A Response to the RATE Team Regarding Helium Diffusion in Zircon

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Abstract

A young-earth creationist research program, called RATE for Radioisotopes and the Age of The Earth, claims to have found scientific evidence for the acceleration of nuclear decay rates by several orders of magnitude. This argument is used to defend their belief in a 6000-year-old earth. I had previously challenged the scientific merits of RATE's helium diffusion study, to which Dr. Russell Humphreys, a leading member of the RATE team, responded with his own criticisms. This paper responds to Humphreys' comments. Three topics are discussed: my old-earth helium diffusion model, RATE's young-earth helium diffusion model, and a test between the two models. Regarding my old-earth model, computational evidence is presented supporting my interpretation of the diffusion kinetics, contrary to Humphreys' unsubstantiated claims. Regarding RATE's young-earth model, a case is made that the apparently good agreement between their model and experimental data is the result of adjusting model parameters to fit preliminary data. Finally, regarding a test between the two models, evidence is presented which shows that the initial heating ramp of a diffusion experiment better supports my old-earth model.

1. Introduction

This monograph responds to questions raised in a recent web article by Dr. Russell Humphreys (2008), who is a leading member of a young-earth research program called RATE for Radioisotopes and the Age of The Earth (Vardiman *et al.*, 2000 and 2005). One of RATE's claims is that their model for a young earth, which includes accelerated nuclear decay, is supported by the apparently large quantity of helium found in zircon crystals from geothermal test wells in northern New Mexico (Humphreys *et al.*, 2003b and 2004; Humphreys, 2000 and 2005). Last year Reasons to Believe published two of my articles which not only exposed errors in the RATE research (Loechelt, 2008a), but also demonstrated how the published helium data could be reconciled with an old-earth model (Loechelt, 2008b). Links to these articles can also be found on the ASA website, and for those who are interested, a detailed technical paper is also available (Loechelt, 2008c).

Before addressing some of Humphreys' specific questions, a major fallacy needs to be exposed. He challenges his critics that the burden lies with them to disprove his accelerated nuclear decay model. This assertion is false. Since I had previously demonstrated that an explanation consistent with the established position of the scientific community is possible, an equal if not greater burden lies with Humphreys to disprove that model, which he has not yet done. A good scientist should always give fair and open-minded consideration to all competing ideas, including those that are not his own. For Humphreys to dismiss my work, which was thoroughly documented in a 37 page technical paper (Loechelt, 2008c), with only three paragraphs of unsubstantiated rhetoric in a web article (Humphreys, 2008) demonstrates his lack of serious scholarship.

2. My old-earth diffusion model

One point of disagreement between Humphreys and me is the interpretation of the results of a laboratory diffusion experiment. I proposed that the helium resides in two different states within the crystal: a loosely-bound state and a tightly bound state. There is precedence for this two-state or multidomain diffusion model in the geochemical literature (Reiners and Farley, 1999, pp. 3850-3853; Farley, 2000, pp. 2906-2908; Reiners *et al.*, 2004, pp. 1872-1874; Shuster *et al.*, 2003, pp. 28-29; Shuster *et al.*, 2005, pp. 669-670). Using this multidomain diffusion model, I demonstrated that retention of helium was possible in these zircon crystals over geologic timescales.

Humphreys' objections to this work are briefly answered below. A paraphrase of his statements is enumerated in italics, followed by my response. For a more extended discussion of some of the particular issues, please see the following web article (Loechelt, 2009).

1. *The loosely bound helium in my model would have been exhausted during the initial heating ramp of the diffusion experiment, and cannot explain the high diffusivity that was observed at low temperatures later in the experiment.*

Humphreys made this claim apparently without performing any calculations. In contrast, I had previously performed these calculations. In Appendix C of my technical paper (Loechelt, 2008c), I discussed how the kinetic parameters of my multidomain diffusion model were extracted by performing forward modeling of the stepwise heating diffusion experiment inside a nonlinear optimization loop. Although not shown in that paper, this extraction technique required the explicit calculation of the fraction of gas released during the diffusion experiment by both the high-retentivity and low-retentivity helium domains. The results of these calculations are shown in Fig. 1, using helium diffusion data from the 2004 RATE CRSQ paper (Humphreys *et al.*, 2004).

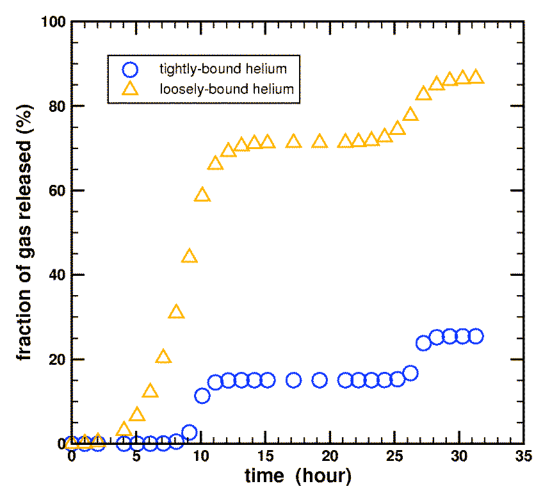

Figure 1. Calculated fraction of helium gas released during a stepwise heating experiment for a two-domain diffusion model.

As can be seen, 30% of the loosely-bound helium (orange triangles) survived the first heating ramp at 15 hours, and 15% even survived the second heating ramp at 30 hours. Therefore, enough loosely-bound helium remained to explain the low-temperature diffusivity through-out the entire experiment. Although the exact gas release values at any given time step are sensitive to the assumptions used in constructing the multidomain model, most notably the partitioning fraction between the two helium domains, it does demonstrate that it is possible for a fraction of the loosely-bound helium to remain throughout the entire experiment, contrary to Humphreys' unsubstantiated claims.

2. *The technical literature did not report any evidence of the loosely-bound helium remaining past the first temperature ramp.*

The experiments in question did not go low enough in temperature for the effect to be noticed. They started at or above 300 °C and remained at higher temperatures for the duration of the experiment (Reiners *et al.*, 2004). In contrast, the effect was only observed in the RATE experiment at temperatures below 300 °C (Humphreys *et al.*, 2004). Furthermore, one author expressed doubt that a single heating ramp could fully deplete the low-retentivity domain in a diffusion experiment, even after removal of 5% of the helium (Farley, 2000, pp. 2907-2908).

3. *The scientist who performed the RATE experiments assured Humphreys that all of the loosely-bound helium was depleted after the initial heating ramp.*

There is no evidence in the RATE publications to support this claim. In fact, in their creation conference paper, one of the original laboratory reports from their researcher was reproduced in Appendix C. In the results section, depletion of helium at the surface and radiation damage were discussed as possible effects, but the presence of loosely-bound helium was not even mentioned (Humphreys *et al.*, 2003b). Based upon my private correspondence with this individual (who is not a member of the RATE research team by the way), I believe his work is being misrepresented by Humphreys.

4. *I misunderstood the geologic literature regarding the thermal history of the site.*

When I made the same claim regarding Humphreys in my technical paper, I backup my allegations with specific examples, including figures and quotations (Loechelt, 2008c). Not only has he avoided addressing any of the particular issues that I raised, he failed to give any supporting evidence to back up his own charges, demonstrating again his poor scholarship (Humphreys, 2008).

Consider the following example. One of the sources cited by Humphreys was a paper by Harrison, Morgan, and Blackwell (Harrison *et al.*, 1986). In this paper, the authors state that

> No reasonable fits to the measured geotherm were obtained for source ages significantly in excess of 40 ka. Thus, if the deep gradient increase in EE-2 is due to conductive heating from a source below Fenton Hill, the source is constrained to be young, much younger than the Valles caldera. (Harrison *et al.*, 1986)

These authors claim that the heating event at Fenton Hill is much younger than the main volcanic eruption that formed the nearby caldera 1.25 million years ago, and place an upper limit on its age around 40 thousand years.

How does Humphreys describe this research? He claims that I failed

> to grasp the essence of the published Los Alamos heat flow models, which is that due to nearby volcanic activity in the past they imagine, temperatures in our borehole would have been higher than today for hundreds of millennia. (Humphreys, 2008)

Harrison, Morgan, and Blackwell claimed that the Fenton Hill heating event was distinct from and more recent than the main volcanic eruption of the Valles caldera. Humphreys blurs that distinction. Harrison, Morgan, and Blackwell claimed an upper limit for the duration of this heating event at 40 thousand years. Humphreys calls it hundreds of millennia.

5. *None of Humphreys' critics published their works in peer-reviewed academic journals.*

If there is any merit to this argument, then Humphreys needs to publish a rebuttal to his critics in a non-creationist, peer-reviewed forum. It is hypocritical of Humphreys to deride the works of others for not being peer-reviewed, when he uses a non-peer-reviewed forum himself to criticize them. If he cannot publish his rebuttal in a non-creationist, peer-reviewed forum, then he must address the works of his critics on the basis of their content, not their venue of publication.

Let's consider more closely Humphreys' claim that the RATE helium diffusion research appeared in five technical publications, one of which was non-creationist. Table 1 summarizes these publications.

Table 1
Published works of the RATE helium diffusion project.

Place	Year	Publisher	Forum	Comments
RATE book	2000	Institute for Creation Research and the Creation Research Society	closed	Privately published.
AGU conference	2003(a)	American Geophysical Union	open (non-creationist)	Presented a 350 word abstract with poster.
ICC conference	2003(b)	Creation Science Fellowship	open (young-earth)	Conference is held once every 4-5 years.
CRSQ journal	2004	Creation Research Society	open (young-earth)	Journal is published 4 times a year.
RATE book	2005	Institute for Creation Research and the Creation Research Society	closed	Privately published.

Once again, when Humphreys' claims are examined in detail, they are found to be extremely misleading. One would typically expect that a multi-author effort with more than seventeen reviewers and editors to have been subjected to serious outside scrutiny. The important question to ask is where in the process of publication could serious objections to the RATE helium diffusion work be raised? Two of their publications are privately published books closed to any outside participation. The International Conference on Creationism is an open forum, at least to other young-earth creationists. It is not clear whether any serious criticism of the RATE project would be allowed in their forum. In any event, the last conference was in 2008, meaning that the next opportunity to publish in this forum will be in 2012 at the earliest. That leaves the Creation Research Society's quarterly journal as the most likely forum for publishing any serious criticism of the RATE project.

I tried publishing in the CRSQ over three years ago, to no avail. The editor strung me out for over a year, leaving me the impression that my manuscript would be accepted as long as I agreed to certain changes. However, whenever I submitted a revision for his approval, he would raise new objections to the paper. After over a year of effort it became clear that the CRSQ was not going to publish my paper under any circumstance. Incidentally, the physics section editor at the time was also one of the three main editors for the RATE books, which raises serious conflict of interest concerns. Furthermore, since the CRSQ published one of Humphreys' RATE papers, they have a moral obligation to the scientific community to allow scholarly criticism. They have not permitted one serious challenge to the RATE work in their forum, however.

We are left with the one publication in a truly public forum, the fall meeting of the American Geophysical Union. The extent of the RATE team's disclosure to the conference organizers was a 350 word abstract, carefully written to avoid many of the controversial aspects of their work. Having cleverly passed their abstract through the review process, they proceeded to include material in their conference poster that went well beyond what was promised in the abstract. Although many in the scientific community are understandably indignant over these deceitful tactics, the RATE team achieved their ends of having their work appear in a non-creationist conference, which they now exploit for propaganda purposes.

The insinuation of Humphreys' claim that his work was published in a non-creationist technical forum is that somehow the mainstream scientific world has accepted his work. If this were really the case, then he needs to publish the rest of his research in a non-creationist academic journal as well, and not just a vague 350 word abstract. Here is the difficulty faced by anyone who wants to challenge the RATE research. The academic journals typically will not publish criticism of any work appearing in a forum outside their purview. For instance, the Journal of Chemical Geology will not admit a manuscript criticizing a paper appearing in the Creation Research Society Quarterly. This is not a hypothetical example – I was told so. Since the CRSQ is not considered a legitimate technical journal by the rest of the scientific community, academic journals have no interest or business in engaging with one of their controversies. Furthermore, since the CRSQ will reject submissions by authors who do not subscribe to their religious beliefs, it is difficult, if not impossible, for an outside critic to publish his work there. Where does one publish?

What we are left with, then, is a system of error created by the young-earth creationist community, seventeen reviewers and editors notwithstanding.

Despite the appearance of a legitimate scientific effort, a popular research program can promote claims which shake the very foundation of the laws of physics without the possibility of *even a single objection* being admitted into their public forum. This is not science, but dogma.

3. RATE's young-earth diffusion model

In addition to criticizing the old-earth helium diffusion model, Humphreys raised the following challenge in defense of his young-earth model. The RATE team had published predictions of their accelerated nuclear decay model before the final experimental data was available (Humphreys, 2000; Humphreys *et al.*, 2003b). Once this data was published alongside the model predictions, the agreement was extremely good (Humphreys *et al.*, 2004; Humphreys, 2005). Humphreys asks how, "*if there is no truth to our model, the data happened to fall right on our prediction?*" (Humphreys, 2008) He claims that the sequence of events places the burden of disproof on his critics. However, once the chronology is carefully examined, the evidence shows that enough preliminary data was available for the RATE team to tune their model prior to publishing the results of their final experiment.

First of all, the RATE team published not one but two different young-earth helium diffusion models, the first in 2000 and the second in 2003 (Humphreys, 2000; Humphreys *et al.*, 2003b). Why did they abandon their original model? Both models consisted of a two material system composed of a zircon core surrounded by a biotite shell. Because of a misunderstanding regarding an obscure Russian paper, the RATE researchers originally believed that helium moved faster through zircon than biotite. Accordingly, they simplified their original 2000 model to comprise a solid biotite shell surrounding a hollow core representing the zircon.

What were the predictions of their first model? On page 348 of the first RATE book (Vardiman *et al.*, 2000), the caption to Figure 7 describes it as the "*predictions of yet-future experiments on He diffusion through biotite, using the observed He retention in Jemez zircons*". Results of these future biotite experiments were later published (Humphreys *et al.*, 2003b). However, a comparison between the original model predictions and the biotite diffusivities was never made in the RATE publications. For reference, Fig. 2 makes this comparison.

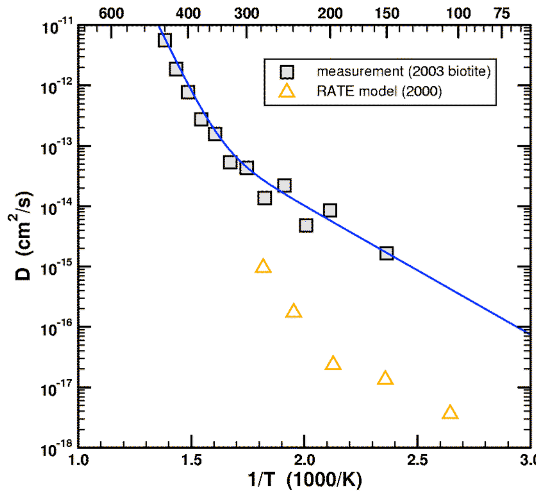

Figure 2. Comparison of the predictions of the 2000 RATE helium diffusion model to measured biotite data published in 2003.

The agreement between the model predictions and measurement was extremely poor, which is probably why this figure was not highly publicized like their later figure with the apparently good agreement. In their later figure (see Fig. 4 below), the model diffusivities were essentially the same, but the mineral zircon was substituted for biotite in the measured data. This substitution of materials invalidated the predictions of the model because it had to be tested against its *original* claims. Therefore, it is misleading for Humphreys to argue that the original 2000 model ever had good agreement to measured data.

By the time the RATE team published their ICC conference paper (Humphreys *et al.*, 2003b), they realized the reason for the failure of their original model. New diffusion data was available which convinced them that helium moved much slower through zircon than they had previously supposed. In light of this new information, their first model was obviously wrong and had to be revised. Given the new diffusion data, an obvious way to correct their original model would have been to turn it "inside-out" by making the zircon core the solid portion and neglecting the surrounding biotite shell. Although never discussed in any of the publications by the RATE team, this alternate model is well supported by their biotite diffusion data and is consistent with their young-earth beliefs.

Surprisingly, they chose a very different model in which both the zircon and biotite had identical diffusion parameters, despite the fact that their own data showed that "*the zircon itself, not the surrounding biotite, was the main restriction to helium outflow*" (Humphreys *et al.*, 2004). Even though the RATE team discussed a more complex two material model in their 2003 paper, they did not fully utilize its capabilities. Instead, they greatly simplified the model by assuming that both material regions had the same diffusion properties (Humphreys *et al.*, 2003b).

The goal of their helium diffusion study was to compare the predictions of a young-earth model to measured data. At the time they published their "new creation" model in 2003, experimental data on zircon was not available at low enough temperatures to make a conclusive comparison. Preliminary data, however, was available at higher temperatures and was published in the same paper. In analyzing these data, the RATE authors mentioned that "*we will need a fit at temperatures below that*" of the experiment and subsequently performed an extrapolation. Although they stressed the importance of having this extrapolation to lower temperatures, they never used the result in the remainder of the paper (Humphreys *et al.*, 2003b).

Figure 3 captures what was known by the RATE team in 2003. It plots the preliminary experimental data for zircon (gray squares), its extrapolation to lower temperatures (solid blue line), the predictions of the new RATE model (orange triangles), and the predictions of my alternate "inside-out" model described above (red stars).

In comparing the predictions of the two models to the extrapolation of the experimental data, it is apparent that the RATE model matches the experimental data better than the alternate model. However, this good agreement by the RATE model cannot be called a true *prediction* because the experimental data was available at the *same time* the model was published. Since other viable models were possible, the RATE model must be justified on the basis of independent physical evidence and not its agreement to the experimental data. Otherwise, it is possible to use unrealistic and artificial means to force even a bad model to match a desired outcome.

It should be pointed out that the RATE team never publicly discussed anything like my alternate model depicted in Figs. 3 and 4. Only they know whether anything like it was considered privately or not. My purpose in bringing it into the discussion is to demonstrate that the published RATE model is not a unique embodiment of their young-earth / accelerated nuclear decay hypothesis. Other reasonable possibilities were certainly available to them within their theoretical framework.

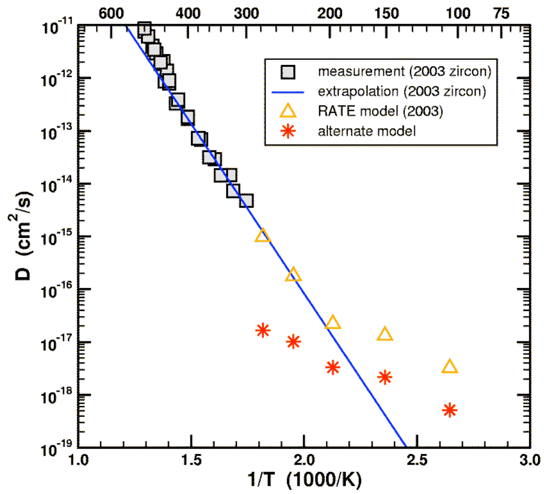

Figure 3. Comparison of the predictions of the 2003 RATE helium diffusion model to measured zircon data published in 2003. The blue line is an extrapolation of the 2003 zircon data. The alternate model was never discussed in the RATE publications, but is an example of other diffusion models consistent with their young-earth / accelerated nuclear decay hypothesis that should have been considered.

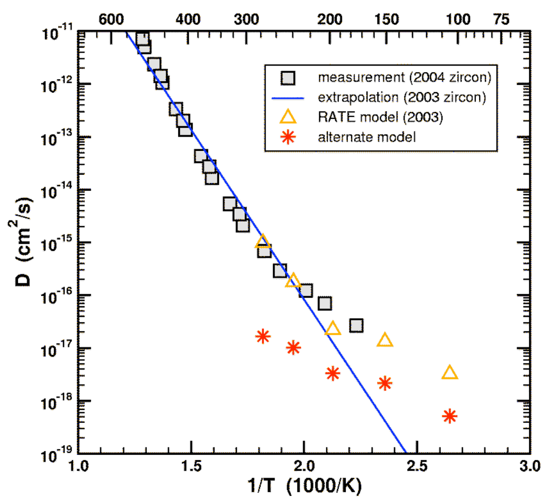

Figure 4. Comparison of the predictions of the 2003 RATE helium diffusion model to measured zircon data published in 2004. The blue line is an extrapolation of the 2003 zircon data. The alternate model was never discussed in the RATE publications, but is an example of other diffusion models consistent with their young-earth / accelerated nuclear decay hypothesis that should have been considered. Note how closely the measured 2004 zircon data follow the extrapolation of the 2003 zircon data.

When a researcher is presented with a range of models fulfilling a given set of criteria, he is responsible for exploring the alternatives, documenting the sensitivities, and rigorously defending his decisions. For a highly tunable model to exactly match a preliminary dataset is suspicious, not impressive, especially when an exploration of the alternatives was not carefully documented. Comments like "*because uniformitarians need to increase the time anyhow, they should not object to this approximation*" fall far short of scientific professionalism (Humphreys *et al.*, 2003b), and statements like "*my calculations might have to be adjusted by a factor of two or so*" are meaningless if they are not backed-up by rigorous analysis (Humphreys, 2008). Figures 3 and 4 clearly show that the difference between models is more than a factor of two, unless Humphreys meant two orders of magnitude in his statement!

The RATE team continued to repeat their experiment in hopes of obtaining data at lower temperatures. In 2004, they published their final result (Humphreys *et al.*, 2004). Figure 4 captures what was known by 2004. It is very similar to Figure 3. – The only difference is that the experimental data for zircon is replaced by a more recent dataset. Even the extrapolated line is from the preliminary 2003 data (Humphreys *et al.*, 2003b).

The apparently remarkable agreement between model and experiment is not remarkable at all once it is realized that the result was anticipated from the extrapolation of the earlier data. If anything, the most impressive result is the reproducibility of the laboratory experiment itself, and not the predictions of the RATE model. The agreement of the RATE model is even less impressive when one considers that the model was artificially tuned to match a desired result, and that they never published the results from any of their intermediate experiments.

4. A test between the two models

Not only does the foregoing discussion answer the concerns raised by Humphreys regarding my old-earth helium diffusion model, it also calls into question the significance of the predictions made by his young-earth RATE model. Nevertheless, when similar claims are made by two competing models, the best solution is to formulate a new test using additional data. If the RATE model is based upon solid physical principles, then its predictions should match new data fairly well. If, on the other hand, the model was contrived to fit a preliminary dataset, then its predictions will not be reliable.

Humphreys already proposed a suitable test. In the CRSQ paper where the RATE team published the results of their final diffusion experiment, they ignored the initial heating ramp in their analysis. They justified this omission of data on the grounds that the amount of gas released during the initial steps of the experiment was affected by the surface helium profile within the crystal. In this context Humphreys suggested that "*a more sophisticated analysis could probably extract accurate values of D from the raw helium-time data for those steps, but we leave that work for later research*" (Humphreys *et al.*, 2004). His diffusion modeling approach lacked the sophistication to tackle this problem. My technique is more general and powerful enough to do the job. (Please see the appendices of my 2008 technical paper for details.)

Since the gas released during the initial heating steps is sensitive to the surface profile, it is worthwhile comparing the helium profiles of the two models. (For a discussion of surface profile effects, see Fechtig and Kalblitzer, 1966, p. 71; and Farley, 2000, p. 2908.) In comparing the profiles (Fig. 5), the young-earth RATE model (blue line) has a significant non-zero concentration at the surface (30 μm), whereas the old-earth model (orange line) has a near zero concentration at the surface (20 μm). The higher surface concentration in the RATE model is due to their boundary condition. They treated the zircon / biotite interface as if biotite has the same diffusivity as zircon, contrary to their own data. The old-earth model assumed a zero concentration boundary condition, essentially recognizing the fact that helium diffuses much faster through biotite than zircon.

Since the two models predict substantially different surface profiles, the effect should be noticeable during the initial heating ramp of the diffusion experiment, which is highly sensitive to the surface profile. Because of its lower surface concentration, the old-earth model will predict a lower diffusivity in the initial ramp. The diffusivity predicted by the young-earth RATE model will be closer to the values obtained from the later heating steps of the experiment. Figure 6 compares the predictions of these two models with the measured data.

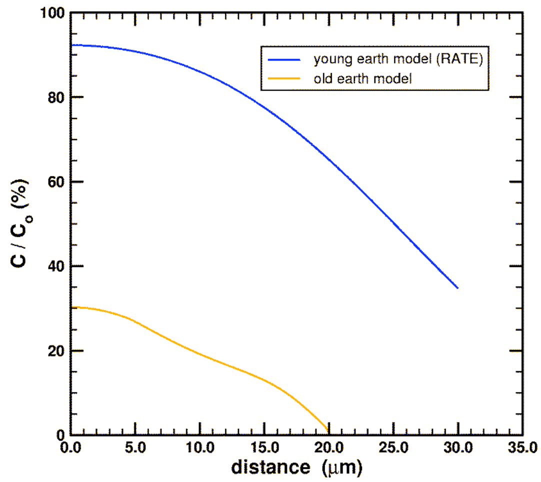

Figure 5. Comparison of the helium profiles for the RATE young-earth diffusion model and my old-earth diffusion model. The RATE model predicts a significant nonzero surface helium concentration whereas my model predicts a helium concentration that approaches zero at the surface.

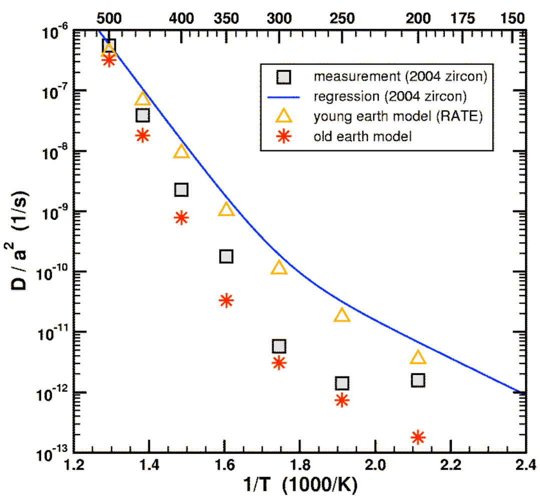

Figure 6. Comparison of the RATE young-earth model and my old-earth model to diffusivities extracted from the initial heating ramp of a stepwise heating experiment. The measured zircon data were published in 2004. The blue regression curve is a fit to the remaining data of the diffusion experiment after the initial temperature ramp.

In order to clarify the figure, only the data points for the initial heating ramp are plotted. A regression curve, however, was fit to the remainder of the measured data. This curve is also plotted on the figure for reference. All of the remaining measured data lie close to this regression curve. As can be seen in Fig. 6, the initial data points from the measurement lie substantially below the regression curve. The old-earth diffusion model accurately captures this trend, and many of its predicted points lie close to the measured data. In contrast, the young-earth RATE diffusion model predicts that the initial ramp should lie very close to the regression curve. This behavior is by-and-large inconsistent with the measurement. The burden now lies with Humphreys to explain why his RATE model deviates from measurement, at times by over an order-of-magnitude, in the initial heating ramp of the diffusion experiment. Overall, the old-earth diffusion model matches the measured data better, especially at the lower temperatures.

5. Final remarks

Humphreys, in his section on "*help for non-experts in deciding*", made the comment that "*another simple point is the number of critics and the long time they've been criticizing. Each one was unsatisfied enough with the previous criticisms (most are familiar enough with the others to borrow their arguments occasionally) to take the time to attack the helium data on their own*" (Humphreys, 2008). I do not think that any of these critics could have said anything more damaging to the RATE case and the young-earth cause in general than this one statement by Humphreys, because it reveals the mindset of a propagandist and not a true scientist.

The very thing that Humphreys criticizes, multiple people who are unrelated and unknown to each other bringing their different and sometimes conflicting ideas to the table, debating and even disagreeing with one another, is how true science works! Scientific consensus is not arrived at by decree, but through a process in which every idea, both good and bad, is tested and tried until the best possible explanation remains. There is no scientific body to give an "official" response to the RATE claims. Rather, it is concerned scientists, working alone without any supporting organization or budget, who are seeking together to find the truth. It is only after ideas are shared and discussed, and sufficient information comes to light, that a true consensus can emerge. This is an example of the scientific process.

The RATE study was not conducted according to these established scientific principles. The group of RATE scientists, many of whom hold a controlling influence on the venues of publication within the young-earth community, collaborated together to propose their official solution to the "radiometric dating problem". Their consensus was not arrived at through public debate but in private agreement. Dissenting opinions were discouraged, and serious challenges to their work were censured, as I can personally attest. If the multitude of external critics is evidence to Humphreys that he is right, then the lack of serious open debate within the young-earth community persuades me to believe that he is wrong.

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