Geosci. Model Dev. Discuss., 5, C1662–C1670, 2013
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Geoscientific Model Development Discussions
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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

Y. Dublyansky

kyoto_yuri@mail.ru

Received and published: 23 October 2013

I was glad to see that the reviewer had no issues with the scientific content of the paper and its conclusions. The reviewer's concerns were primarily with the form of the paper, and the way the data are presented in it. The main request of the reviewer is:

(1) The reviewer wants me to shorten the paper to about 10% of its present length and, essentially, re-write it as a comment on "Thermal history of the unsaturated zone in Yucca Mountain, Nevada, USA" by Whelan et al. (2008).

The main "technical" points of criticism are:

(2) The reviewer thought that the paper presents "no new data, and no new model results". (3) The reviewer also opined that the paper contains too much previously

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published information. (4) The reviewer thought that the paper contains excessive or even unnecessary details.

Below I explain my position with respect to the request and criticism above.

AR point 1: make it a short commentary paper

This paper was written as a COMPREHENSIVE EVALUATION of the conceptual model proposed by the US Geological Survey and officially accepted by the US Department of Energy. The latter model was presented in numerous publications between 2000 (Marshall and Whelan) and 2010 (Houseworth and Hardin). The evaluation, therefore, cannot be reduced to a mere commentary on Whelan et al. (2008) without losing its comprehensiveness.

The time is ripe for such a comprehensive evaluation because all studies at Yucca Mountain were closed in 2008, and models were included in the "official" documentation of the US DOE supporting its Yucca Mountain License Application (submitted to US Nuclear Regulatory Commission in 2008). It is unlikely, therefore, that the conceptual model which is being evaluated in this paper will change significantly.

In summary, with due respect, I do not think that this request of the reviewer could (or should) be accommodated.

AR point 2: no new data/model results are presented in the paper

Apparently, this comment stems from a (mistaken) presumption, on the part of the reviewer, that he reviews a scientific-report paper. However, as was explained above (and as stated in the paper's title), it is a MODEL EVALUATION paper.

By its very nature, evaluations deal primarily with existing data and models. Unlike scientific report-type publications, evaluations are not supposed to present new factual data. New information (understanding, scientific knowledge) is produced in such papers through analysis of the existing information.

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AR point 3: reporting already published information

Partial evaluations of the USGS-DOE model were published previously. These evaluations were not comprehensive, dealing with certain aspects of the model available (published) at that time. These partial evaluations became (partly) outdated, as the model was modified and updated in subsequent publications by the proponents. Also, the "data mining" in the collection of the US DOE documents placed in Licensing Support Network, resulted in finding new important data that were not available earlier.

Referring a reader to previous partial evaluations, as the reviewer suggested, and explaining meanwhile, which parts of these texts are still relevant and which are no longer so, is simply not practical. For a reader, digesting such information would be extremely difficult and time-consuming. Instead, to ensure comprehensiveness of my evaluation, I took the approach of including the still-relevant arguments in this paper, albeit in a strongly condensed form.

AR point 4: excessive or unnecessary details

I strived to prepare a document, in which every step, every statement, and every conclusion is solidly backed up by both the factual data and transparent reasoning. The reason why I took this seemingly over-rigorous approach lies in the rather extraordinary conclusion of my evaluation: that despite years of research, two high-level US Government agencies failed to produce an acceptable (scientifically defensible) model for a process, which may be critically important for the safety of a high-level nuclear waste disposal facility. The failure was not recognized, and the problematic model was relied upon in the performance assessment of the facility.

I felt that these conclusions, and their potential consequences, are serious enough to warrant the most scrupulous attention to detail. Obviously, such serious charges cannot and should not be made lightly.

The paper was written for an interested and attentive reader, willing to spend time and

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expend effort understanding the details. Such a reader, I believe, will not find the details "unnecessary" and the level of detail excessive. A less rigorous reader could just as well hop to the conclusions.

Responses to more-specific comments (numbered comments on pages C1656-C1657) of the reviewer are provided below.

AR1: 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).

This statement is not factually correct. In contrast to this paper, the "conductive heating" model in Dublyansky and Polyansky 2007 was tested primarily by means of the numeric modeling (i.e., the cited paper reported the original numeric modeling results). This was hardly a comprehensive evaluation. More importantly, the paper of Dublyansky and Polyansky was published before the latest version of the "conductive heating" model, which showed a dramatically improved match to the benchmark, was published in Whelan et al. (2008), and before some auxiliary conceptual models were proposed by Houseworth and Hardin (2010).

AR2: 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 45oC and 65oC fluid inclusion data points.

The sections 2 and 3 are introductory. They set the stage for the evaluation and explain the character of the data on which the model is based. I do not see how a reader would understand the nature of the model without this basic information.

Information in these two sections is given in abbreviated, almost abstract-like form. For example, all information on paleotemperatures determined from secondary minerals

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takes 16 lines. The known thermal history of Yucca Mountain is squeezed into 27 lines. It would be extremely inconvenient and unproductive for a reader to be forced to go and dig for this information in other publications.

The early version of Fig. 1, indeed, appears in Dublyansky and Polyansky 2007 (in which it was modified from Bish and Aronson, 1993). I feel it is very important to present it in this evaluation, as the figure gives a concise "snapshot" visualization of the thermal situation during the Timber Mountain Caldera event. Even more important is the modification of the figure for this publication: it now includes two additional temperature data points, which were not known to me earlier; these temperatures are very important as factual constraints on the paleotemperature field, and I would not know how to introduce them into discussion without showing them graphically in relation with pertinent features (caldera, hydrothermal plume, water table, ESF footprint, etc.). The bottom line is: the figure is absolutely essential and should not be removed.

AR3: 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.

With due respect, this is not correct. The benchmark has NOT been evaluated previously. In all previous publications, including Dublyansky and Polyansky (2007) and Whelan et al. (2008), it was used in the form it was introduced in Whelan et al. (2003). My evaluation in section 4.1 shows that the benchmark is deficient and requires revision. In section 4.3 I propose an improved benchmark. This topic, absolutely, has not been "covered extensively in previous work".

Figure 2 (early version of the benchmark) is, in fact, reproduced from Whelan et al. (2003). I believe it must be retained. In Section 4.1 I discuss and evaluate specific components of this figure. For example, I reconsider how colored lines were constructed and conclude that the methodology was erroneous and that the lines are largely unreliable (subsection "Best-fit curves" on p. 3861). I imagine that it would be awkward (and

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extremely inconvenient for a reader) if in section 4.1 I based my discussion on a figure, located in a different paper.

AR5: 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).

Again, this statement is not entirely correct. Sections 4.3 and 4.4 are dedicated to the new numeric simulations, reported in Whelan et al. (2008). Obviously, this topic cannot have been discussed in Dublyansky and Polyansky (2007). The arguments which were used from previous publication are given in the paper in a very abbreviated form. For example, discussion of petrogenetic models takes 14 lines in this paper, as opposed to Dublyansky and Polyansky (2007), where it takes more than 60 lines. Section 4.5 "Reproducibility of modeling" is entirely original.

Although discussion of three sub-models in section 5 is indeed taken (in shortened form) from Dublyansky and Polyansky (2007), this section also discusses three additional sub-models, introduced after 2007 (subsections: 5.4 Heating by vapor-phase convection, 5.5 Elevated heat flows related to extensional tectonics, and 5.6 Cooling action of meteoric infiltration).

Section 6 is entitled "Additional approaches to validation of the MICH model". This topic must be discussed, as it is part of the regulatory framework of model validation. Two topics discussed in this section are: The analog-system observations (6.1) and Observations on spatial structure of the thermal field (6.2). Nothing new has been done by DOE in terms of the analog-system observation since 2004; the same (negative) results are therefore reported here. Discussion of the spatial structure of the thermal

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field was significantly updated relative to an earlier version (Dublyansky and Polyansky, 2007). The update (lines 13-20 on p. 3873) includes very important constraints on the paleotemperatures obtained by USGS from two boreholes located to the north of Yucca Mountain (see discussion of Fig. 1 in #2 above)

Finally, I do not understand what the reviewer views as "extensive presentations/comments/replies on fluid inclusions in the past" that "do not need to be repeated". The only more-or-less detailed discussion of fluid inclusion data is present in section 6.2 (lines 13-24 in p.3873). The data, however, are not REPEATED here. This data from a borehole located to the north of Yucca Mountain has not been known to me before. The discussion of this data is critically important, as it provides insight into the structure of the paleothermal field.

AR6: Section 7, the discussion and conclusions, could be more concise.

I will do my best to make them more concise.

AR7: 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.

Let us take a closer look at Figure 6 (actually, Figure A1). The panel a shows the depth from surface in the ESF tunnel. This information has not changed; because of that, the panel is indeed similar to that appearing in previous publication. Panel c was significantly updated as new data were published in 2008. Panels b, d, and e provide information, which was not published before. All information presented in this figure is relevant to the discussion and each panel is relied upon in the discussion. I therefore disagree with the assessment of the reviewer.

The reviewer's argument about the distractive effect of the text presented in an appendix seems to defy logic. Information is typically placed in an appendix so as not to detract a reader from the main point. An appendix represents optional reading for a

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person who wants to learn more about the issue under discussion.

AR: 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.

This paper is not aimed at presenting a single "major point". I strived to provide as comprehensive, and up to-date evaluation of the USGS-DOE model as possible. Beside the "major" point, there are many others, bearing on the overall plausibility of the model, which need to be discussed.

ADITIONAL

Not all statements of the anonymous reviewer are technically accurate. For example, he suggested that I address the point that "... computational models have not been published that examine ... deep-seated hydrothermal fluids into the unsaturated zone (as proposed by Dublyansky and Polyanksy, 2007)" (C1655). Contrary to the assertion in parentheses, circulation of deep-seated hydrothermal fluids in the unsaturated zone has not been proposed in the cited publication (actually, it was not even mentioned there).

Not all of them are logically flawless. For example, the reviewer requests: "... 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 ... that provide a match with all empirical data." (C1655-1656). Why would that be necessary? The fact that there is no published alternative model does not make the demonstrably wrong USGS-DOE model right. No matter whether the alternative hypotheses exist or not, the conclusion of this evaluation remains: the

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currently accepted USGS-DOE model in untenable.

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