.

AB: "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."

Again, I have to disagree. Uncertainties in fluid inclusion temperatures are shown in Fig. 3 as error bars, which were calculated from the original raw data. What other

GMDD

5, C1658–C1661, 2013

uncertainties does the reviewer want me to present?

The belief of the reviewer that the uncertainties of fluid inclusion temperatures are "almost certainly" higher has no basis. All experts who took part in the research or reviewed the data agree that the technical quality of the fluid inclusion data obtained from Yucca Mountain samples is exceptional. For example, it was not uncommon that as many as 25-30 measurements of homogenization temperatures from the same Fluid Inclusion Assemblages fell within a 5-7°C-interval. This shows an excellent data consistency, according to standards used in fluid inclusion research (e.g., Goldstein and Reynolds, 1994). In addition, for many samples, identical distributions of fluid inclusion temperatures were obtained independently in as many as three laboratories (Institute of Mineralogy and Petrography of the Russian Academy of Sciences, University of Nevada Las Vegas, and US Geological Survey). Summarizing, the consistency of the fluid inclusion results in the Yucca Mountain studies, and the degree to which this consistency was confirmed, is almost unsurpassed.

AB: "Also, there are not really sufficient 'benchmark' data to allow a comprehensive evaluation."

The benchmark compiled in Fig. 3 of this paper includes ALL relevant data available after almost three decades of research at Yucca Mountain. In this respect, the evaluation is as comprehensive as it could possibly be. If these data are not sufficient for comprehensive evaluation of the model, then, it can be argued, they are equally insufficient for US DOE to base a reliable model upon.

AB: ". . . 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 reinterpretation 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."

GMDD
5, C1658–C1661, 2013

Not only does this paper not present a “sufficiently detailed” description of an alternative model; actually, it does not present it at all. This paper is an EVALUATION of the model officially accepted by US DOE. Presentation of alternative models is not an intrinsic component of such evaluation. Furthermore, such presentation, particularly in a “useful degree of detail”, is simply not feasible within the framework of this paper.

I am quite sympathetic with the wish of the reviewer to see the alternative model formally presented. This, however, would be an entirely new and entirely different paper. Given the character and the volume of evidence which would need to be discussed (geochemistry, mineralogy, etc.) GMD would hardly be the primary choice for publication of such a paper.

AB: "Dr Dublyansky would suggest that his previous publications, cited in this paper, provide that degree of reinterpretation and modelling, but in my opinion (provided in previous communications with him) there is still insufficient detail in that alternative model."

I need to point out that the issue raised by the reviewer in this comment is detached from the paper under review. Issues with the content of an alternative model do not make this evaluation less important, as they do not make the wrong model right.

References for this response

Pashenko S.E., Dublyansky Y.V. (2006): Migration of radiogenic lead isotopes during formation of minerals in open cavities in the presence of colloids: theoretical aspects as applied to U-Pb dating of young minerals. Russian Geology and Geophysics, 47(2), 201-215.

Goldstein R.H., Reynolds T.J (1994): Systematics of fluid inclusions in diagenetic minerals. SEPM Short Course 31, Tulsa, Oklahoma

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