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Interactive comment on "Evaluation of the US DOE's conceptual model of hydrothermal activity at Yucca Mountain, Nevada" *by* Y. V. Dublyansky

Anonymous Referee #2

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The paper, "Evaluation of the US DOE's conceptual model of hydrothermal activity at Yucca Mountain, Nevada" by Y.V. Dublyansky, provides a detailed review and extensive comments about previously proposed explanations for high depositional temperatures measured in fluid inclusions from the unsaturated zone of Yucca Mountain, Nevada. The focus is on the lack of model results to explain these high depositional temperatures. The author provides extensive review of Yucca Mountain and its thermal history, previously published benchmark data, and previously published model results to explain, in detail, the key point that there is not yet a published model that explains all observations. The paper, however, provides little new information, no new data, and no new model results. Much of the paper is a restatement of previously published information and arguments (e.g., in Dublyansky et al., 2004 and Dublyansky and Polyansky, 2007), and many of the figures have been published previously. There are some im-

portant points made by the author. However, these points would be more accessible and more convincing if they were made in a concise and succinct manner. A major concern is with the length and unnecessary detail, and with the republishing of significant amounts of previously published material. These concerns could be addressed by eliminating details and figures related to arguments that have been published previously, and focusing instead on writing a concise comment that addresses the key points about the modeling:

1. No computational modeling results have yet produced a satisfactory match with all of the empirical benchmark data, specifically with fluid inclusion data that indicate high temperatures (up to ∼80C) in the unsaturated zone (e.g., as shown in Figure 4). 2. The current published explanations to account for these highest temperatures in fluid inclusions have not been adequately explained or reproduced using computational models (i.e., computational models have not been published that examine either development of vapor-convection cells/shallow-rooted fumarolic systems (as proposed by Whelan et al., 2008) or deep-seated hydrothermal fluids into the unsaturated zone (as proposed by Dublyansky and Polyanksy, 2007) to account for the high fluid inclusion temperatures. 3. As noted in the second paragraph of the introduction, if the safety case for Yucca Mountain as a disposal facility rests on water having been scarce in this zone in the past, then the possibility that there may have been past hydrothermal activity should be considered.

In this way the contribution would become a much more accessible and readable paper, specifically a comment on "Thermal history of the unsaturated zone in Yucca Mountain, Nevada, USA" by Whelan et al., 2008, and it could be shortened considerably. The key points could be made in ∼10% or less of the space currently used, with prior work referenced as appropriate. In addition, within this comment it needs to be acknowledged that, while the case is made that the existing conduction model does not adequately explain all benchmark data, there also are no published alternative computational models (e.g., that consider hydrothermal activity or fumarolic activity) that provide a match

with all empirical data.

In summary, because this paper dominantly refutes previously published material but does not provide new data or model results, it should be shortened to present only new material and new arguments, and should be considered as a comment. More in depth suggestions for shortening the paper and tightening the key arguments are given below:

1. Section 1 at the end states that "no formal evaluations of this model have been published," but Dublyansky and Polyansky published a detailed evaluation in 2007 (that is reviewed here extensively).

2. Information in sections 2 and 3 is well covered in both Whelan et al. 2008 and Dublyansky and Polyansky, 2007 (though the term MICH model is not used), and does not need to be discussed in this level of detail. Figure 1 is from Dublyansky and Polyansky 2007 (Fig 5 in that publication) with the only difference being the addition of locations for 45degC and 65degC fluid inclusion data points.

3. Section 4 and evaluation of the benchmark has also been covered extensively in previous published work, with Figure 2 identical to Dublyansky and Polyansky 2007 figure 4, and does need to be presented in this level of detail.

4. Figure 3 is a useful summary of the author's improved benchmark, and Figure 4 is a concise summary of fits of existing data (the improved benchmark) and previously published model results of Whelan 2001, Dublyansky and Polyansky 2007, and Whelan et al., 2008.

5. Much of the rest of section 4, 5, 6 and 7 are discussed in Dublyansky and Polyansky 2007, with details of fluid inclusion and isotope data having been published earlier in Dublyansky, 2001, Whelan et al., 2001, 2002, 2004, and Dublyansky et al., 2004, and Figure 5 is identical to Figure 6b of Dublyansky and Polyansky, 2007. The extensive presentations/comments/replies on fluid inclusions in the past do not need to be

repeated given that there is, in general, an acceptance that there is evidence of high temperatures (e.g., Whelan et al., 2008).

6. Section 7, the discussion and conclusions, could be more concise.

7. Information in appendices has been published previously, with Figure 6a and 6c being similar to figure 6a and 6b of Dublyansky and Polyansky 2007. This information does not need to be presented in detail, and as written it detracts from the main point.

The major point, that there is still no computational model of conductive heating, or of vapor-phase convection in the vadose zone (fumarolic activity), or of hydrothermal activity that reproduces observed high temperatures in calcite and fluorite is currently buried in detail and repetitive arguments. The point would be made more strongly and convincingly if the paper was limited to presentation of new information, and a concise summary of previously published material (e.g., as shown in figures 3 and 4) that supports this main point.

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