Dr. Humphreys' Young-Earth Helium Diffusion "Dates"

Numerous Fallacies Based on Bad Assumptions and Questionable Data

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[Original version: March 17, 2005]
[Revisions: November 24, 2005; July 25, 2006 and June 20, 2010]

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LITERATURE UPDATES SINCE 2005

Dr. Humphreys has not silenced his critics, we are waiting for him to answer our numerous questions.

Talkorigins permanently archived the original version of this essay after the first update on November 24, 2005 contrary to erroneous statements in footnote #25 of Humphreys (2008b). I further revised my essay on November 24, 2005 to reply to Humphreys (2005a) and again on July 25, 2006 in response to Humphreys (2006). In the current June, 2010, version, I extensively updated and reorganized the essay to: 1) include materials from other critics of Dr. Humphreys' work, 2) address criticisms from additional peer-reviewers of this essay, 3) respond to Humphreys (2008a), Humphreys (2008b), Humphreys (2010) and statements from Dr. Humphreys' allies at the CreationWiki webpage, and 4) discuss new revelations on how Dr. Humphreys unethically manipulated results in Magomedov (1970) to protect his YEC agenda.

Dr. Humphreys' growing number of critics includes physicists, engineers, and geologists. Humphreys (2008b) even admits that his critics not only include secular scientists, but a diverse group of young- and old-Earth creationists, including members of the American

Scientific Affiliation (ASA). In particular, Humphreys (2008a) criticizes the religious diversity of ASA and briefly responds to criticism of his helium diffusion study from Isaac (2007, 2008a, 2008b). However, the most extensive and devastating recent criticisms of Dr. Humphreys' claims originate from old-Earth creationist and materials engineer Dr. Gary H. Loechelt. Dr. Loechelt applied multi-domain diffusion models to Dr Humphreys' and R. V. Gentry's data, which raise many new arguments that further undermine Dr. Humphreys' young-Earth creationist (YEC) claims. Loechelt (2008a; 2008b), which are at the old-Earth creationist Reasons to Believe website, are brief and less technical summaries of Loechelt (2008c). Loechelt (2008c) is a detailed report that argues that Dr. Humphreys' claims and his underlying assumptions are oversimplistic, inconsistent and erroneous, and that Dr. Humphreys' helium diffusion data are actually consistent with a date of about 1.5 billion years for the Fenton Hill zircons. Although Humphreys (2008b) and Humphreys (2010) briefly mention Loechelt (2008a; 2008b; 2008c), Dr. Humphreys provides no detailed responses to Dr. Loechelt's models and his numerous criticisms. Loechelt (2009a) is a detailed rebuttal of Humphreys (2008b) and Loechelt (2009b) is a less technical summary of his response to Humphreys (2008b). Most recently, Humphreys (2010) is a brief letter, where Dr. Humphreys largely recycles the materials in Humphreys (2008b) and prematurely declares "victory" without appropriately answering the numerous questions from his critics (*e.g.*, my Appendix C).

Figure A. The old Earth multi-domain model from Loechelt (2008c) better explains helium diffusion in the Fenton Hill zircons than Dr. Humphreys' young Earth RATE model. The sample numbers are from Gentry *et al.* (1982a) and Humphreys *et al.* (2004). Both Dr. Loechelt and I (my Appendix A) demonstrate that Dr. Humphreys' Q_0 value is too small. More realistic Q/Q_0 values from Loechelt (2008c) are based on data from Zartman (1979) and utilize the alpha-correction procedure in Meesters and Dunai (2002b). As shown in the calculations in my Appendix B of this essay, data from Gentry *et al.* (1982b) provide better ranges of Q/Q_0 values for samples 1, 5 and 6 than the values used by Gentry *et al.* (1982a) and Dr. Humphreys. For sample 3, my range of Q/Q_0 values was derived from data in Zartman (1979). Zartman (1979) analyzed a zircon taken a few meters from sample 3 and probably from the same granodiorite. All of the models assume that none of their Q/Q_0 values were elevated by contamination from extraneous helium. This graph was modified from Loechelt (2008c) with permission.

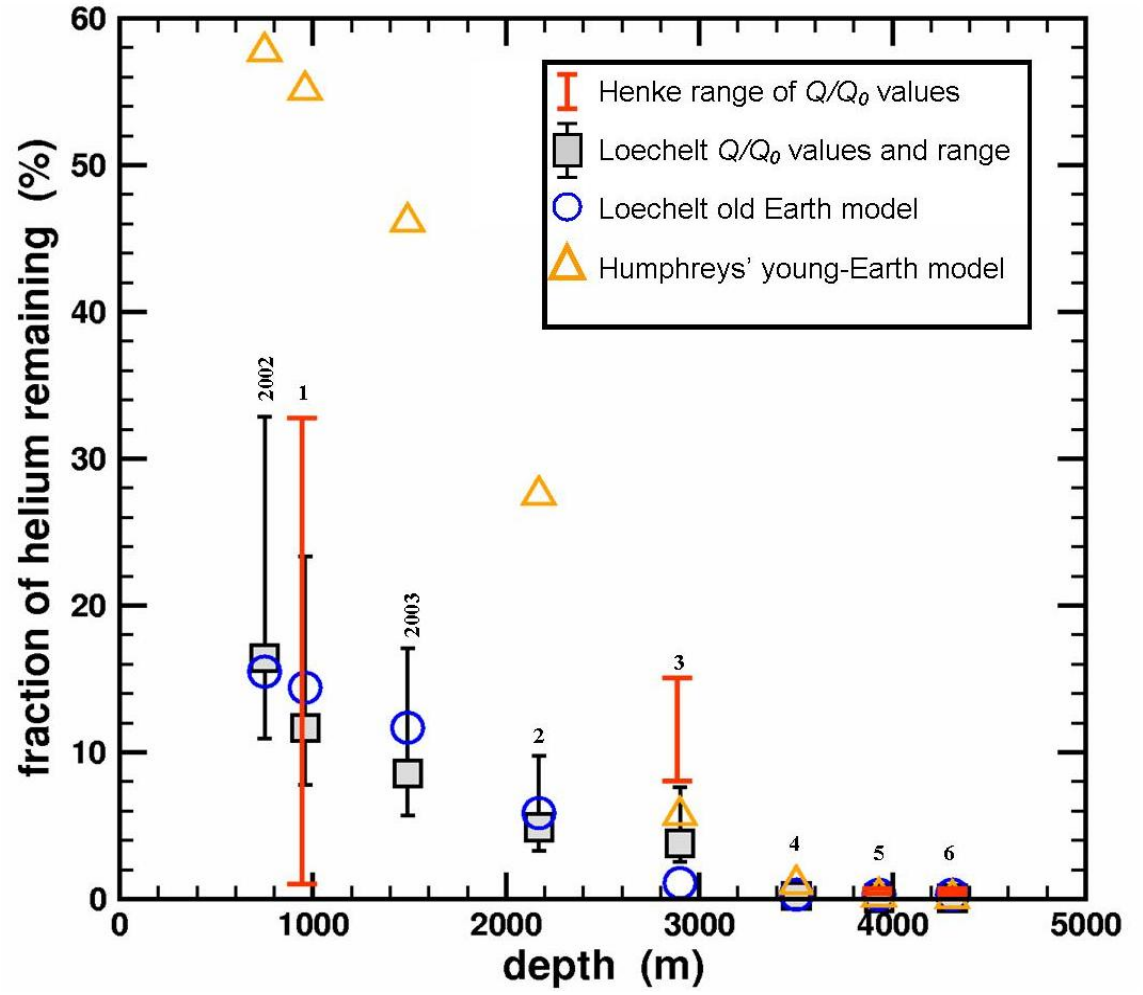
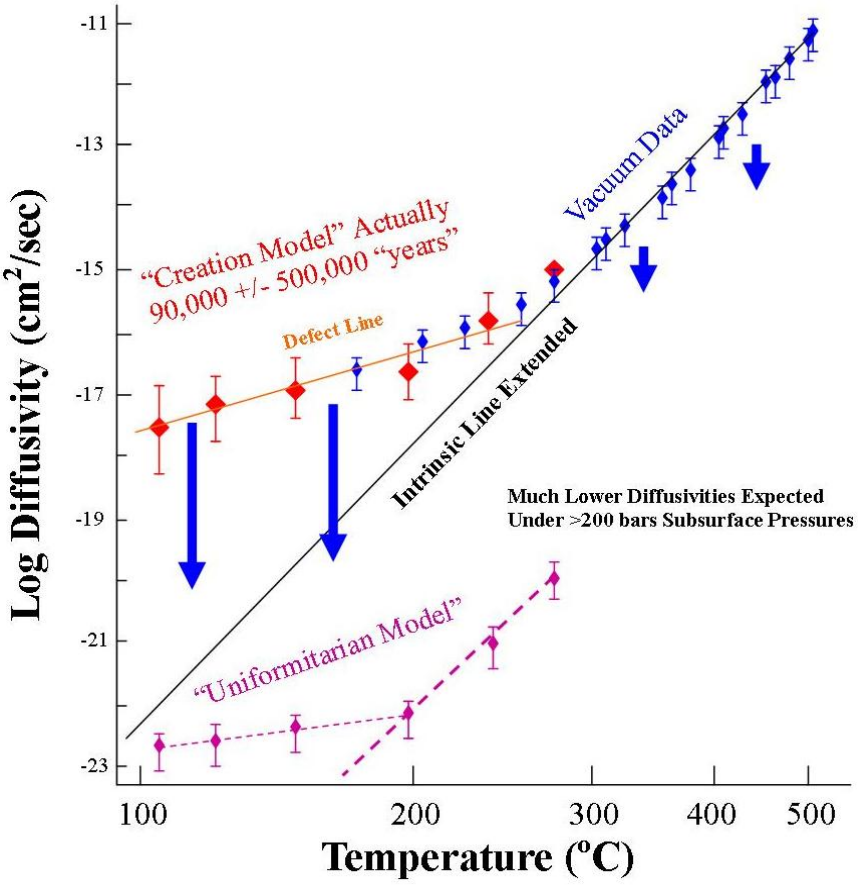


Figure B. Another hypothesis to explain the helium diffusion data. Subsurface pressures on Dr. Humphreys' and R. V. Gentry's zircons in the Fenton Hill cores would have been about 200 to 1,200 bars. If the defect curve in Dr. Humphreys' diffusion studies resulted from voids, fractures and other openings in the zircons, then some of these openings could have been at least partially closed under subsurface pressures. If the openings were substantially closed, the defect curve of Dr. Humphreys' zircons, which is used to support his young-Earth creation model, would have been lower, aligned more with the intrinsic curve, and perhaps even approached the diffusion results for Dr. Humphreys' strawperson uniformitarian model. The intrinsic curve would be less affected by pressure. Humphreys (2006) argues that zircon is too "hard" to have its helium diffusion affected by subsurface pressures. However, laboratory studies in Dunai and Roselieb (1996) show that under 250 bars of pressure and at temperatures as high as 700°C, helium would take **tens to hundreds of millions of years** to just partially diffuse out of garnet, a "hard" silicate mineral like zircon. Dr. Humphreys has the responsibility to evaluate any pressure effects on his "dating" scheme before he can proclaim that the Earth is only 6,000 years old, "accelerated" radioactive decay is factual and that all radiometric dating methods must be discarded. Even without pressure effects, the best available *a*, *b*, and Q/Q_0 data show that the "creation dates" from the equations in Humphreys *et al.* (2003a) provide ridiculous answers that range from hundreds to over one million years (an average and two standard deviations of 90,000 ± 500,000 years old, using only one significant digit; see below for details).



BACKGROUND

Dr. D. R. Humphreys and other young-Earth creationists (YECs) believe that zircons from the Fenton Hill rock cores, New Mexico, USA, contain too much radiogenic helium to be billions of years old (Humphreys *et al.*, 2003a; Humphreys *et al.*, 2003b; Humphreys *et al.*, 2004; Humphreys, 2003). In my original essay, I extensively criticized and documented some of the numerous problems in Dr. Humphreys' work. Rather than dealing with most of his mistakes, it's obvious from Humphreys (2005a) and Humphreys (2006) that Dr. Humphreys did not even read and comprehend the vast majority of my criticisms. Dr. Humphreys also fails to properly deal with many problems and questions raised by other critics, especially Loechelt (2008c; 2009a). This essay contains additional evidence and discussions that demonstrate that Dr. Humphreys' work is fatally flawed and never achieves its YEC objectives.

Throughout Humphreys (2005a) and as recently as Humphreys (2008b), Dr. Humphreys stresses that his YEC conclusions must be correct because Figure 2 in Humphreys (2005a) shows a supposedly strong correlation between his creation model and helium diffusion measurements from Humphreys *et al.* (2003a; 2004). Dr. Humphreys is so mesmerized by his Figure 2 in Humphreys (2005a) that he is in denial and will not deal with the serious errors and numerous questions about this figure and his work. Although Humphreys (2008b) accuses his critics (including me) of supposedly ignoring his diagram, I have long-argued that Dr. Humphreys' diagram has little scientific merit (for example, see Figure B and also discussions in Loechelt, 2008c). Any effort to nullify the entire field of geochronology and promote radical changes in our fundamental understanding of nuclear physics would require far more than a single pretty diagram produced from incomplete data, invalid assumptions and numerous faulty interpretations. Materials engineer Dr. Gary Loechelt in Loechelt (2008c; 2009a) also argues that since the beginning of the project, Dr. Humphreys and his colleagues have "tuned" their creation model and its assumptions so that the "consistency" between the creation model and the helium diffusion data is not the decisive result that Humphreys (2005a) and Humphreys (2008b) want us to believe.

The "dating" equations in Humphreys *et al.* (2003a) are clearly based on many questionable assumptions (including: isotropic helium diffusion in minerals, constant subsurface temperatures over time, ignoring the possibility of extraneous helium, etc.). The vast majority of Humphreys *et al.*'s critical a, b, and Q/Q_0 values that are used in these "dating" equations are either missing, poorly defined, improperly measured or inaccurate. Using the best available chemical data on the Fenton Hill zircons from Gentry *et al.* (1982b) and Zartman (1979), the equations in Humphreys *et al.* (2003a) provide ridiculous "dates" that range from hundreds to millions of years old (average: 90,000 ± 500,000 years old [one significant digit and two unbiased standard deviations] and not 6,000 ± 2,000 years as claim by Humphreys *et al.*, 2004). There are also serious ethical questions about how Dr. Humphreys handled data from Magomedov (1970) and other documents. Contrary to Humphreys (2005a), his mistakes are not petty or peripheral, but completely discredit the reliability of his work. To correct his mistakes, Dr. Humphreys needs to perform spot analyses for ^{3}He, ^{4}He, lead, thorium and uranium

on numerous zircons from all of his and R. V. Gentry's samples so that realistic Q/Q_0 values may be obtained. Finally, Loechelt (2008a; 2008b; 2008c; 2009a; 2009b) shows that multi-domain helium diffusion models, which are far more realistic than the "creationist" and "uniformitarian" models presented by Humphreys *et al.* (2003a), are actually consistent with a date of about 1.5 billion years for the Fenton Hill zircons.

As indicated in the Acknowledgments, my essay has been peer-reviewed. Rather than recognizing my peer-reviewers, many of which are scientists, Humphreys (2005a) repeatedly challenges me to publish my criticisms of his work in a peer-reviewed scientific journal. However, it's obvious from Dr. Humphreys' publication record on this topic (*e.g.*, Humphreys *et al.*, 2003a; 2003b; Humphreys, 2003; Humphreys *et al.*, 2004, etc.) that he has no real interest in fully presenting his ideas for critical scrutiny from some of the world's authorities on zircon and helium chemistry. So, before Dr. Humphreys screams about the importance of peer-review, he needs to follow his own advice. He needs to openly publish his work and conclusions as a **full article in a legitimate peer-reviewed science journal** (such as *Geochimica et Cosmochimica Acta* or *American Mineralogist*). Suitably peer-reviewed documents don't include a brief abstract in *EOS* (Humphreys *et al.*, 2003b: where any controversies could be minimized or entirely avoided in the abstract and then presented unpeer-reviewed in the poster session, see Loechelt, 2009a), YEC proselytizing materials edited by his friends and/or fellow RATE members (*e.g.*, Humphreys, 2003), and the *Creation Research Society Quarterly* (*CRSQ*), whose "peer-review" system and scientific quality have even been discredited by YECs (Whitmore *et al*., 2007). If Dr. Humphreys is really sincere about his devotion to peer-review, let him wean himself off the reliance on miracles for his "accelerated" radioactive decay claims, honestly recognize and correct his numerous mistakes, thoroughly answer the numerous questions from his critics, and submit what's left as a detailed article in a real science journal, where he doesn't have friends that will rubber stamp his work.

Dr. Humphreys in Humphreys (2005a) and Humphreys (2006) thinks that he can just read through the abstract of my original essay or other brief snippets of my work, throw out some insults, try to trivialize his serious mistakes, make bold assertions without any calculations to support them, make a couple of minor corrections here and there, misrepresent critical details in the literature, invoke several irrelevant analogies (*e.g.*, lead self diffusion in Humphreys, 2006), ignore the details, promise better answers in the future (*e.g.*, Humphreys, 2005a), repeatedly rely on his deceptive figure (*i.e.*, Figure 2 in Humphreys, (2005a), and then hope that his readers will just go away on faith. Now, some individuals might accept this type of arm waving, the invoking of "God did it!", and the brushing off of serious criticisms, but real scientists and editors of scientific journals would not. Science doesn't work this way and Dr. Humphreys should know better. Dr. Humphreys has had more than five years to make a thorough and air-tight case for his claims and produce the detail calculations that he promised in Humphreys (2005a). As explained in this and my earlier essays, he has wasted a lot of time and money, made a lot of empty claims and promises, and has utterly failed to support his YEC agenda. As further seen in Humphreys (2008b) and Humphreys (2010), Dr. Humphreys continues to ignore the numerous questions and problems with his work. He simply repeatedly points

to his figure with its contrived and/or coincidental distributions, embraces dogma rather than science, and proclaims that because some diffusion data happen to line up with his creation model that his model must be infallible "proof" for a 6,000 year old Earth. Dr. Humphreys needs to overcome his denial and answer the questions, defend the details of his claims, and fully admit and correct his mistakes. To illustrate the long list of serious flaws in Dr. Humphreys' work, I have summarized some of the problems through a series of questions in Appendix C of this essay

OUTLINE

Introduction

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Mysterious Modifications of the Helium (Q) Measurements in Gentry *et al.* (1982a): More Questions than Answers

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INTRODUCTION

For decades, young-Earth creationists (YECs) have desperately sought "scientific evidence" to attack radiometric dating and protect their religious views of Earth history. Although YECs claim to believe that the Bible is the "powerful word of God", they fully realize that just quoting their scriptures is not going to convince geochronologists and other scientists to abandon their research and stream to church altars in repentance. Therefore, a small group of YEC PhDs associated with the Institute for Creation Research (ICR), the Creation Research Society (CRS) and Answers in Genesis (AiG) formed the **RATE** (**R**adioisotopes and the **A**ge of **T**he **E**arth) committee (Vardiman *et al.*, 2000; Humphreys *et al*., 2004, p. 3). Simply put, their activities included combing the scientific literature and designing laboratory "experiments" that would somehow verify what they have already concluded, namely that a "literal" interpretation of Genesis is "The Truth" and **anything** that conflicts with their biblical interpretations is "wrong." As AiG personnel dogmatically admit in Section 4, #6 of their Statement of Faith:

> "By definition, no apparent, perceived or claimed evidence in any field, including history and chronology, can be valid if it contradicts the scriptural record."

In 2003, many Christian fundamentalists became very excited about a RATE project described in Humphreys *et al.* (2003a), Humphreys *et al.* (2003b) and Humphreys (2003). Humphreys *et al.* (2003a) claim that zircons from the "Jemez granodiorite" of the Fenton Hill rock core, New Mexico, USA, contain too much radiogenic helium to be billions of years old. By inaccurately modeling the helium diffusion rates in the zircons, making numerous invalid assumptions and assuming some unfounded miraculous increases in radioactive decay rates, Humphreys *et al*. (2004) concluded that the zircons are only "6,000 ± 2,000 years old." Not surprisingly, their results conveniently straddle Bishop Ussher's classical 4004 BC "Genesis creation date" for the world. Loechelt (2008c; 2009a) argues that this is no coincidence.

Since 2005, a number of PhD physicists and other scientists (including at least one young-Earth and several old-Earth creationists) have criticized the validity of Humphreys *et al.*'s claims (*e.g.*, Loechelt, 2008a; 2008b; 2008c; 2009a; 2009b); Whitefield, 2008; Isaac, 2007; 2008a; 2008b; Christman, 2005). Dr. Humphreys' responses to his critics (such as Humphreys, 2005a; Humphreys, 2006; Humphreys, 2008a; Humphreys, 2008b; Humphreys, 2010) have been superficial and have totally lacked suitable mathematical and technical details to defend his procedures and YEC conclusions. Most recently, Humphreys (2010) continues to dodge the critical questions that I list in my Appendix C and that other scientists have raised. In his effort to dismiss the extensive criticism of his work from a number of very qualified physics, materials engineering, and geology PhD's, Humphreys (2008b) even suggests that his opponents are disorganized and disagreeing with each other:

> "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."

Even if Dr. Humphreys' critics disagree, does that necessarily make Dr. Humphreys right? For example, if Dr. Humphreys says 2 + 2 = 5, and one critic says "no, it's four" and another says "no, it's six," does that mean that Dr. Humphreys is automatically right because his critics can't agree? Nevertheless, a review of the available documents from Dr. Humphreys' critics shows a lot of agreement among us. For example, Whitefield (2008), Loechelt (2008c) and I all agree that Dr. Humphreys' Q/Q_0 values are inflated. Many of us have also protested against Dr. Humphreys' mysterious changes in the Q values from Gentry *et al.* (1982a) and his inability to justify his Q_0 value of only 15 ncc STP/µg despite the promise in Humphreys (2005a) to present his math "soon" in a *Creation Research Society Quarterly* (*CRSQ*) article. Had Dr. Humphreys been more open and honest with his math, adequately answered our questions about his work back in 2005, and not been so flippant and evasive with the numerous problems with his work, the list of his critics might have been much shorter. Dr. Humphreys is also simply failing to realize that his critics have very diverse areas of expertise in physics, materials engineering and geology and that each one had no difficulty discovering new errors and questionable claims in his work. Dr. Humphreys' work is finally undergoing a thorough scientific peer-review that it never received from the editors of the Creation Research Society (CRS) (Humphreys *et al.,* 2004) or when he submitted his brief and vague abstract (Humphreys *et al.*, 2003b) to the secular American Geophysical Union. Unfortunately for him, Dr. Humphreys' critics have shown overwhelming evidence that his study is flawed and useless, and perhaps even contrived to unfairly promote his creation model (Loechelt 2008c; 2009a).

The vast majority of the errors and unsound assumptions in the Humphreys *et al.* documents are not the "mountain of minutiae" as claimed by Humphreys (2005a), but serious mistakes that completely invalidate any confidence in his work and claims. Some of the major flaws in the Humphreys *et al.* documents are discussed below, including additional errors recently discovered by Loechelt (2008a; 2008b; 2008c; 2009a), Whitefield (2008), Isaac (2007; 2008a; 2008b) and others. In particular, Loechelt (2008c) corrects many of the equations and parameters in Dr. Humphreys' documents. He further demonstrates that Dr. Humphreys' data actually support an age of about 1.5 billion years for the Fenton Hill zircons, which refute Dr. Humphreys' claims for a "young" (6,000 years old) Earth and his need for "accelerated" radioactive decay.

Rather than engaging in responsible science, Humphreys (2005a), Humphreys, (2006), Humphreys (2008a), Humphreys (2008b), and Humphreys (2010) are rash and superficial notes that fail to provide the required evidence to defend the numerous assumptions in his creation model. As discussed below, using his equations and data, Dr. Humphreys' creation model actually provides a ridiculous "creation date" of 90,000 ± 500,000 years (two unbiased standard deviations) instead of 6,000 +/- 2,000 years (one biased standard deviation). Instead of relying on evasion and ridicule, Dr. Humphreys needs to take some time to actually think about the numerous problems in his work. To begin with, the "dating" equations in Humphreys *et al.* (2003a) are based on many blatantly false

assumptions (isotropic diffusion, constant temperatures over time, etc.) that cannot be dismissed with any claims of "generosity" to the "uniformitarians." As Loechelt (2008c, p. 8) keenly points out:

> "The RATE radiohalo theory proposes the following mechanism for the formation of polonium radiohalos. Radon gas escapes uranium bearing minerals, such as zircon, which are embedded in biotite crystals, and migrates to accumulation sites where it decays into polonium, thereby forming a radiohalo. This theory requires that the heaviest of all noble gases, radon, have the ability to leave its host mineral and travel scores of microns between biotite plates, all within the time constraint determined by the 3.8235 day half-life of 222Rn. On the other hand, the helium diffusion theory requires that this same biotite trap helium, the lightest of all noble gases, and hold it for thousands of years. Clearly, the RATE researchers were focused on two isolated phenomena (helium diffusion and radiohalos) rather than solving a more general problem, like noble gas migration in biotite. Ironically, the helium diffusion study and the polonium radiohalo study are published as consecutive chapters in the same [2005 RATE] book... [references from Vardiman *et al.*, 2005 omitted]."

The vast majority of Dr. Humphreys' critical a, b, and Q/Q_0 values that are used in his "dating" equations are either missing, poorly defined, improperly measured or inaccurate. For example, Dr. Humphreys should stop picking and choosing from the obviously questionable data in Gentry *et al.* (1982a) and instead take several months to redo the analyses. He must further realize that the uranium and thorium data in Gentry *et al.* (1982b) indicate that his Q_0 is far too low and that his Q/Q_0 values are probably inflated by at least an order of magnitude, which by themselves invalidate his YEC agenda (my Appendix B). Rather than ignoring the problems or relying on invalid assumptions about the concentrations of ^{3}He, ^{4}He, uranium and thorium in his zircons, Dr. Humphreys actually needs to perform some detailed analyses similar to those in Gentry *et al.* (1982b) and high pressure studies that are similar to those in Dunai and Roselieb (1996). Through his small and invalid study, Dr. Humphreys and his allies have made the rash claim that he has overthrown the physics of radioactive decay and radiometric dating. Well, before Dr. Humphreys can make such a radical claim, he needs evidence. Extraordinary claims demand extensive and high quality data, which Dr. Humphreys doesn't have.

MISIDENTIFICATION OF FENTON HILL GNEISSES AND THE SERIOUS CONSEQUENCES FOR DR. HUMPHREYS

Gneisses are not "Granites", "Granodiorites" or other Igneous Rocks

When performing research, scientists must carefully follow all quality control/quality assurance (QC/QA) procedures. Essential QC/QA procedures include properly collecting, identifying, labeling, storing and monitoring all samples. If the collection site

of a specimen is unknown or if it has been improperly stored for several decades, any resulting data are often useless.

Unfortunately for them, Dr. Humphreys and his colleagues have failed to comply with the most fundamental QC/QA requirements. Throughout their paper, Humphreys *et al.* (2003a) claim to have studied biotites and zircons from samples of the "Jemez granodiorite" collected at a depth of 750 meters from the Fenton Hill borehole site. More recently, Humphreys *et al.* (2004, p. 5) and Humphreys (2005b) continue to refer to their "granodiorite" samples from depths of 750 and 1,490 meters. Granodiorites are igneous rocks that crystallize from melts (magmas) deep in the subsurface. As their name implies, they have intermediate chemical compositions between granites and diorites, which means that granodiorites tend to have more silica than diorites and more magnesium and iron than granites (Hyndman, 1985, p. 46).

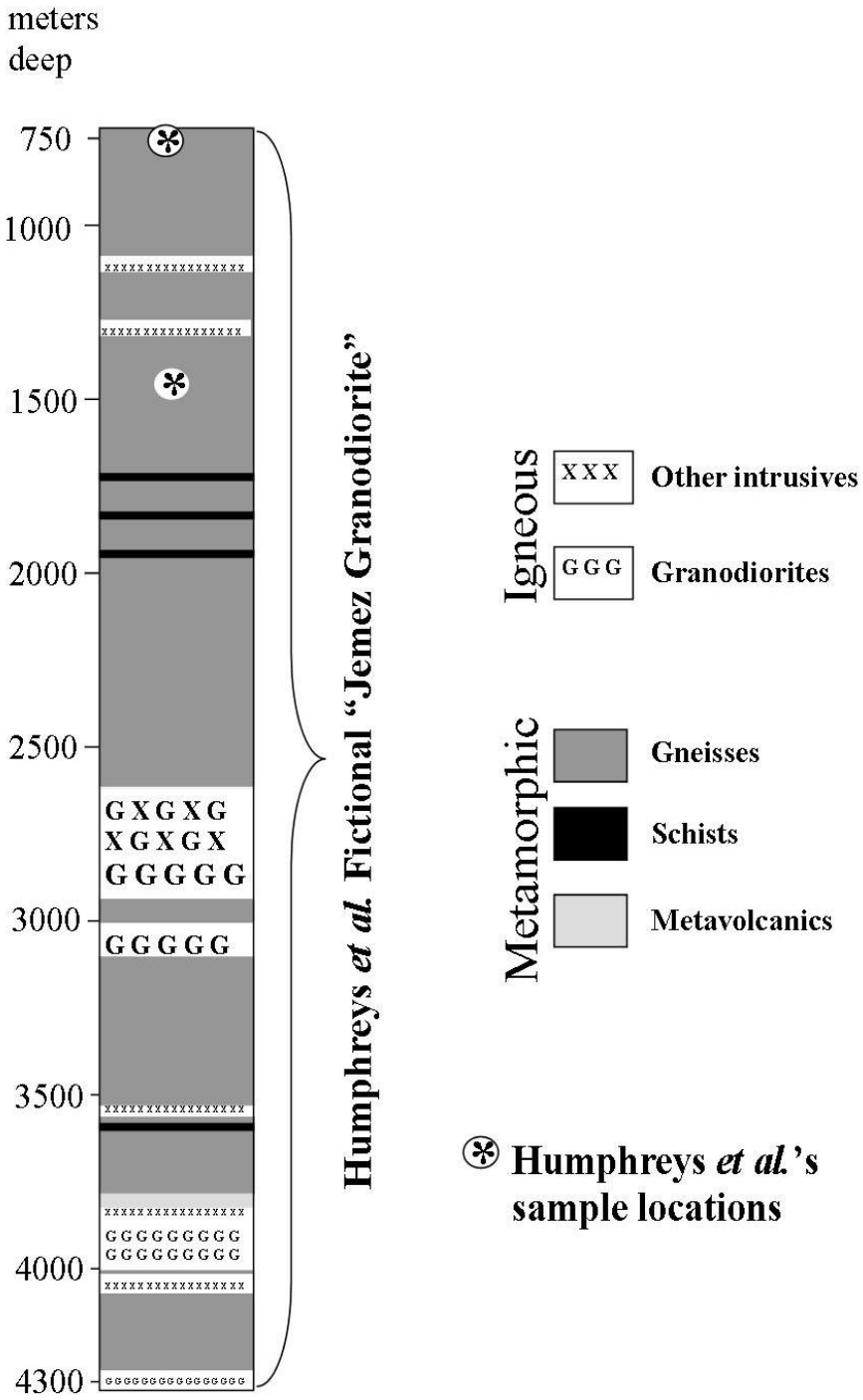
A review of the scientific literature on the subsurface geology of the Fenton Hill borehole site indicates that about 75% of the GT-2 and EE-1 cores consist of gneisses (Laughlin, 1981, p. 308; Laney *et al.*, 1981, p. 2) and that granodiorite is not encountered in the cores until depths of 2591 meters (my Figure 1) (Laney *et al.*, 1981, p. 1; Laughlin *et al.*, 1983; Burruss and Hollister, 1979; Sasada, 1989, Figure 2, p. 258). Information in Laughlin *et al.* (1983) and other references clearly indicate that Humphreys *et al.*'s 750 and 1,490-meter samples are gneisses (Figure 1). Gneisses are former igneous or sedimentary rocks that have been metamorphosed under relatively high temperature and pressure conditions (highly altered), but without melting (Hyndman, 1985, p. 442; Chernicoff *et al.*, 2002, p. 128).

Even after being presented with evidence from the literature, Humphreys (2005a) still refused to admit that he and his colleagues misidentified gneisses as "granodiorites." He continued to insist that most of the Precambrian sections of the Fenton Hill cores are "granodiorites." In contrast, YEC R. V. Gentry readily admitted in Gentry *et al.* (1982a) that the Fenton Hill cores consist of a large number of different rock types, including **gneisses** and other rocks that provided his zircon samples.

Humphreys *et al.*'s misidentification of the rock types in the Fenton Hill cores is not a trivial issue as Humphreys (2005a) claims. When Humphreys *et al.* (2003a, p. 6) were under the delusion that the Precambrian rocks of the Fenton Hill cores largely consisted of only one rock unit (the "Jemez granodiorite"), they openly admitted that any mixing of experimental results from different rock types would be inappropriate for their modeling efforts:

> "Measurements of noble gas diffusion in a given type of naturally occurring mineral often show significant differences from site to site, caused by variations in composition. For that reason it is **important** to get helium diffusion data on zircon and biotite from the **same** rock unit (the Jemez Granodiorite [*sic*]) which was the source of Gentry's samples." [my emphasis]

Figure 1. Geology of the Fenton Hill GT-2 and EE-2 cores based on information in Laughlin *et al.* (1983, p. 25, 26) and Sasada (1989, p. 258). The zircons and biotites utilized in Humphreys *et al.* (2003a; 2004) are from gneisses and not granodiorites.



Of course, the sizes of zircons and biotites can be highly variable depending on the host igneous or metamorphic rock. Because Humphreys *et al.* didn't realize which rocks they sampled and how the sizes of the minerals could vary, serious errors could easily be introduced into the *a* and *b* values that are used in their "dating" equations (13-14 and 16 in Humphreys *et al.*, 2003a) (see further discussions below). Chemical data in Gentry *et al.* (1982b) and Zartman (1979) also suggest that the zircons from the different rocks of the Fenton Hill cores have highly variable concentrations of uranium and thorium, which would mean that Dr. Humphreys cannot accurately represent the zircons from various depths in the cores with just one Q_0 value (see discussions below).

Rather than relying on information in Laughlin *et al.* (1983), Laney *et al.* (1981), and other detailed studies from the literature, Humphreys (2005a) responded to my criticisms by referring to the naked-eye observations of their samples by YEC and coauthor Dr. John Baumgardner. The following statements by Dr. Baumgardner are quoted in Humphreys (2005a):

> "Yes, there are occasional veins of material other than the coarse-grained granodiorite that forms the vast majority [*sic*] of the core. In making the selections I made of what samples to use, I purposely avoided these occasional veins. In fact I tried to select sections of the core well removed from such veins. So at least from my vantage point, the samples of core we used for the helium diffusion measurements were indeed coarse-grained granodiorite, not gneiss."

Dr. Baumgardner's statement that a "coarse-grained granodiorite" forms "the vast majority of the core" blatantly contradicts statements in Laughlin (1981, p. 308) and analytical data in Laughlin *et al.* (1983), which state that approximately 75% of the cores consist of gneisses (not granodiorite). The dominance of gneisses in the Precambrian rocks of the Fenton Hill cores is also obvious from my Figure 1. Because Dr. Baumgardner's conclusions are inconsistent with the results of professional geologists that have examined and analyzed the cores in great detail, I emailed him with a list of questions about the samples that he had collected for Humphreys *et al.* In his kind reply, Dr. Baumgardner described the core as consisting of dark **gneissic** "veins" surrounded by an "unaltered granodiorite" consisting of "large (typically, 2-3 mm)" pinkish grains. Although I requested any mineralogical (such as petrographic or X-ray diffraction analyses) or chemical data (that is, major oxides, minor and trace element analyses) that Dr. Baumgardner might have to support his claims, he provided none.

By definition (Hyndman, 1985, p. 442), gneisses consist of alternating dark- and light-colored bands and not "veins." If "dark gneiss veins" [*sic*, bands] were present in Humphreys *et al.*'s samples as Dr. Baumgardner claims, where are the light-colored bands of the gneiss? By the definition of a gneiss, how can the Fenton Hill samples have dark gneissic bands and no light-colored gneissic bands associated with them? Dr. Baumgardner seems to have misidentified the light-colored gneissic bands as "unaltered granodiorite." The light-colored layers of a gneiss often consist of blocky feldspar and quartz grains. Without detailed chemical and microscopic studies, feldspars and quartz in a light-colored gneiss can readily appear "igneous" and "unaltered" to the naked eye. In a

later email to me, Dr. Baumgardner generously sent photographs of a couple of Fenton Hill cores and described the 750-meter core as a "granite" on the basis of its "abundant" pink orthoclase (potassium feldspar) grains. He further argued that the 1490-meter core was a "granodiorite" because of its lack of pink orthoclase. However, identifying a mineral on the basis of color is unreliable. Orthoclase can come in many colors besides pink. Other minerals besides orthoclase are also pink. Furthermore, pink feldspars can be abundant in some gneisses and entirely absent in others. Even if the samples were igneous, Dr. Baumgardner's naked-eye observations would not have been adequate enough to distinguish a granodiorite from a granite, monzonite, or any other intermediate or felsic intrusive igneous rock. Again, Humphreys *et al.* have yet to produce **any** definitive chemical or microscopic evidence to challenge the metamorphic identifications of their samples in Laughlin *et al.* (1983) and other documents. Finally, according to an April, 2007 report by Dr. Todd Feeley of a RATE presentation, Dr. Baumgardner now admits that Dr. Humphreys' zircons came from gneisses. Dr. Feeley writes:

> "Well, after the Q & A session Humphreys called me 'evil' for asking such a question (I thought it was a valid question, but Humphreys apparently didn't and I don't think he is a very nice man). I also told him that he had a problem because the core sample he showed in his talk from where his zircons were separated was clearly a gneiss and not a granodiorite ('with schist veins through it'), as he claimed. I could see this from the back row, as could the undergraduate geology students in attendance. At this point he called me 'dumb' and asked if I had the guts to tell Baumgardner (who selected the core) that the sample was a metamorphic rock and not an igneous rock. Sure, I'd tell him. As we walked over to speak with Baumgardner, a young woman who identified herself as a Christian, scolded Humphreys for being mean and not behaving in a Christian-like manner by calling me evil and dumb. She didn't think he was a very nice man either. To get back to the point, Baumgardner conceded that the core sample was indeed a gneiss and not a granodiorite. To his credit, Humphreys did begrudgingly apologize. Personally, I didn't care about the apology, which wasn't sincere anyway. I was more concerned that this guy was conducting expensive research on the age of the earth, yet couldn't even tell the difference between a metamorphic rock and an igneous rock. Oh yeah, I forgot, he's a creationist physicist and not a geologist."

In Humphreys (2008a), Humphreys (2008b) and Humphreys (2010), Dr. Humphreys continues to inaccurately refer to the relevant metamorphic sections of the Fenton Hill cores as "granitic rock."

Outdated and Inaccurate Petrologic Claims at CreationWiki

In an effort to cloud the issue on the petrology of the host rocks of Dr. Humphreys' and R. V. Gentry's zircons, the author(s) at CreationWiki states:

> "The claim that some of the rocks are gneiss is based on a couple of papers, one of which has a labeling of the upper portion as gneiss, but this conflicts with a

> paper from Los Alomos [*sic,* Alamos] labs that shows much of that portion to be granite and granodiorite. This paper clearly shows that Gentry's samples 1, from a depth of 960 m (3149.6 ft), was in granodiorite. Now it does label the rock of RATE's two new samples, depth 750 m (2461 ft) and 1490 m (4888ft) core, as granite."

Due to inadequate referencing of this citation, the CreationWiki author(s) may be referring to a Los Alamos report by Purtymun *et al.* (1974) or perhaps another report by Laney *et al.* (1981), both of which appear in the bibliography of the CreationWiki essay. The information in Purtymun *et al.* (1974) is outdated and only relied on drill cuttings (fragments) to identify the Precambrian rock types. This report has been replaced by more thorough studies by Laughlin *et al.* (1983) and Laney *et al.* (1981) that used far more complete and reliable well cores. While drill cuttings from intrusive igneous rocks and gneisses are often difficult to distinguish, the well cores used Laughlin *et al.* (1983) and Laney *et al.* (1981) are easier to identify. Laney *et al.* (1981) mostly discuss the deeper EE-2 core rather than the GT-2 core, the source of R.V. Gentry's sample 1 and Dr. Humphreys' zircons. However, Laney *et al.* (1981) do not support the CreationWiki claims that R. V. Gentry's sample 1 and Dr. Humphreys' zircons came from intrusive igneous rocks. The diagram in Laney *et al.* (1981, p. 17) clearly shows the presence of "Precambrian gneiss" at depths of 750, 960, and 1490 meters, where Dr. Humphreys' and R. V. Gentry's sample 1 zircons originated.

Next, the CreationWiki author(s) cites some webpage definitions and attempts to claim that a "granodiorite" has a composition that is close enough to be called a "granite." Of course, this argument has nothing to do with gneisses, which are metamorphic rocks and not granites or granodiorites. Nevertheless, careful geologists don't accept the sloppy use of terms to identify igneous rocks. Laney *et al.* (1981), which the CreationWiki author(s) cite, even contains a classification diagram on p. 10 that stresses the important mineralogical differences between granodiorites, granites and other intrusive rocks. So, there are definite chemical and mineralogical differences between granites and granodiorites that can and must be distinguished by geologists. In the end, the author(s) of CreationWiki is being sloppy with rock terms and is trying to create a controversy where one has not existed since the work of Laughlin *et al.* (1983) and Laney *et al.* (1981). Misidentifying a gneiss (a metamorphic rock) as an intrusive igneous rock is no more acceptable than calling frogs "reptiles," and referring to a granodiorite as a granite is like calling a gorilla, a "chimpanzee."

Gneisses have Complex Histories that Dr. Humphreys Ignores

I predicted in my original essay that if Dr. Humphreys ever began to suspect that he and his colleagues sampled gneisses and not a granodiorite, he would try to trivialize his mistakes and argue that misidentifying a gneiss would not significantly affect their zircon diffusion studies or "dating" results. This is exactly what Humphreys (2005a) attempts to do. Humphreys (2005a) tries to argue that any misidentification of the rock types in the Fenton Hill cores would not be a serious mistake:

> "The important point is that, regardless of the name we put on the rock unit [*sic*, rock units!, my Figure 1], the zircons throughout it have been measured to contain essentially the same amounts and ratios of lead isotopes [Gentry *et al.*, 1982b], and therefore have undergone the same amount of nuclear decay. The uranium, helium, and lead levels in our samples are perfectly consistent with the corresponding levels Gentry reported for his. The effect of variation from sample to sample is probably smaller than the 2-sigma error bars around our prediction. So here Henke is making a distinction without a difference."

First of all, two zircons can have **identical** U/Pb dates and Pb/Pb isotope ratios, but still greatly differ in size, which affects Dr. Humphreys' *a* values, and have radically different absolute concentrations of lead, uranium, thorium and helium (that is, very different Q/Q_0 values; my Appendix B). Secondly, Dr. Humphreys makes several bold assertions in the above paragraph that are flatly refuted by the chemical data in the very reference that he cites (*i.e.*, Gentry *et al.*, 1982b). Gentry *et al.* (1982b) show that uranium and thorium concentrations in the Fenton Hill zircons can vary by more than an order of magnitude even if the zircons are taken from the same section of the cores (my Appendix B). In the case of zircon 1A in Table B1 of my Appendix B, the uranium concentrations vary by more than an order of magnitude within the zircon! Gentry *et al.* (1982b, p. 296) readily admit:

> "Frequently, there were significant differences in the U and Th concentrations from two different locations on the same zircon."

As shown in my Appendix B and associated discussions in this and my original essay, orders of magnitude variations in the uranium and thorium concentrations of the Fenton Hill zircons could produce orders of magnitude variations in lead concentrations and Q/Q_0 values. Because Dr. Humphreys did not fully realize that his and R. V. Gentry's teams had sampled zircons from a diverse group of igneous and metamorphic rocks (my Figure 1 and Table 1, below), Dr. Humphreys did not carefully consider that the uranium, Q/Q_0, and *a* values of the zircons from these rocks could be extremely different, which would greatly impact his "helium diffusion dates." As discussed below, this oversight alone nullifies the "helium diffusion date" of 6,000 years in Humphreys *et al.* (2004).

YECs might argue that because Precambrian granodiorites and gneisses were all magically zapped into existence during the six 24-hour days of the "Creation Week" (*e.g.*, Snelling and Woodmorappe, 1998, p. 530), distinctions between Precambrian rocks really aren't important. While most YECs invoke miracles to explain away most Precambrian intrusive rocks (*e.g.*, Snelling and Woodmorappe, 1998, p. 530), Humphreys *et al.* (2003a, p. 2) unintentionally admit that at least some intrusive rocks have significant histories when they claim that zircon crystals become imbedded in larger crystals as a magma "cools and solidifies." So, Dr. Humphreys has the impossible task of explaining why the numerous metamorphic and igneous rocks in the Fenton Hill cores (my Figure 1) have complex structures and textures that indicate a long history (Laney *et al.*, 1981, Laughlin and Eddy, 1977, Laughlin *et al.*, 1983, Sasada, 1989, Loechelt (2008c), and their references) rather than a supposed rapid and miraculous formation in only six 24-

hour creation days. Like an old scratched phonograph record or a dented old car (scroll down to "Creationist Car Deal" by Dave Thomas), the properties of a metamorphosed rock often indicate an extensive and complex history involving erosion of precursor igneous, sedimentary and metamorphic rocks; sediment deposition; deep burial of sediments; multiple cooling and heating events; various complex metamorphic reactions; faulting and uplifting.

Because Dr. Humphreys collected his zircons from gneisses and not granodiorites (my Figure 1), he needs to realize that thermodynamic and other laboratory studies indicate that gneisses and their metamorphic zircons form under much greater metamorphic pressures than could ever have existed at depths of only 750 to 4,310 meters (Hyndman, 1985; Winkler, 1979). The gneisses at Fenton Hill were obviously uplifted from much greater depths. By definition, gneisses have gneissic banding, which requires minimum pressures of about 4,000 to 6,000 bars and temperatures of about 600-750°C to form. So, Dr. Humphreys' gneisses and their zircons were once at depths of at least 15-22 kilometers (Winkler, 1979, p. 5), perhaps for a significant portion of their history. Loechelt (2008c) in his Appendix A also provides a detailed geologic history of the Fenton Hill cores, which is hardly consistent with a six 24-hour "Creation Week" or even a mere 6,000 years of history. Considering that the metamorphic rocks of the Fenton Hill cores probably spent a lot of their history at depths greater than 15 kilometers, Dr. Humphreys is sadly mistaken when he believes that his modeling of helium diffusion in some zircons from current depths of 750 meters to 4.3 kilometers yield valid information on the beginning of the Earth's geologic history.

MORE BAD SCIENCE: HUMPHREYS *ET AL.* VIOLATE THE RULES ON NAMING ROCKS

Humphreys (2005a) admits that he "invented" the term "Jemez granodiorite" to describe all of the diverse Precambrian rocks in the Fenton Hill cores. However, the U.S. Geological Survey's (USGS) Geologic Names Committee and the North American Stratigraphic Code maintained by The North American Commission on Stratigraphic Nomenclature long ago established rules that professional geologists and other scientists ***must*** follow if they want to introduce a name for a rock unit into the literature. The USGS Geologic Names Committee also maintains a searchable on-line database of accepted names for American geological units. Dr. Humphreys and his colleagues violated these rules when they lumped together all of the diverse Precambrian metamorphic and igneous rocks of the Fenton Hill cores and misnamed them the "Jemez granodiorite" (Figure 1). Dr. Humphreys has not only produced an inaccurate name to describe this diverse group of rocks that could eventually spread clutter and confusion in the literature, he has also shown his inability to properly identify rocks. Dr. Humphreys has further demonstrated that he is incapable of following established scientific rules.

Humphreys (2005a) and Humphreys *et al.* (2003a, their Appendix B) also mention the existence of a "Beartooth gneiss." Humphreys (2005b, p. 41) later refers to rock as the

"Beartooth amphibolite", an entirely different metamorphic rock. Nevertheless, the USGS database and the literature databases *Georef* and *Web of Science* contain no references to the existence of the "Beartooth" or "Bear tooth" gneiss or amphibolite (accessed June 7, 2010). These metamorphically diverse names were probably pulled out of the ether by YECs at the ICR. Considering their inabilities to distinguish metamorphic from intrusive igneous rocks when they named the "Jemez granodiorite," who knows if this Beartooth rock is even a gneiss or an amphibolite.

QUESTIONABLE SAMPLE PROCESSING

Grinding of Biotite Samples

Humphreys *et al.* (2003a, p. 17) states that the biotites from the "Beartooth gneiss" ("Beartooth amphibolite" in Humphreys, 2005b) and the "Jemez granodiorite" were extracted through "crushing, magnetic separation, and density separation with heavy liquids." However, silicate minerals can lose much of their helium through crushing (Trull and Kurz, 1993, p. 1314; Mussett, 1969, p. 298). Allowing personnel from the Institute for Creation Research (ICR) laboratory to grind the biotite specimens could have resulted in substantial helium loss and significant errors in Appendix B of Humphreys *et al.* (2003a). Some researchers cut rather than crush micas for argon diffusion studies (Dalrymple and Lanphere, 1969, p. 147-148).

Dr. Humphreys' Impure Biotite Separations

According to Humphreys *et al.* (2003a, p. 6, 17) and Humphreys (2005a), the ICR laboratory extracted the biotite samples for the helium diffusion studies in Humphreys *et al.* (2003a). The results in Appendix B of Humphreys *et al.* (2003a) indicate that the Fenton Hill biotites were impure. As shown by candid statements from ICR's Dr. Steve Austin, the ICR laboratory has a history of not being able to consistently provide adequate mineral and volcanic glass separations (also see: "Young-Earth Creationist 'Dating' of a Mt. St. Helens Dacite: The Failure of Austin and Swenson to Recognize Obviously Ancient Minerals"). As I stated in my original essay, which Humphreys (2005a) ignores, adequately pure biotite separations may not be possible for the Fenton Hill samples. Certainly, Humphreys (2005a) is correct when he states that different samples provide different degrees of difficulty in mineral separation. That is, another laboratory also may not have been able to adequately separate the biotites from the Fenton Hill samples. However, considering the poor record of the ICR laboratory, Dr. Humphreys should have at least tried.

Humphreys (2005a) again tries to belittle his failures by claiming that the biotite separations are irrelevant. However, if these separations were not important, why did he bother having them done and the questionable helium analyses published? It's also obvious that without these biotite analyses, Dr. Humphreys' case is weakened. For example, sample #6 doesn't fit into their modeling scheme (see discussions below). So,

Humphreys *et al.* (2003a, p. 3, 8) used their questionable biotite analyses to argue that sample #6 is a "special case" and can be ignored in their models (see discussions below). Also, biotite and its helium diffusion properties have critical roles in some of the models described in Humphreys *et al.* (2003a, especially their Figure 7), in deriving *b* (which is needed for dating equations 12-14 and 17 in Humphreys *et al.,* 2003a, see below), and in Dr. Humphreys' invalid Lyell uniformitarian claim that current measurements of the diffusion of helium in his Fenton Hill biotites somehow rules out the possibility of extraneous helium contamination (see below).

Instead of adequately responding to the questionable Fenton Hill results from the ICR laboratory, Humphreys (2005a) challenged me to do a better job. But, why should I do his work for him? I simply have no interest in processing samples for Dr. Humphreys so that he can manipulate them to promote his YEC agenda.

MYSTERIOUS MODIFICATIONS OF THE HELIUM (*Q*) MEASUREMENTS FROM GENTRY *ET AL.* (1982a): MORE QUESTIONS THAN ANSWERS

Q refers to the **measured** quantity of helium (presumably only radiogenic ^{4}He) in a mineral. Helium will only begin to accumulate in a zircon if the mineral is below its helium closure temperature, which is about 200^{o}C (Reiners *et al.*, 2002). Even if the "dating" equations (12-14, 16, etc.) in Humphreys *et al.* (2003a) were reliable, they still would need accurate and precise measurements of Q_0, Q, a, and possibly b before any of the equations would work. Otherwise, it's garbage data in, garbage "dates" out. As discussed in this and the following sections, the data in Humphreys *et al.* (2003a) and Humphreys *et al.* (2004) are often poorly defined and inaccurate. Most of the results in Humphreys *et al.* (2003a) and Humphreys *et al.* (2004) simply fail to meet any acceptable scientific standards despite Dr. Humphreys' claims about his results being "peer-reviewed."

Gentry *et al.* (1982a) contains helium (*Q*) measurements of zircons from their Fenton Hill samples 0-6. While Humphreys (2000) simply listed the helium measurements from Gentry *et al.* (1982a), Humphreys *et al.* (2003a, post-conference version) in consultation with YEC R. V. Gentry concluded that the helium measurements in Gentry *et al.* (1982a) had "typographic errors" (see my Table 1). Their undocumented "corrections" to the measurements in Gentry *et al.* (1982a) usually included lowering most of the *Q* values by 10 times (my Table 1).

Table 1: Information on the Fenton Hill, New Mexico, GT-2 and EE-2 well cores, including the original helium concentrations (Q in nano cubic centimeters of helium per microgram of zircon at standard temperature and pressure [STP], ncc STP/μg) from Gentry *et al.* (1982a, p. 1130). Samples 0-6 are from Gentry *et al.* (1982a) and Humphreys *et al.* (2004) is the source of samples 2002 and 2003. Revised helium (Q) values are from Humphreys *et al.* (2003a, p. 3 of the post-conference revised version) and Humphreys *et al.* (2004, Table I, p. 3). Depths are from Humphreys *et al.* (2004, Table I, p. 3). Gentry *et al.* (1982a) identified the surface lithology as the Bandelier Tuff. The other lithologies are from Laughlin *et al.* (1983). The ratios of measured helium to theoretical radiogenic helium (Q/Q_0 values, defined in my Appendix A) are from Humphreys *et al.* (2003a), Humphreys *et al.* (2003b), Humphreys *et al.* (2004) and Gentry *et al.* (1982a). Humphreys (2005b, p. 30) indicates that the +/- 30% for the Q/Q_0 values are "very conservatively" one sigma random errors.

Sample No.	**Depth (meters)**	**Well Core No.**	**Lithology**	**He measurements (Q) (ncc STP/μg) from Gentry *et al.* (1982a)**	**New or Revised He measurements in Humphreys *et al.* (2004), (Q) (ncc STP/μg)**	**Q/Q_0 (±30%, 1σ)**
0	0	---	Bandelier Tuff	82	8.2	---
2002	750	GT-2	Gneiss	---	~12.1	~0.80
1	960	GT-2	Gneiss	86	8.6	0.58
2003	1490	GT-2	Gneiss	---	6.3	0.42
2	2170	GT-2	Gneiss	36	3.6	0.27
3	2900	GT-2	Granodiorite; Monzogranite	28	2.8	0.17
4	3502	EE-2	Gneiss; Monzogranite	0.76	0.16	0.012
5	3930	EE-2	Granodiorite	~0.2	~0.02	~0.001
6	4310	EE-2	Gneiss; Granodiorite	~0.2	~0.02	~0.001

Interesting Insights from CreationWiki and Humphreys (2005a)

As others (*e.g.*, Isaac, 2008b) and I have noted, Dr. Humphreys has yet to reveal adequate details on how these "typographic errors" in Gentry *et al.* (1982a) were discovered and reliably corrected, and how the associated Q/Q_0 values could remain unaffected. An unknown writer(s) at CreationWiki makes the following interesting statement about the discovery of the "typographic errors" in Gentry *et al.* (1982a):

> "The errors were discovered when Humphreys was doing the retention calculations for RATES [*sic*, RATE's] sample. He noticed an order of magnitude discrepancy in the absolute helium amounts. When he contacted Gentry, Gentry agreed that they probably were typographical errors."

It is not known whether this statement is based on a rumor or first-hand knowledge from Dr. Humphreys and/or R. V. Gentry. If this account is true, R. V. Gentry agreed that his paper "probably" contained typographical errors ***after*** Dr. Humphreys obtain his results and noticed a discrepancy between his results and the data in Gentry *et al.* (1982a). Humphreys (2005a) also admitted that:

> "Gentry's original calculations are no longer available."

If Dr. Humphreys and R. V. Gentry did not have R. V. Gentry's original calculations or laboratory notes, how do they know after more than 20 years that typographic errors had been made in Gentry *et al.* (1982a)? Was R. V. Gentry simply admitting to the possibility of "typographic errors" to help his friend, Dr. Humphreys, and the RATE project? Also, why were the Q values affected by the "typographic errors", but not the associated Q/Q_0 values? How is this mathematically possible? Correcting errors in previous manuscripts is certainly honorable. However, authors should not agree to any "corrections" unless they can first review their original laboratory notes and confirm that copying, analytical or other errors were indeed made. In other words, scientists should not admit to making mistakes before seeing the evidence.

As discussed below, there are numerous incidences where Dr. Humphreys has unjustly manipulated (*e.g.*, a graph in Magomedov, 1970) or sloppily handled data (*e.g.*, the units of measure in Appendix C of Humphreys *et al.*, 2003a). Therefore, documenting the validity of the changes to the helium values from Gentry *et al.* (1982a) is even more urgent. Dr. Humphreys needs to fully explain this issue and dispel any possible thoughts that "typographic errors" were invoked so that the data in Gentry *et al.* (1982a) could be modified (like the data in Magomedov, 1970) to comply with Dr. Humphreys and his YEC agenda. Until Dr. Humphreys and/or R. V. Gentry give a full and detailed account of what actually happened, we simply have no reason to trust any of the data in Gentry *et al.* (1982a) or any revisions of that data. Nevertheless, as discussed below, even if the revisions of the Q values in Gentry *et al.* (1982a) are completely justified, the problems associated with Dr. Humphreys' other values (Q_0, Q/Q_0, a and b) and his "dating" methods remain.

Finally, the lack of documentation from Dr. Humphreys to justify changes in the published data of Gentry *et al.* (1982a) would never be tolerated in authentic scientific journals. Any editor or peer-reviewer of a legitimate scientific journal would demand a thorough and complete explanation of why these changes are justified before any revisions would be allowed to appear in their journals. Competent editors and reviewers would also insist that if the original laboratory notes had been lost that the results be discarded and the analyses redone.

QUESTIONABLE AND UNEXPLAINED ORIGIN OF R. V. GENTRY'S AND DR. HUMPHREYS' Q_0

Once a mineral cools below its helium closure temperature and remains below that temperature, Q_0 is the **maximum** amount of radiogenic helium (^{4}He) that is expected to accumulate in the mineral from the radioactive decay of its uranium and thorium. A certain percentage of alpha particles (^{4}He nuclei) will escape from the host mineral during radioactive decay and this loss is considered when calculating the Q_0 values. My Appendix B, Loechelt (2008c), and their references discuss how alpha particle loss may be estimated.

Using a series of questionable and vague assumptions, Gentry *et al.* (1982a) derived a single maximum helium retention (Q_0) value for their 1-6 samples and used it to calculate the amount of retained helium (Q/Q_0 values) for the six samples. Humphreys *et al.* (2003a; 2004) took the high Q/Q_0 values from Gentry *et al.* (1982a) (which are essential in supporting his creation model) and "corrected" the "typographic errors" in the helium measurements (Q), which yield a Q_0 of about 15 nano cubic centimeters at standard temperature and pressure per microgram of zircon (ncc STP/µg). Using the available information from Gentry *et al.* (1982a) and ignoring the possibility of extraneous ^{4}He and ^{3}He, I was unable to derive a Q_0 of 15 ncc STP/µg for the zircons. Instead, I found that the assumptions in Gentry *et al.* (1982a) yield a Q_0 of 41 ncc STP/µg (Appendix A). Loechelt (2008c, p. 5) also concluded that the assumptions in Gentry *et al.* (1982a) would yield a Q_0 of about 40 ncc STP/µg and not 15 ncc STP/µg.

Meanwhile, Humphreys (2005a) still won't adequately explain how he and supposedly Gentry *et al.* (1982a) calculated a Q_0 of only 15 ncc STP/µg (also see my Appendix A) and why chemical data in another article by R. V. Gentry, Gentry *et al.* (1982b), indicate that Q_0 is typically much greater than 15 or even 41 ncc STP/µg (perhaps as high as 800 ncc STP/µg; see Table B8 in my Appendix B). Rather than admitting that the assumptions in Gentry *et al.* (1982a) do not support a Q_0 value of 15 ncc STP/µg or his high Q/Q_0 values (which must be high to support his creation model), Humphreys (2005a) attempts to salvage his high Q/Q_0 values by claiming that there are additional "misstated" numbers in Gentry *et al.* (1982a) related to the alpha particle loss percentage:

> "In his Appendix A Henke derives his value for Q_0, 41 ncc/µg (1 ncc = 1 "nano-cc" = 10^{-9} cm^3 at standard pressure and temperature, STP). He is in the right ball

> park, but he is probably using too small a value for the percentage of alpha particles (helium nuclei emitted by the nuclear decay) escaping the zircons. The percentage came from Gentry's paper, but Gentry may have misstated what he meant by the number."

Certainly, there are plenty of questionable assumptions and unreliable numbers in Gentry *et al.* (1982a). So, why is Dr. Humphreys still willing to trust the Q/Q_0 values in Gentry *et al.* (1982a) after he's admitted in Humphreys (2005a) and Humphreys *et al.* (2003a, post conference version) that almost every other datum in this paper is a "typographic error" or "misstated" number? When will the list of errors in Gentry *et al.* (1982a) end? Clearly, Dr. Humphreys invokes "typographical errors" and "misstatements" in Gentry *et al.* (1982a) as a convenient excuse to avoid explaining his math and justifying his measurements. As further discussed in Appendix A, the dodging and delays in Humphreys (2005a) and his willingness to selectively alter the values and assumptions in Gentry *et al.* (1982a) to protect his high Q/Q_0 values and support his YEC agenda do not add any public confidence in his ability to perform science. Dr. Humphreys either needs to thoroughly justify the Q_0 value of 15 ncc STP/µg under the assumptions in Gentry *et al.* (1982a) or admit that the approach in Gentry *et al.* (1982a) is wrong, the Q/Q_0 values given by Gentry *et al.* (1982a) are unreliable and should be discarded, and a better way must be found to estimate Q_0 and Q/Q_0 values.

Humphreys (2005a) claims that revising the Q_0 value from 15 to 41 ncc STP/µg as shown in my Appendix A would "only" reduce his Q/Q_0 values by "a factor of two or so." The author(s) at CreationWiki goes even further and inexplicably claims that increasing the Q_0 value to 41 ncc STP/µg would increase Dr. Humphreys' "date" for the Fenton Hill zircons by "only" two orders of magnitude or from "6,000" to "600,000 years." However, data in Gentry *et al.* (1982b) indicate that the Q_0 value in some cases could be as high as 800 ncc STP/µg (see Table B8 in my Appendix B). Rather than recognizing the likelihood of Q_0 values far greater than 41 ncc STP/µg or that Dr. Humphreys' and R. V. Gentry's errors associated with Q_0 are just one of many problems associated with Dr. Humphreys' work and claims, the CreationWiki author(s) attempts to dismiss this two orders of magnitude "dating" problem by invoking an inexplicable "heating event." How would a heating event help the YEC argument that the zircons are only 6,000 and not at least 600,000 years old? CreationWiki doesn't say. Again, Dr. Humphreys and his allies fail to realize that the errors associated with their Q_0 value and the numerous other questions and errors associated with Dr. Humphreys' equations and parameters only accumulate and illustrate how frail and unreliable his "dating" methods really are.

By just arguing over whether Q_0 is 15 ncc STP/µg, 41 ncc STP/µg or some value in between, Humphreys (2005a) actually misses an important point that goes way beyond Appendix A. Certainly, Appendix A demonstrates that there are serious errors in the calculations of Gentry *et al.* (1982a). However, even if Gentry *et al.* (1982a) and I had obtained the same Q_0 value, I would still argue that their approach and assumptions were flawed from the very beginning and that their Q_0 and Q/Q_0 values should be discarded. Gentry *et al.* (1982a) admit that their samples 1-6 came from a variety of rock types, which means that the uranium concentrations in the zircons from these various igneous

and metamorphic rocks are likely to be very different, and so would the Q_0 and Q/Q_0 values at the different depths within the Fenton Hill rock cores. Indeed, Gentry *et al.* (1982b) even show that the uranium and thorium concentrations of the Fenton Hill zircons are highly variable **within** single zircons (Table 2; Table B1 in my Appendix B).

Loechelt (2008c, p. 4) admits that it is not a good assumption to apply only one Q_0 value to all of the lithologically diverse samples from the Fenton Hill core. However, given the limited options with the data from Gentry *et al.* (1982a), Loechelt (2008c, p. 5) decided that the best way to test the validity of Dr. Humphreys' models was to derive a single Q_0 value (not corrected for alpha ejection) of 74 ncc STP/μg from the uranium and thorium data of a zircon from Zartman (1979) (~ sample 3 in Gentry *et al.,* 1982a). While the calculations in Gentry *et al.* (1982a) and my Appendix B assume a percent alpha particle loss for a given zircon size, Loechelt (2008c, p. 5) advocates a different method from Meesters and Dunai (2002b), where the correction for the loss of alpha particles is done during the diffusion simulations. Loechelt (2008c, p. 13) explains the advantages of the Meesters and Dunai (2002b) method:

> "Since the alpha-ejection depletes the helium in the surface region of the crystal, it takes comparatively longer for the remaining helium to escape because it is concentrated toward the center of the crystal. Hence, when the standard correction for alpha-ejection is made for samples which have also experienced loss due to diffusion, significant errors can result."

Loechelt (2008c, p. 4-6) then derives his own set of Q/Q_0 values for each one of Dr. Humphreys' and R. V. Gentry's samples. Table 2 lists Dr. Loechelt's values and compares them with the values in Gentry *et al.* (1982a), Humphreys *et al.* (2004), and my Appendix B. Like Dr. Humphreys, Loechelt (2008c, p. 16) applies his Q/Q_0 values to his own "young-Earth" and "old-Earth" models. However, unlike Dr. Humphreys, Dr. Loechelt's results strongly favor his "old-Earth" model (Figure A). Nevertheless, as stated above, the differences in the lithologies of the Fenton Hill core and the diverse chemical data of the zircons in Gentry *et al.* (1982b) demonstrate that Humphreys *et al.* (2003a), Humphreys *et al.* (2004), Gentry *et al.* (1982a), Loechelt (2008c), or anyone else simply cannot justify assigning only one Q_0 value (whether 15 ncc STP/μg, 41 ncc STP/μg or whatever) to all of the Fenton Hill samples (also see Whitefield, 2008). Statistically valid ranges of Q_0 and Q/Q_0 values are needed for each sample, which (unfortunately) are not currently available. Until reliable a, Q/Q_0 values, and other data become widely available for the rocks of the Fenton Hill cores, no "old-Earth" or "young-Earth" modeling results are definitive.

Isaac (2008b) also questions the validity of the Q_0 value used by R. V. Gentry and Dr. Humphreys from a different perspective:

> "It is not at all clear that Gentry's theoretical concentration of helium can correctly be interpreted as an initial concentration Q_o of helium. If so, how did Gentry make that calculation? What were his assumptions? If Gentry's calculation is based on an estimate of all possible helium generated by alpha-

emitters in the 1.5 billion year age of the zircon, corrected for near-surface losses, then the RATE team's assumption that at some time in the past the zircon contained a helium concentration of Q_o cannot be supported. That amount of helium was never concentrated in the zircon at the same time. [new paragraph] The physical mechanism that Humphreys proposes to explain an initial value of Q_o with a subsequent decrease in concentration is that accelerated nuclear decay during Noah's flood caused a very large alpha generation rate which then dropped to its current value. Subsequent discussion by the RATE team shows that the justification for speculating that accelerated nuclear decay occurred is based largely on a young earth as determined by helium diffusion in zircons. This is circular reasoning at best."

Table 2: Comparison of Q/Q_0 values from Humphreys *et al.* (2004), Loechelt (2008c), and my Appendix B. My revised Q/Q_0 values were derived by using the Q values from Humphreys *et al.* (2004) and uranium and thorium data on seven zircons from samples 1, 5 and 6 in Gentry *et al.* (1982b). In Appendix B, the Q/Q_0 values for sample 3 were estimated from data in Zartman (1979), which are from a zircon collected at a depth of 2903.8 meters that was probably from the same biotite granodiorite as sample 3 of Gentry *et al.* (1982a). Due to a lack of suitable uranium and thorium data, no Q/Q_0 values were derived for samples 2002, 2003, 2, and 4 in my Appendix B. Q/Q_0 values from Loechelt (2008c) have not been corrected for alpha ejections in the denominator.

Zircon(s) ID	**Depth (m)**	**Q/Q_0 from Humphreys *et al.* (2004)**	**Q/Q_0 from Loechelt (2008c)**	**Maximum and Minimum Q/Q_0 Values from Appendix B**
2002	750	~0.80	~0.16	---
1A	960	0.58	0.12	0.011-0.33
1B	960	0.58	0.12	0.048-0.26
1C	960	0.58	0.12	0.018-0.10
2003	1490	0.42	0.086	---
2	2170	0.27	0.049	---
3	2900	0.17	0.038	---
~3	2903.8	---	---	0.08-0.15
4	3502	0.012	0.002	---
5A	3930	~0.001	~0.0003	~0.0007-0.003
5B	3930	~0.001	~0.0003	~0.001-0.003
6A	4310	~0.001	~0.0003	~0.0002-0.002
6B	4310	~0.001	~0.0003	~0.0006-0.002

TWO WRONGS (Q AND Q_0) DON'T MAKE A RIGHT (Q/Q_0)

Without large Q/Q_0 values, Dr. Humphreys' dating efforts fail.

Rather than properly explaining the mysterious changes in the helium concentrations (Q) taken from Gentry *et al.* (1982a) or how Gentry *et al.* (1982a) supposedly obtained a Q_0 of only 15 ncc STP/µg, Humphreys (2005a) tries to argue that any errors in Q_0 would somehow "cancel out" and maintain the high Q/Q_0 values that he considers "crucial":

> "But after discussing the matter with him [R. V. Gentry], I'm inclined to think that even if he [R. V. Gentry] had an error in Q_0, the error canceled out when he calculated the ratio Q/Q_0, which is the crucial quantity in this analysis."

So, why does Dr. Humphreys consider the high Q/Q_0 values in Gentry *et al.* (1982a) to be "crucial"? The answer is clear. Unless the Q/Q_0 values are high, his creation model fails (also see Loechelt, 2008c and my Figure A above). However, the above statement from Humphreys (2005a) is no more than a pitiful attempt to argue that two wrongs (Q and Q_0) can somehow make a right (Q/Q_0). Just as he avoided explaining and justifying the mysterious changes in the Q values of Gentry *et al.* (1982a) (see discussions above), Humphreys (2005a) provides no details or mathematical calculations on how the errors associated with Q_0 can just happen to miraculously cancel out and continue to support the high Q/Q_0 values that are crucial to his creation model. Instead of promptly showing his calculations to quickly settle these critical issues, Humphreys (2005a) simply delayed the inevitable by making a vague promise to provide the necessary details in another *CRSQ* article sometime in the near future:

> "The paper I plan to submit to *CRSQ* will discuss this issue more fully."

> "However I did not spell out the details of that calculation, so I plan to do that in the paper I intend to submit to *CRSQ* soon."

Of course, after all these years, we're still waiting for Dr. Humphreys' promised paper with its critical calculations and justifications. Because Humphreys (2005a) had no problem performing the necessary calculations and correcting his mistake in Appendix C of Humphreys *et al.* (2003a) so that he could promptly counteract some criticisms in my original essay, why shouldn't he be able to readily explain why Q_0 is only 15 ncc STP/µg? Why the delay? The evidence indicates that after Dr. Humphreys discovered the "typographic errors" that allowed him to "correct" the Q values in Gentry *et al.* (1982a) to make them consistent with his results and after realizing that any decrease in the Q/Q_0 values would harm his creation model, Dr. Humphreys was forced to accept a single Q_0 value of only 15 ncc STP/µg that he couldn't mathematically explain without circular reasoning or justify with the diverse uranium and thorium concentrations in the Fenton Hill zircons (Gentry *et al.*, 1982b).

To derive accurate Q_0 and Q/Q_0 values for their samples, Humphreys *et al.* somehow need to obtain statistically representative uranium, thorium, lead, ^{3}He and ^{4}He analyses

on numerous individual zircons from each of their core samples. Until these accurate values become available, the best available chemical data for these calculations are in Gentry *et al.* (1982b) and Zartman (1979). The data in Gentry *et al.* (1982b) and Zartman (1979) indicate that the Fenton Hill zircons typically contain a lot more uranium and thorium than what Humphreys *et al.* (2003a) and Humphreys *et al.* (2004) realized. The uranium and thorium concentrations also show that Humphreys *et al.*'s Q/Q_0 values are far more uncertain than the ± 30% (one sigma) as claimed by Humphreys (2005b), Humphreys *et al.* (2003a) and Gentry *et al.* (1982a) (see my Appendix B and Table 2). Specifically, Gentry *et al.* (1982a) lists the Q/Q_0 value of sample #1 as 0.58. That is, the zircons of sample #1 supposedly still contain 58% of their radiogenic ^{4}He after alpha-ejection. In contrast, the chemical data from Gentry *et al.* (1982b) indicate that the Q/Q_0 for sample #1 is lower, perhaps as low as 0.011 (see my Appendix B and Table 2) or about 0.12 according to Loechelt (2008c) using the Meesters and Dunai (2002b) method to correct for alpha-ejection. As discussed below, when my lower Q/Q_0 values are entered into Dr. Humphreys' "dating equations", they often raise Humphreys *et al.*'s "helium diffusion dates" to well above 6,000 years and, in some cases, over one million years. In other cases, the revised Q/Q_0 values actually lower the "ages" of the zircons to ridiculous values of only 200 years. Meanwhile, Humphreys (2005a) and his subsequent documents never comment on the results in my Appendix B and how they have even greater negative impacts on his YEC model than the values in my Appendix A.

HUMPHREYS (2005a) CORRECTS AN ERRONEOUS UNIT OF MEASURE IN APPENDIX C OF HUMPHREYS *ET AL.* (2003a)

In my original essay, I obtained some ridiculous Q/Q_0 values using the "nmol/g" values from Table C1 in Appendix C of Humphreys *et al.* (2003a, the post-conference version). Humphreys (2005a) admits that the units should be ncc (nano cubic centimeters) rather than nmol/g. After correcting his mistake, Humphreys (2005a) chides me for not being skeptical enough of his work. In this case, Dr. Humphreys is right. There is no reason to trust Dr. Humphreys to properly handle any data.

MISSING AND QUESTIONABLE *a* VALUES

Dr. Humphreys' "dating equations" require accurate and well-defined values of a, which are currently unavailable.

In their modeling efforts, Humphreys *et al.* (2003a, p. 8; their Figure 7) assume that helium diffusion in zircons is isotropic (that is, spherical) and could be represented by a single effective radius, *a*. Of course, zircons have tetragonal (anisotropic) rather than isotropic crystalline structures, which would cause at least some anisotropy in the flow of

helium through the minerals. Nevertheless, Loechelt (2008c, p. 6) cites Meesters and Dunai (2002a) and states:

> "A rigorous diffusion model would use a realistic 3-dimensional geometry. It has been demonstrated through direct computation, however, that a simpler spherical geometry is a reasonably good approximation **provided** the effective radius is chosen such that the surface-to-volume ratio of the sphere is the same as the geometry..." [Dr. Loechelt's emphasis]

Humphreys *et al.* (2004, p. 15) respond to the issue of zircon anisotropy by claiming that switching the diffusion geometry of their zircons from an isotropic sphere to an anisotropic cylinder would change their results by less than a factor of two. This claim might be true. However, as usual, Humphreys *et al.* (2004) provide no calculations to support this claim. Furthermore, many scientists would use the more rigorous equations rather than tolerate uncertainties as high as a factor of two. (Also, see below: "A Factor Here and a Factor There Result in Huge Uncertainties for Dr. Humphreys' Agenda.")

According to Humphreys *et al.* (2003a, p. 8), Magomedov (1970) defined the *a* of a zircon crystal as one-half of its **length**. Humphreys *et al.* (2004, p. 7), Humphreys (2005b, p. 44) and Humphreys *et al.* (2003a, p. 20) adopted this convention for their isotropic helium model. In contrast, Reiners *et al.* (2004, p. 1859) describe *a* as the "average half-**width** of the tetragonal prisms" [my emphasis]. The width of a tetragonal prism may be readily determined by sieving, whereas the lengths cannot (this is why you can push a long strand of uncooked spaghetti through a window screen, but not a piece of typing paper). Because zircon crystals tend to be elongated, Dr. Humphreys' *a* values would be significantly longer than values obtained using the more widely accepted definition in Reiners *et al.* (2004). Rather than always carefully measuring critical factors such as the lengths and widths of his zircons, Humphreys (2005a) admits that the sizes of the zircons in his 750-meter (2002) sample were never determined. Instead, he simply assumed that *a* was 30 microns. Gentry *et al.* (1982a) also does not contain adequate information on the lengths and widths of their zircons.

Heimlich (1976) performed numerous measurements on zircons from various sections of the Fenton Hill cores. Based on the widths in Heimlich (1976), the average half-width (*a* as defined by Reiners *et al.*, 2004) of the zircons is probably close to 20 microns (also see my Appendix B). Loechelt (2008c, p. 6) also argues that *a* for the Fenton Hill zircons should be closer to 20 rather than 30 microns. Estimating *a* at 30 microns, 20 microns or a similar value may seem trivial. However, similar studies show that poorly defined *a* values can introduce huge errors in the argon diffusion coefficients of feldspars, which are silicate minerals (McDougall and Harrison, 1999, p. 147-148). Specifically, Mussett (1969) showed that improper estimates of *a* can cause the argon diffusion coefficients (*D* values) to vary by over **seven** orders of magnitude at a given temperature (also see McDougall and Harrison, 1999, p. 147). So, even if isotropic diffusion is a reasonable assumption for Dr. Humphreys' zircons, inaccurate *a* values for the Fenton Hill zircons could introduce unacceptable errors into "dating" equations 12-14 and 16 of Humphreys *et al.* (2003a). Like many other issues dealing with Dr. Humphreys' helium in zircon

studies, the author(s) at CreationWiki also fails to recognize the inadequacy of Dr. Humphreys' measurements of *a* and the seriousness of poorly defined and inaccurate values of *a*, Q_0, and other parameters to Dr. Humphreys' YEC equations and agenda.

POORLY DEFINED AVERAGE *b* VALUE

Some of Dr. Humphreys' "dating equations" require accurate values of b that are not currently available.

Biotite is a mica, which is a well-layered silicate mineral. Because of the well-developed and prominent cleavage planes between the biotite layers, the layers can be readily peeled off with finger nails. The cleavage planes also make biotite very anisotropic. Helium would tend to migrate through the planes rather than perpendicular or oblique to them. Obviously, Humphreys *et al.* (2003a) made a serious mistake when they assumed that biotite is isotropic in their models. The models in Humphreys *et al.* (2003a) are further harmed because Humphreys *et al.* (2003a, p. 8) failed to indicate how many biotite grains were measured to obtain *b* (the radius of the biotite supposedly surrounding each zircon as shown in their Figure 7). The variable *b* must be known in order to obtain "helium diffusion dates" from equations 12-14 and 17 in Humphreys *et al.* (2003a). Dr. Humphreys' documents only list one *b* value, which is an average of ~1000 microns for an unknown number of biotites from the 750-meter (2002) sample (Humphreys *et al.*, 2003a, p. 8). In my original essay, I criticized Humphreys *et al.* (2003a, p. 8) for failing to indicate how many grains were measured to obtain this average, providing no standard deviations for this value, and then erroneously applying this one average (like he did with his Q_0 value) to other samples from the Fenton Hill cores (Table 1). Because descriptions in Laughlin *et al.* (1983) indicate that samples 1-6 in Gentry *et al.* (1982a) and samples 2002 and 2003 from Humphreys *et al.* (2004) were from diverse metamorphic and igneous rocks (my Table 1), it's likely that the sizes, and therefore the *b* values, of the biotites from these different rocks are very dissimilar.

Rather than providing suitable measurements and standard deviations for *b*, Humphreys (2005a) again throws out the same old lame excuses. He tries to belittle his mistakes by claiming that accurate *b* values really aren't important because the biotites supposedly only have minor effects on his results. However, Dr. Humphreys fails to remember that his single *b* value played a key role in his efforts to remove sample 6 from his models and obtain his desired "helium diffusion date" of 6,000 years (see discussions below).

Even if accurate *b* values were not very important to his "dating" efforts, Dr. Humphreys' omission of valid averages and standard deviations for any of his data is not a trivial issue. His lack of suitable averages and standard deviations (whether for his *a* or *b* values) exposes serious shortcomings in his laboratory procedures.

Finally, Humphreys (2005a) replies to my criticisms of his *b* measurements with the following nonsensical statement:

> "However, Henke has the raw data we published, so he can compute the standard deviations for himself."

I need to remind Dr. Humphreys that his papers **only** contain **one** *b* value, which is supposedly an average as listed at Humphreys *et al.* (2003a, p. 8). Contrary to the claims in Humphreys (2005a), the necessary raw data to calculate a standard deviation for *b* are **not present** in any of his documents. ***So, how can anyone obtain an unbiased (n-1) standard deviation from only one number?! Calculating the standard deviation would lead to division by zero!*** This is yet another example of Dr. Humphreys flippantly trying to dismiss criticism without really thinking about the ridiculous implications of his rash replies.

DR. HUMPHREYS FUDGES SOVIET HELIUM DIFFUSION DATA TO SUPPORT HIS AGENDA

Dr. Humphreys' Manipulation of the Magomedov (1970) Data

Without his log base-10 manipulation of Magomedov's graph, Dr. Humphreys' methodology provides ridiculous "creation" dates of only a few decades for Magomedov's zircons.

Humphreys *et al.* (2003a, p. 6) and Humphreys *et al.* (2004, p. 2) cite Magomedov (1970), a Soviet article, which contains some early data on helium diffusion in zircons. Only a brief abstract of Magomedov (1970) is readily available in English:

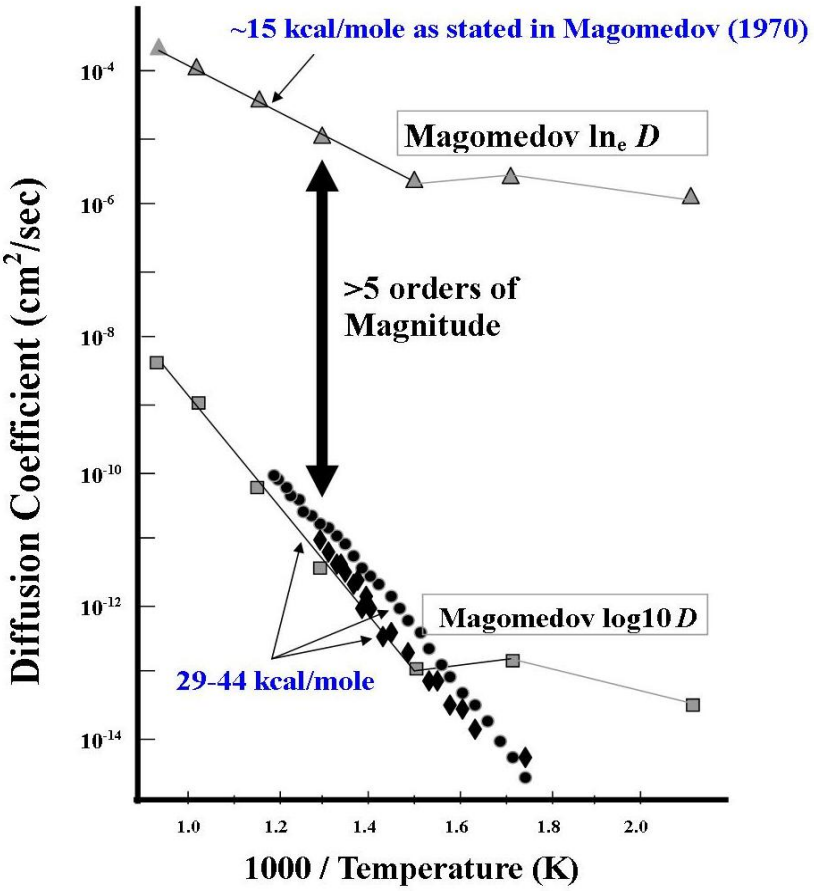
> "Heating experiments at 1000 and 1150°C and up to 48 hours on zircon suggest loss of surface lead and helium is considerable during the first few hours. Estimates of activation energy of bulk diffusion are 58 kcal/mole for Pb in zircon, and only 15 kcal/mole for He."

Dr. Humphreys, however, has an English translation of the entire Russian article (Humphreys *et al.*, 2003a, p. 16).

Humphreys *et al.* (2003a, p. 6) describe a graph in Magomedov (1970, his Figure 3) and reproduce it in their Figure 5 (p. 6) (also see my Figure 2). The y-axis of the graph in Magomedov (1970) has the English units of "ln(D,σ)," where "ln" refers to natural log, D represents the diffusion coefficient and σ refers to electrical conductivity, which may influence diffusion in some crystals as cited in Girifalco (1964, p. 92-102), a reference used by Humphreys *et al.* (2003a). Based on helium diffusion results of zircons from the Fish Canyon Tuff (Reiners *et al.,* 2002), Humphreys *et al.* (2003a, p. 6) conclude that the units on Magomedov's graph must be "incorrect" and that the actual units should be log

base 10 D ($\log_{10} D$). Based on this faulty assumption, Humphreys *et al.* (2003a, p. 6) manipulate the Magomedov (1970) data from natural log (ln) to log base 10 to comply with their data and the data in Reiners *et al.*'s (2002). As further discussed below, Dr. Humphreys' unjustified manipulation of the data in Magomedov (1970) exposes his inability to properly handle the literature, even with an English translation.

Figure 2. Arrhenius plot of helium diffusion in zircons from the Soviet Union (gray triangles; Magomedov, 1970), Nevada (black circles; Reiners *et al.*, 2002) and the Fenton Hill core (black diamonds; Humphreys *et al.*, 2003a) (based on Figure 5 of Humphreys *et al.*, 2003a). Magomedov (1970) reported that the activation energy of his zircons was 15 kcal/mole, which is consistent with the slope of the intrinsic portion of the ln D curve (gray triangles). When Humphreys *et al.* (2003a, p. 6) improperly changed the diffusion units of the Magomedov data from natural log (ln) to log base 10 (log) (gray squares) to correspond with their and the Reiners *et al.* results, the activation energy of the intrinsic curve became approximately 40 kcal/mole, which contradicts the results in Magomedov (1970).



Equations in Magomedov (1970) Definitely Indicate the Use of Natural Logs

Although Dr. Humphreys has an English translation of Magomedov (1970), it's obvious that he did not carefully study the article and its equations. The equations in Magomedov (1970) clearly refute Dr. Humphreys' $\log_{10} D$ interpretation. The standard Arrhenius equation in Magomedov (1970, his Equation 4), states:

$$D = D_0 \exp\left(-\frac{E}{RT}\right)$$

Magomedov (1970) even admits that he used his e-based Equation 4 to construct his Figure 3, which is the graph that Humphreys *et al.* (2003a, p. 6) incorrectly claim has units of $\log_{10}$ D rather than ln D. Magomedov (1970) states:

> "Используя формулу (4) можно определить значения E и D_0, строя график зависимости ln D 1/T. На рис. 3 нанесены значения ln D в зависимости от обратной температуры для свинца и гелия. По углу наклона кривых рассчитаны соответствующие значения E."
>
> *English Translation:* "Using Equation (4), it is possible to determine the values of E and D_0 by constructing a graph of the relationship ln D vs. 1/T. In Figure 3, ln D is plotted as a function of reciprocal temperature for lead and helium. The slope of the curves calculates the corresponding values of E."

Magomedov's Equation 4 also appears as Equation 2 in Humphreys *et al.* (2003a, p. 5). So, Dr. Humphreys should know that this equation is e-based and not base 10. (To use $\log_{10} D$ in Equation 4 of Magomedov, 1970, a conversion factor of 2.303 would have to be added to the equation, which yields: $\log D = \log D_0 - ((E/2.303R)(1/T))$, see McDougall and Harrison, 1999, p. 144.)

From his Equation 4, Magomedov (1970) derives the following natural log (ln) equation (his Equation 5):

$$E = \frac{R \ln\frac{D_1}{D_2} T_1 T_2}{\Delta T}$$

The steps for deriving Magomedov's Equation 5 from his Equation 4 would be as follows:

$$D_1 = D_0 \exp\left(-\frac{E}{RT_1}\right)$$

$$D_2 = D_0 \exp\left(-\frac{E}{RT_2}\right)$$

$$\frac{D_1}{D_2} = e^{-E/RT_1}\, e^{+E/RT_2}$$

$$\frac{D_1}{D_2} = e^{-\frac{E}{R}(1/T_1 - 1/T_2)}$$

Natural logs (ln) are the taken on both sides of the equation. (The use of log base 10, as Dr. Humphreys desires, would require inserting the 2.303 conversion factor into the equation.)

$$\ln\frac{D_1}{D_2} = -\frac{E}{R}(1/T_1 - 1/T_2)$$

$$\ln\frac{D_1}{D_2} = -\frac{E}{R}\left(\frac{T_2 - T_1}{T_1 T_2}\right)$$

$$E = \frac{R \ln\frac{D_1}{D_2} T_1 T_2}{T_1 - T_2}$$

Let $\Delta T = T_1 - T_2$.

The result is then Equation 5 from Magomedov (1970), which is natural log based:

$$E = \frac{R \ln\frac{D_1}{D_2} T_1 T_2}{\Delta T}$$

Lead Data in Magomedov (1970) Further Confirm the Use of Natural Logs

Magomedov (1970) only shows a graph of his helium diffusion data and does not list any numerical results in a table. However, he does list his lead diffusion results in his Table 1, which conclusively demonstrate that Magomedov (1970) was using natural logs in his equations and graphs, and not $\log_{10} D$ as Dr. Humphreys desires. Specifically, Table 1 in Magomedov (1970) lists the diffusion of lead in zircon as $D/a^2 = 1.2 \times 10^{-8}$ 1/sec at 1000°C and $D/a^2 = 1.32 \times 10^{-7}$ 1/sec at 1150°C, or with $a = 75$ microns, $D = 2.1 \times 10^{-4}$ cm^2/sec at 1000°C and 2.3×10^{-3} cm^2/sec at 1150°C. If these data are entered into Magomedov's Equation 5, the activation energy for lead (E) is 58 kcal/mol, which is the exact value that is listed in the English abstract of Magomedov (1970). (Also, see my Table 3 and the English abstract above). If "ln" means $\log_{10}$ in Magomedov (1970), as Dr. Humphreys claims, Equation 5 would yield an incorrect value of $E = 25$ kcal/mol for lead (see my Table 3). Also, there is also no reason to believe that Magomedov (1970) would inconsistently use "ln" to represent natural log in Equation 5, but have "ln" represent $\log_{10}$ on the y-axis of his Figure 3.

Table 3: Confirmation that lead diffusion results in Table 1 of Magomedov (1970) are based on natural logs and not log base 10 as desired by Humphreys *et al.* (2003a).

Temp., °C	**D/a^2, 1/sec**	**D for a = 75 microns, cm^2/sec**	**log D_1/D_2**	**ln D_1/D_2**	**E in kcal/mol from log base 10 in Magomedov's Equation 5**	**E in kcal/mol from natural log (ln) in Magomedov's Equation 5**
1150	1.32×10^{-7}	2.3×10^{-3}	1.04	2.40	25 (incorrect)	58 (exactly matches the value in Magomedov's abstract)
1000	1.2×10^{-8}	2.1×10^{-4}				

The use of ln as a natural log rather than log base 10 is also verified by further comparing the lead data at 1000 and 1150°C in Magomedov's Table 1 with the ln graph in his Figure 2b. Natural log values from the results in Table 1 correspond well with the points in the graph of Figure 2b in Magomedov (1970), but log base 10 values are far too small.

As an additional confirmation that Magomedov (1970) was using natural logs, when the temperature and diffusion coefficients from Magomedov's Table 2 are entered into Magomedov's Equation 5, the resulting activation energy (E) is 23.5 kcal/mole, which is very close to the value of 23.4 kcal/mol in his Table 2. The use of log base 10 diffusion

values with the data in Table 2 of Magomedov (1970) would yield an activation energy of only 10.2 kcal/mole.

Clearly, the data within Magomedov (1970) overwhelmingly indicates that he was using natural logs. Dr. Humphreys has absolutely no justification for arguing for a log base 10 interpretation of the Magomedov data and fudging Magomedov's helium diffusion data to support his YEC agenda. As discussed below, the ramifications of the natural log format in Magomedov (1970) undermine Dr. Humphreys' YEC agenda.

The high helium diffusion coefficients in the Magomedov (1970) are not surprising considering that Magomedov's zircons were very metamict (damaged by a lot of radioactive decay). There is also an 11 order of magnitude difference (wow!) between the lead diffusion coefficients in the zircons of Magomedov (1970) and a gem-quality Sri Lankan zircon described in Lee *et al.* (1997). Considering how the physical and chemical properties of zircons may significantly vary from one specimen to another, Humphreys *et al.* (2003a) simply has no justification for "correcting" the Magomedov (1970) data to comply with their helium diffusion results and the results in Reiners *et al.* (2002) (see my Figure 2). While Humphreys *et al.* (2003a, p. 6) boast that their $\log_{10} D$ interpretation of the Magomedov (1970) data is five orders of magnitude too high for their "uniformitarian model," they forget to mention that before they "corrected" the Magomedov (1970) data, the Magomedov (1970) data were at least five orders of magnitude higher than their zircon results and the Fish Canyon Tuff data from Reiners *et al.* (2002) (see my Figure 2).

Distorted Magomedov Graph at CreationWiki

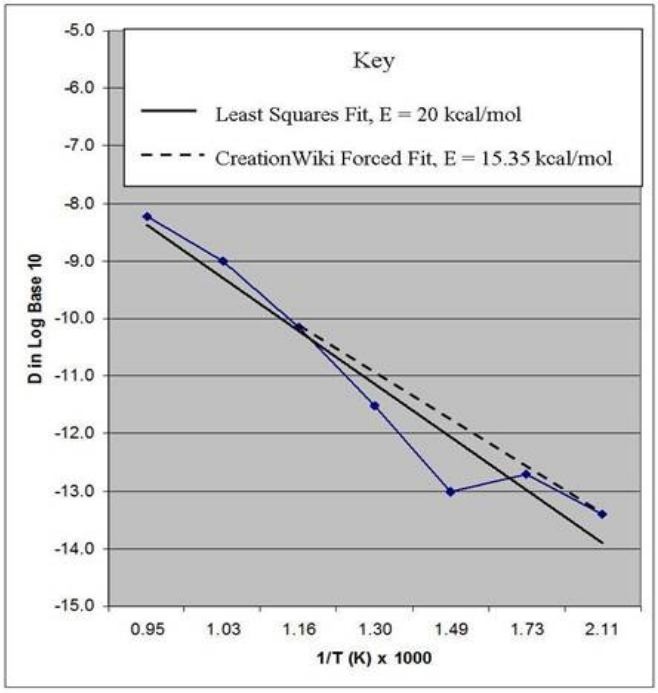
Even statements in Humphreys (2000) contradict the desperate efforts of the CreationWiki author(s) to conjure up a line with a 15 kcal/mol slope and salvage Dr. Humphreys' manipulation.

In the process of fudging the units on the y-axis of Figure 3 in Magomedov (1970) from natural log to log base 10, Humphreys *et al.* (2003a) did not realize that the slope of the intrinsic curve automatically changed (see my Figure 2). Because the slope of the intrinsic curve determines the activation energy of the sample, the activation energy of the base 10 log intrinsic curve no longer complies with Magomedov's value of 15 kcal/mole (see the above English abstract). Like the 29-44 kcal/mole results in Humphreys *et al.* (2003a, p. 7) and Reiners *et al.* (2002, p. 301), the activation energy of the base 10 log curve is now about 40 kcal/mole (see my Figure 2).

Like Dr. Humphreys, the author(s) at CreationWiki completely ignores the equations, figures and tables of supporting data in Magomedov (1970) that conclusively indicate the use of natural logs. The author(s) attempts to defend Dr. Humphreys' log base 10 interpretation of the Magomedov (1970) data by showing that an "activation energy" (E) of 15.35 kcal/mol could be derived by passing a line through two points from the combined Magomedov (1970) intrinsic and extrinsic data in a log base 10 format (see my Figure 3). Although the CreationWiki author(s) claimed to have drawn a "best fit" line

through the Magomedov (1970) data, it is difficult to believe that Magomedov (1970) would derive his activation energy for helium by passing a single line through only two points selected from his obviously diverse intrinsic and extrinsic curves. Not even Dr. Humphreys supports the CreationWiki approach for determining the activation energy of this sample (see Figure 6 in Humphreys, 2000, p. 347, where Dr. Humphreys clearly associates the 15 kcal/mol with only Magomedov's intrinsic curve). In reality, a statistically valid least squares (best) linear fit through all of the Magomedov (1970) helium data in a log base 10 format yields an activation energy (*E*) of about 20 kcal/mol, which is inconsistent with the 15 kcal/mol result from Magomedov (1970). In contrast, the slope of the intrinsic curve of the natural log distribution of the Magomedov (1970) data provides a better activation energy of 16-17 kcal/mol. Considering that numerical values of Magomedov's *D* and temperature results are not listed and must be estimated from his Figure 3, an activation energy of 16-17 kcal/mol for helium is reasonably close to Magomedov's value of 15 kcal/mol.

Figure 3. In an unsuccessful attempt to defend Dr. Humphreys' manipulation of the Magomedov (1970) data and to obtain the desired activation energy of 15 kcal/mol listed in the text of Magomedov (1970), the author(s) of CreationWiki contradicts the approach in Figure 6 of Humphreys, (2000, p. 347), disregards the equations and data in Magomedov (1970) and "fits" a linear curve using only two of the seven Magomedov (1970) data points in a log base 10 format. Although the CreationWiki author(s) claims that his/her/their line is a "best fit", a statistical least squares best linear fit for all of Magomedov's data in a log base 10 format yields an unacceptable activation energy of 20 kcal/mol and not 15 kcal/mol.



Dr. Humphreys' Fudging of the Magomedov (1970) Data is Inexcusable and his Actions Show that He cannot be Trusted with Data

Humphreys (2005a) accuses me of lying when I stated in my original essay that Humphreys *et al.* (2003a) fudged the Soviet helium diffusion data from Magomedov (1970). But how else can we describe the actions of Humphreys *et al.* (2003a)? Without any legitimate justification and in contradiction to the very equations, figures and tables of data in Magomedov (1970), Humphreys *et al.* (2003a) changed the units of measure on the y-axis of the Magomedov (1970) graph from natural logs to base 10 logs so that the Soviet data lined up with their results and the results in Reiners *et al.* (2002) (my Figure 2; Figures 5 and 6a and p. 11 in Humphreys *et al.*, 2003a). Because Humphreys *et al.*, (2003a, p. 16) admit to having an English translation of Magomedov (1970) and Equations 4 and 5 in the original Russian manuscript of Magomedov (1970) are in an English format, Dr. Humphreys has no excuse for either ignoring or incompetently misinterpreting the equations and data in Magomedov (1970).

Dr. Humphreys' willingness to alter results from the literature to suit his religious agenda is not a "ridiculous quibble" as he claims in Humphreys (2005a), but a serious act of misconduct that illustrates how fast and loose he is with data. Contrary to Humphreys (2005a), there is **NOTHING** reasonable about him altering data to line up with his YEC expectations and "everybody else's zircon data." Since when does any scientist manipulate a data set to "line up with everybody else's zircon data"? What happens if everybody else's "view" of helium diffusion doesn't apply to these highly metamict Soviet zircons? Even Humphreys *et al.* (2003a, p. 6) admit that minerals from different locations **should not** have the same properties:

> "Measurements of noble gas diffusion in a given type of naturally occurring mineral often show significant differences from site to site, caused by variations in composition."

So, when individuals (like Dr. Humphreys) ignore their own previous warnings, unquestionably manipulate a data set from the literature to comply with the results that they want and then boast that their results are "consistent" with the manipulation (Humphreys *et al.*, 2003a, p. 11), THAT IS FUDGING. Dr. Humphreys' misuse of the Magomedov data shows that he is willing to do just about anything if an opportunity arises to manipulate a data set to promote his anti-science agenda. One can only wonder if Dr. Humphreys used the same type of manipulation to convince R. V. Gentry to admit to "typographic errors" in his *Q* values so that R. V. Gentry's values could also be "corrected" to comply with Dr. Humphreys' results.

The Serious and Inconvenient Consequences of the ln *D* Magomedov (1970) Data to Dr. Humphreys' "Dating Equations"

The high helium diffusion rates in the Soviet zircons based on natural logs have dire consequences for Dr. Humphreys' "dating" equations. Instead of discussing the Magomedov data and adequately explaining his manipulation, Humphreys (2005a)

accuses me of just wanting to reject the Magomedov data because I find them "inconvenient." In reality, it was Humphreys *et al.* (2003a) that found the extremely fast helium diffusion results from Magomedov (1970) to be so **inconvenient** that they manipulated the units of measure and ignored the obvious natural log-based equations and data in Magomedov (1970) to protect their "creation date" of 6,000 years. Without fudging the Soviet data, Dr. Humphreys is put into an extremely **inconvenient** position of trying to explain why these Soviet results are several orders of magnitude higher than his measurements (my Figure 2) and why his "dating" approach indicates that these Soviet zircons were "created" in the 20th century. As Humphreys (2000, p. 347) admitted, when he combined a = 22 microns and a Q/Q_0 value of 0.58 from Fenton Hill sample #1 to the natural log helium diffusion values from Magomedov (1970), he got a ridiculous "creation date" of 23 years.

Dr. Humphreys Admits that the Magomedov (1970) Helium in Zircon Data are "Ambiguous." So, Why Didn't He Discard Them?

Although the natural log Magomedov data support my arguments by exposing the ridiculous nature of Humphreys *et al.*'s "dating" scheme, I still advocate discarding the Magomedov data because they were probably produced on antiquated equipment and they were not unambiguously listed as numbers in a table. While Dr. Humphreys is willing to manipulate ambiguous data produced with old Soviet technology or the claims of ancient Middle Eastern texts, scientists would want state-of-the-art results (for example, as discussed below, the lead data from Lee *et al.,* 1997 and Cherniak and Watson, 2000 and not outdated results from Magomedov, 1970). Dr. Humphreys also admits in Humphreys *et al.* (2003a, p. 6) and Humphreys (2005a) that the Soviet data are ambiguous. So, if Dr. Humphreys recognizes that these data are ambiguous, why didn't he simply discard them rather than manipulate them to support his agenda?

The Results of Dr. Humphreys' Fudging Spreads into the Scientific Literature

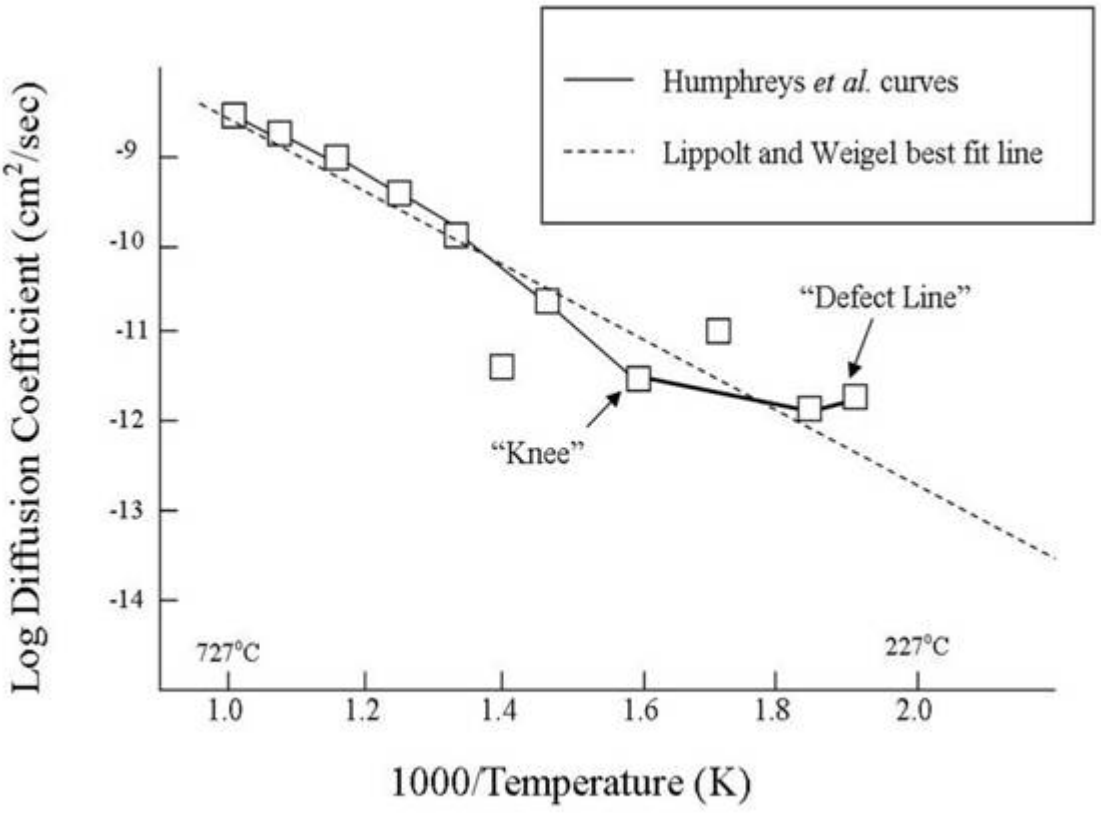
The deceptive effects of Dr. Humphreys' manipulation of the Magomedov (1970) data have already gotten a foothold in the mainstream science literature via a 2004 article by YEC Mark Armitage. Armitage (2004, p. 19) claims that the Reiners *et al.* (2002) data "lined up well" with the results from Magomedov (1970). Of course, the Magomedov and Reiners *et al.* results ONLY "lined up well" after Magomedov's data had been fudged as shown in Figure 5 of Humphreys *et al.* (2003a, p. 6) (also see my Figure 2). While scientists generally know better than to quote literature from YEC organizations, Armitage (2004) is in a legitimate analytical chemistry journal.

Dr. Humphreys Misrepresents Another Arrhenius Plot from the Literature

In Dr. Humphreys' results, a defect line on an Arrhenius graph has an essential role in supporting his creation model (my Figure B). Humphreys *et al.* (2003a, p. 5) further assume that Arrhenius graphs typically, if not always, have defect lines. However, Reiners *et al.* (2002) and Lippolt and Weigel (1988) contain examples without defect lines. Rather than faithfully reproducing an Arrhenius graph from Lippolt and Weigel

(1988, p. 1454), Humphreys *et al.* (2003a) selectively connected some data points on the graph, which easily creates the false impression that a "knee" and "defect line" are present (my Figure 4). Dr. Humphreys in Humphreys (2005a), Humphreys (2006) and his subsequent documents never comments on his "interpretation" of the Lippolt and Weigel diagram and how his modifications of data from the literature could easily lead to misinterpretations.

Figure 4: Arrhenius plot of muscovite data from Lippolt and Weigel (1988, p. 1454) and their linear best-fit curve. In their Figure 6b, Humphreys *et al.* (2003a, p. 7) remove the Lippolt and Weigel best fit line and selectively connect some of the data points, which could create false impressions that a "knee" and "defect line" are present.



A FACTOR HERE AND A FACTOR THERE RESULT IN HUGE UNCERTAINTIES FOR DR. HUMPHREYS' AGENDA

When confronted with the large uncertainties in his modeling assumptions, data and other claims, Dr. Humphreys frequently attempts to trivialize them as being "inconsequential" without showing any calculations to back up his dismissive assertions. For example, Humphreys (2005a) claims that revising his Q_0 value from 15 ncc STP/μg to a more realistic value of 41 ncc STP/μg as shown in my Appendix A would supposedly only reduce his Q/Q_0 values by "a factor of two or so." The author(s) at CreationWiki further claims that increasing the Q_0 value to 41 ncc STP/μg would increase Dr. Humphreys' "date" for the Fenton Hill zircons by "only" two orders of magnitude or from "6,000" to "600,000 years"! Neither Dr. Humphreys nor the author(s) at CreationWiki provide any calculations to back up their assertions.

In a different situation, Humphreys *et al.* (2004, p. 15) admits that switching the diffusion geometry of his zircons from an isotropic sphere to a more realistic anisotropic cylinder would change his results by "less than a factor of two." When asked by YEC Roger Wiens about whether the accumulation of radiation defects in the zircons would significantly affect his helium diffusion results for the creation model, Humphreys (2008b) sounds like a broken record. He again attempts to trivialize the issue and emphasizes the "100,000 discrepancy" between the diffusion data and the "uniformitarian model":

> "Effect turns out to be only a factor of two, within our error bars, and again vastly smaller than the factor of 100,000 discrepancy observed."

When discussing the effects of one kilobar of pressure in the subsurface of Fenton Hill on the helium diffusivity in his zircons, Humphreys (2006) again arm waves and provides no calculations to support his claims. Instead, he tries to trivialize the problem by stating:

> "For a change of only 1 kilobar pressure, the change in diffusivity would probably be about one order of magnitude. This is far less than Henke's desired six orders of magnitude."

When confronted with the more realistic results from the models in Loechelt (2008c), Humphreys (2008b) further attempts to deny the potentially fatal implications of Dr. Loechelt's work by once more invoking his old unsubstantiated excuse:

> "Loechelt also whacks away at some of my calculations. If he were correct, my calculations might have to be adjusted by a factor of two or so. But that would still be within the error bars of the models."

After invoking all of these "factors of two" or other discrepancies in numerous situations, what makes Dr. Humphreys think that he's still within the error bars of his models? Where are Dr. Humphreys' calculations to support his conclusions that all of these "factors of two", etc. adjustments are actually trivial and all together add up to nothing?

Without any calculations, how does Dr. Humphreys even know that any of these various discrepancies are only a "factor of two or so" and not much greater? The problem is that an order of magnitude or a factor of two change here or there can quickly negate his claims for a 6,000 year old Earth, especially if each of these changes are actually much greater than "a factor of two" or "an order of magnitude." Based on his statements on the errors supposedly canceling out in the calculation of R. V. Gentry's Q/Q_0 values (see above), Dr. Humphreys probably hopes that all of these errors would somehow miraculously cancel out and preserve his bogus creation date of 6,000 years. Again, Dr. Humphreys never produces any calculations to support his tedious and flippant excuses, and the evidence presented in this essay and its references does not support his sloppy attempts to belittle the numerous problems with his work. Certainly, any real scientists would perform the calculations (such as what was done in Loechelt, 2008c) rather than just waving their arms and hoping that all of these "factor of two or so" discrepancies would magically cancel out and disappear.

DR. HUMPHREYS' INCONSISTENT TREATMENT OF SAMPLES 5 AND 6 TO SUPPORT HIS "CREATION MODEL"

Data Points are not to be Rejected Just to Protect Bad Models

To develop and promote their creation model, Humphreys *et al.* must explain the helium distributions in the Fenton Hill core samples and also demonstrate that helium diffusion in the zircons under actual subsurface conditions is only consistent with a 6,000 year-old time span. While reviewing their data, Humphreys *et al.* readily noticed that their Q and Q/Q_0 values for samples 1-5 consistently decrease with depth and increasing subsurface temperatures. Humphreys *et al.* (2003a, p. 3) recognized that the helium concentration (~0.02 ncc STP/μg) in sample 5 agrees with the temperature and helium concentration trends in samples 1-4, but that an identical helium measurement from sample 6 is too high to fit their model. To validate their creation model, Humphreys *et al.* (2003a, p. 3, 8) must demonstrate that the Q and Q/Q_0 values for sample 5 are trustworthy and should be included in their models. At the same time, Humphreys *et al.* must think of some excuse to treat the identical result from sample 6 as a "special case" (Humphreys *et al.*, 2003a, p. 3) and somehow eliminate it from their modeling efforts.

Questionable Validity of Both Samples 5 and 6

According to Laughlin *et al.* (1983), sample 5 is a biotite granodiorite, whereas sample 6 consists of a gneiss and a biotite granodiorite (Table 1). Gentry *et al.* (1982a, p. 1130) admit that the low concentrations of helium in the zircons of these samples may not be *in-situ* radiogenic ^{4}He:

> "In fact, at present we are **not** certain whether the minute amounts of He recorded from the deepest zircons (3930 and 4310 m [*i.e.*, samples 5 and 6]) are actually residual He in the zircons **or derived from some other source.**" [my emphasis]

"Derived from some other source" could mean extraneous helium (see below) or possibly interferences from the analytical equipment. It's also possible that both the helium in samples 5 and 6 are in equilibrium with extraneous background concentrations that may include contributions from regional volcanic, hydrothermal and/or tectonic activities sometime in the recent geologic past (*e.g.*, Harrison *et al.*, 1986).

Due to the uncertainties associated with the helium measurements of samples 5 and 6, Gentry *et al.* (1982a, p. 1130) only listed the Q and Q/Q_0 values for samples 5 and 6 as approximations. Although Humphreys *et al.* (2003a, p. 3) claim that they will "allow for the possibility" that the error on the helium measurement of sample 5 is considerably larger than the errors of samples 1-4, their Table 1 lists no error for the Q/Q_0 value of sample 5 and they generally treat the helium concentration of the sample in a quantitative manner in their models (as examples, Tables 4 and 5 in Humphreys *et al.*, 2003a, p. 12). The semiquantitative (at best) nature of the helium (Q) results for samples 5 and 6 must also be remembered when evaluating Humphreys *et al.*'s helium diffusion "dates" (see my Table 4 and associated discussions below).

Rather than treating both samples 5 and 6 as contamination during analysis, unreliable instrument noise, minor helium background concentrations, or in another consistent manner, Humphreys *et al.* (2003a) attempt to justify eliminating sample 6 from their models. At the same time, they show unjustified bias and fail to apply the same standards to sample 5.

Dr. Humphreys Confuses Area and Volume

As part of their efforts to remove sample 6 from their models, Humphreys *et al.* (2003a, p. 8) make the following nonsensical arguments:

> "Because b is more than 32 times larger than a, the disk-like (not spherical) volume of biotite the helium enters is more than 1000 (~32 squared [*sic*]) times the volume of the zircon. This consideration affects the boundary conditions we choose for $r = b$, and how we might interpret sample 6 (see sect. 2), as follows. [new paragraph] Suppose that helium could not escape the biotite at all. Then as diffusion proceeds, C would decrease in the zircon and increase in the biotite, until the concentration was the same throughout the two materials. After that C would remain essentially constant, at about 0.001 C_0. The fraction Q/Q_0 remaining in the zircon would be about 0.001, which is just what Gentry observed in sample 6."

First of all, what is meant by "disk-like volume"? How can Humphreys *et al.* (2003a, p. 8) say: "...the disk-like (not spherical) **volume** of biotite the helium enters is more than 1000 (~32 **squared**) times the **volume** of the zircon, [my emphasis]" when volumes have three dimensions and not two? (That is, cubed and **not** squared dimensions.) If Humphreys *et al.* are trying to compare a and b by passing a random plane through the center of a zircon and into its surrounding biotite, how can $C \sim 0.001\ C_0$ because in the real world the plane would probably intersect several other zircons that are additional

sources of helium? Perhaps, Humphreys *et al.* (2003a) are suggesting in their statements that all of the helium diffusing out of a sample 6 zircon enters into only one **apparently** two-dimensional "disk-like" biotite cleavage plane. If so, the volume of this biotite feature is **not** 1000 times the volume of Humphreys *et al.*'s spherical zircon with a = 30 microns. The volume of their ideally spherical zircon = $4/3\pi a^3 = 4/3\ (3.141)\ 30^3$ = 113,000 cubic microns. The typical width [h] of a biotite cleavage is about 3.4 Å [0.00034 microns] (Bailey, 1984, p. 20-23). Using a b value of 1,000 microns as argued by Humphreys *et al.* (2003a, p. 8), the volume of that cleavage would only be 1070 cubic microns ($V = \pi b^2 h = 3.141\ [1000]^2\ [0.00034]$ = 1070 cubic microns). So, $V_{biotite} / V_{zircon}$ = 0.0095 and not 1000. So, the vague arguments about "two-dimensional" volumes in Humphreys *et al.* (2003a, p. 8) do absolutely nothing to support their efforts to dispose of sample 6.

Invalid Comparisons in Another Attempt to Eliminate Sample 6

In another attempt to justify the elimination of sample 6 from the creation model, Humphreys *et al.* (2003a, p. 8) state:

> "Our measurements (see Appendix B [in Humphreys *et al*., 2003a]) showed that the helium concentration in the Jemez [*sic*, gneiss] biotite at a depth of 750 meters was small, only about 0.32×10^{-9} cm^3 (at STP) per microgram. Taking into account the difference in density of biotite and zircon (3.2 g/cm^3 and 4.7 g/cm^3), that corresponds to almost exactly the same amount of helium per unit volume as sample 6 contained. That suggests the zircon and biotite were near equilibrium in sample 6, thus supporting our hypothesis."

In the above statements, Humphreys *et al.* (2003a, p. 8) claims that there are similarities between the helium concentration of impure and ground biotites (Appendix B in Humphreys *et al.*, 2003a, p. 19) from a gneiss collected at a depth of 750 meters and their revised helium concentration for the zircons from sample 6 (a different lithology [gneiss with granodiorite intrusions] at 4310 meters depth, Laughlin *et al*., 1983). They then illogically concluded that the biotites from sample 6 must have the same helium concentration as the biotites from the 750-meter sample. Certainly, the helium concentrations of the zircons and biotites in both samples 5 and 6 may be in equilibrium with extraneous background helium; however, how can anyone argue that the helium concentrations of the zircons and biotites in sample 6 are essentially the same on the basis of comparing the amount of helium in the sample 6 zircons at 4310 meters depth with the helium concentration of an impure biotite sample from a different lithology at only 750-meters depth? Again, this approach utterly contradicts the admission in Humphreys *et al.* (2003a, p. 6) that mixing measurements from different lithologies is inappropriate. Dr. Humphreys needs to actually measure the helium concentration in the sample 6 biotites to confirm that they are not even lower. Rather than deal with the irrational statements in Humphreys *et al.* (2003a), Humphreys (2005a) simply refers to the same erroneous sections of Humphreys *et al.* (2003a) and once again appeals to his deceptive Figure 2.

Besides invalidly comparing the helium content of a biotite with the content of the sample 6 zircons at much greater depth, the helium measurements for the biotite and the zircons are far too unreliable to support the efforts in Humphreys *et al.* (2003a, p. 8) to eliminate sample 6 from their models and protect their YEC agenda. As previously discussed, Gentry *et al.* (1982a, p. 1130) admit that there are serious uncertainties about the concentrations and origin(s) of the helium in their samples 5 and 6. Furthermore, information in Appendix B of Humphreys *et al.* (2003a) also raises questions about the suitable purity of the Fenton Hill biotite and the nature of its helium concentration. The scientist that performed the helium diffusion measurements for Humphreys *et al.* (2003a) concluded that there were "multiple sources" of helium in the Fenton Hill biotite:

> "He diffusion in this sample follows a rather strange pattern, with a noticeable curve at intermediate temperatures. I have no obvious explanation for this phenomenon. Because [the Wyoming] biotite BT-1B did not show this curve, I doubt it is vacuum breakdown. I ran more steps, with a drop in temperature after the 500ºC step, to see if the phenomenon is reversible. It appears to be, i.e., the curve appears again after the highest T step, but the two steps (12, 13) that define this curve had very low gas yield and high uncertainties. It is possible that we are dealing with more than one He source (multiple grain sizes or multiple minerals?)."

Humphreys *et al.* (2003a, Appendix B) reasonably conclude:

> "We think it is likely there were some very small helium-bearing zircons still embedded in the biotite flakes, which would be one source. The other source would be the helium diffused out of larger zircons no longer attached to the flakes."

Since the biotite probably contained zircon impurities that produced excess helium and since Gentry *et al.* (1982a, p. 1130) were uncertain about the concentration and origin of the helium in their sample 6 zircons, what justification do Humphreys *et al.* (2003a, p. 8) have for relying on these questionable data to get rid of sample #6 from their models?

A Peer-reviewer of this Essay Uncovers Another Error in a Later Version of Humphreys *et al.* (2003a)

As discussed above, the comparison in the following paragraph from Humphreys *et al.* (2003, p. 8) is invalid for a number of reasons:

> "Our measurements (see Appendix B [in Humphreys *et al*., 2003a]) showed that the helium concentration in the Jemez [*sic*, gneiss] biotite at a depth of 750 meters was small, only about 0.32×10^{-9} cm^3 (at STP) per microgram. Taking into account the difference in density of biotite and zircon (3.2 g/cm^3 and 4.7 g/cm^3), that corresponds to almost exactly the same amount of helium per unit volume as sample 6 contained. That suggests the zircon and biotite were near equilibrium in sample 6, thus supporting our hypothesis."

A scientist that **peer-reviewed** this essay decided to perform the calculations and verify Dr. Humphreys' conclusion that the amount of helium in the Fenton Hill biotite at a depth of 750 meters and the amount in the zircons of R.V. Gentry's sample 6 from a depth of 4310 meters were "almost exactly the same." Here are his calculations based on the description in the above paragraph from Humphreys *et al.* (2003, p. 8) and data from a copy of Humphreys et al. (2003a) at the ICR website:

> *Q* x density (in biotite) = *Q* x density (in zircon)
>
> Before multiplying, the *Q* values from Humphreys *et al.* (2003a) must be converted from ncc STP/μg to cc STP/g so that the units are consistent with the density units of the minerals. The results are then:
>
> Biotite: 0.32×10^{-3} cc STP/g x 3.2 g/cc = 0.001 (unitless)
> Zircon: ~0.02×10^{-3} cc STP/g x 4.7 g/cc = 0.00009 (unitless)

The scientist then wrote the following comments to me:

> "Almost exactly the same???? The numbers are off by over an order of magnitude! I cannot find any mistake in my math, but then again, if Humphreys meant something else, he should have elaborated on his argument in the first place. What is worse, for Humphreys, is the fact that the biotite value is higher than the zircon value (which cannot happen through out-diffusion), which goes back to your point that one cannot claim that the biotite was in equilibrium with the zircon when the two minerals were separated by 3.5 kilometers."

Again, the scientist obtained the *Q* values for the Fenton Hill biotite and R. V. Gentry's sample 6 zircons from an ICR copy of Humphreys *et al.* (2003a), which has a link above and in the references of this essay. However, this is **not** the original version of Humphreys *et al.* (2003a) that was presented at the 2003 International Conference on Creationism (ICC). It turns out that there are **two** versions of Humphreys *et al.* (2003a) in circulation. Besides the version at the ICR website, the original version was released on CD for the 2003 ICC. This earlier version had the original and uncorrected *Q* values from Gentry *et al.* (1982a) (see my Table 1 and discussions above), where the *Q* for the sample 6 zircons was ~0.2 ncc STP/μg. When this original *Q* value is used, Dr. Humphreys' calculation in Humphreys *et al.* (2003, p. 8) is true. Although it's a completely invalid and meaningless calculation (as described above), the amount of helium in the Fenton Hill biotite at a depth of 750 meters and the amount in the zircons of R.V. Gentry's sample 6 from a depth of 4310 meters were "almost exactly the same." However, without giving any proper notification by inserting an errata statement in his paper, Dr. Humphreys changed the *Q* values in Humphreys *et al.* (2003a) after the conference and had the revised version posted on the ICR website. When Dr. Humphreys' "corrected" the *Q* values in Humphreys *et al.* (2003a), which included changing the *Q* value of sample 6 from ~0.2 to ~0.02 ncc STP/μg, his claim was no longer true. As shown above in the scientist's calculations, the results now vary by an order of magnitude. In the process of "correcting" the *Q* values in Gentry *et al.* (1982a), Dr.

Humphreys unknowingly undermined one of his frail arguments for removing sample 6 from his models.

Despite several awkward and erroneous attempts, Humphreys *et al.* (2003a) have not shown that the Q and Q/Q_0 values for sample 6 should be treated any differently than the results for sample 5. They simply have no justification for accepting the results of sample 5 and yet dismissing the sample 6 results to promote their YEC model. Contrary to the approximations in Gentry *et al.* (1982a) and even statements by Humphreys *et al.* (2003, p. 3) that they will "allow for the possibility" that the error on the helium measurement of sample 5 is considerably larger than the errors of samples 1-4, Dr. Humphreys has failed to show that the Q and Q/Q_0 values for sample 5 are quantitative enough to justify their use in his "creation" and "uniformitarian" models.

Consequences of Removing Sample 5 to Dr. Humphreys' 6,000 Year Old "Date"

Finally, in a rash response to my criticisms over his inconsistent treatment of samples 5 and 6, Humphreys (2005a) claims that he could remove sample 5 from his models and that his "dating" results of 6,000 years would remain unaffected:

> "However, we could dispense with both samples [*i.e.,* samples 5 and 6] entirely with no damage to our case at all. This is just another quibble about an inconsequential issue."

However, the mathematics refute Dr. Humphreys' superficial claims. Without sample 5, the dating scheme in Table III of Humphreys *et al.* (2004, p. 8) would only consist of samples 2, 3, and 4 ("dates" of 7270, 2400, and 5730 years). This small dataset would provide an outlandish average "date" of 5,100 ± 5,000 years (2-sigma using the unbiased equation, Davis, 1986, p. 33; Keppel, 1991, p. 43-44, 58). In other words, at 95% confidence and without sample 5, Dr. Humphreys' "date" for the Fenton Hill zircons is worthless even by YEC standards and now spans two orders of magnitude: anywhere from 100 to about 10,000 years. The dire consequences of removing just one sample from his dataset shows how weak Dr. Humphreys' claims really are.

The Real Issue Beyond the Numbers

Although the above calculations and disputes over the accuracy of the values in Dr. Humphreys' documents are important, there is a danger that all of us (including Dr. Humphreys) could get bogged down in these numerical disputes and overlook the even more critical questions about Dr. Humphreys' behavior and claims. Dr. Humphreys needs to answer critical questions about his sloppy methodology and his flippant approach to scientific research and criticism from scientists. That is, why are the claims and numerical results of Dr. Humphreys and his allies so often shown to be wrong when other individuals perform their calculations (*e.g.*, my Appendix A)? Why did he change the text in Humphreys *et al.* (2003a) after the 2003 International Conference on Creationism (ICC) without publicly announcing the changes with an errata statement? Where is the *Creation Research Society Quarterly* article promised in Humphreys

(2005a) that would explain how he obtained a Q_0 of only 15 ncc STP/μg? How can Dr. Humphreys in Humphreys (2005a) claim that any errors in Q and Q_0 would cancel out and not affect his Q/Q_0 values? What valid justification does Dr. Humphreys have for omitting sample 6 from his models, but including sample 5? etc. (see my Appendix C for further questions).

QUESTIONABLE STANDARD DEVIATIONS IN HUMPHREYS *ET AL.* (2004)

What justification does Dr. Humphreys have for using a biased standard deviation?

In Humphreys *et al.* (2004, Table III, p. 8), the "dates" for samples 2-5 (*i.e.*, 7270, 2400, 5730 and ~7330 years) were averaged. Humphreys *et al.* (2004) rounded off the average value of 5,681 years to 6,000 years. Humphreys *et al.* (2004) then list the "date" and "standard deviation" for their creation model as 6,000 ± 2,000 years.

Typically, standard deviations are calculated with a "unbiased" equation, which uses degrees of freedom (*n*-1) in the denominator rather than the total number of samples (*n*) (Davis, 1986, p. 33; Keppel, 1991, p. 43-44, 58). Furthermore, the errors are often given as two standard deviations, which are large enough to include 95% of all theoretical measurements. Such an approach would yield 6,000 ± 4,600 years for the results in Table III of Humphreys *et al.* (2004, p. 8) (**not** ± 4,000 years as stated in Humphreys, 2005a). Instead of utilizing the traditional approach, Humphreys *et al.* (2004, Table III, p. 8) minimized their standard deviation at ± 2,000 years by using the "biased" equation (*n* instead of *n*-1 in the denominator) and only reporting one standard deviation (about 68% of the measurements). This is an old statistical trick that some individuals use to make their errors appear as small as possible. Obviously, Humphreys *et al.* (2004) would rather have their method provide a range with a most recent "creation date" of 2,000 BC instead of 600 AD!

Humphreys (2005a) and his other documents never justify his use of the unconventional "biased" equation to calculate his standard deviations. Humphreys (2005a) simply mentions that he prefers to use one standard deviation rather than two. Certainly, many scientists only use one standard deviation. However, Humphreys (2005b) contains measurements with one and two standard deviations (1 and 2σ). His inconsistent use of one or two standard deviations seems to depend on which approach best serves his YEC agenda. As examples, Figure 13 in Humphreys (2005b, p. 55) uses 2σ, which helps to overlap the diffusion data with the creation model. In contrast, the errors on his high Q/Q_0 values are only given in 1σ, which deemphasizes the errors associated with these values that are "crucial" components of his creation model (Humphreys, 2005b, p. 30).

DR. HUMPHREYS' INACCURATE CLAIMS ABOUT LEAD DIFFUSION IN ZIRCONS: WILL HISTORY REPEAT ITSELF WITH HELIUM?

Lead loss is compatible with ancient zircons.

Using activation energy and diffusion coefficients from Magomedov (1970) (which are listed in footnote 16 of Gentry *et al.*, 1982b), Humphreys *et al.* (2004, p. 10) performed some calculations and claimed that 60-micron long zircons (assuming $a = 30$ microns) from sample 6 should lose about 50% of their lead if they were exposed to 313°C for 1.5 billion years. Because the zircons supposedly have only lost about 10% of their lead (Humphreys *et al.*, 2004, p. 9), Humphreys *et al.* (2004, p. 10) spuriously argue that the zircons must be much younger than 1.5 billion years old.

Lee *et al.* (1997, p. 160, 161) list a more recent activation energy value (161 kcal/mol) and temperature-independent diffusion coefficient (approximately 3.9×10^9 cm^2/sec) for lead in a gem-quality Sri Lankan zircon. The Lee *et al.* (1997) diffusion coefficient is 11 orders of magnitude larger than the measurement in Magomedov (1970), which was obtained on exceptionally metamict (radiation damaged) zircons. Inserting the values from Lee *et al.* (1997) into the same equation used by Humphreys *et al.* (2004, p. 9-10) (that is, Nicolaysen, 1957 in footnote 16 of Gentry *et al.*, 1982b, p. 298) predicts only about 1% lead loss at 313°C over 1.5 billion years rather than a loss of approximately 50% as claimed by Humphreys *et al.* (2004, p. 10). Entering data from another lead diffusion in zircon study (Cherniak and Watson, 2000) into the Nicolaysen equation also predicts about 1% lead loss in the zircons over 1.5 billion years.

A 10% actual lead loss in the sample 6 zircons could be easily explained by metamorphic fluids leaching lead from metamict portions of the zircons (Geisler *et al.*, 2002) and/or prolonged exposure to temperatures well above 313°C sometime in the distant past. Rather than deal with reasonable possibilities, Humphreys *et al.* (2004) used measurements on extremely metamict zircons and made fallacious assumptions, which cause them to erroneously conclude that the lead data are incompatible with an ancient age for the zircons. One must wonder how future studies might change their views of helium diffusion in zircon, especially further studies that use multi-domain models.

Although zircons in the Fenton Hill core may have lost some lead, typically Pb-Pb dates are unaffected (Ludwig *et al.*, 1984; Faure, 1998, p. 288). The masses of the lead isotopes are so similar (204, 206, 207 and 208 atomic mass units [amu]) that loss events would not be able to remove more of one lead isotope than another. As expected, the $^{207}Pb/^{206}Pb$ dates for the zircons in Appendix A of Humphreys *et al.* (2003a) are about 1.43 billion years old, which are consistent with other Fenton Hill results (Brookins *et al.*, 1977).

ENTERING MORE REALISTIC *a*, *b*, and Q/Q_0 VALUES INTO DR. HUMPHREYS' "DATING" EQUATIONS FAIL TO SUPPORT HIS YEC AGENDA (MODIFICATIONS AND CORRECTIONS MADE)

Using the "dating" equations from Humphreys et al. (2003a), the currently best available a, b, and Q/Q_0 values yield a ridiculous average date of 90,000 +/- 500,000 years (2 unbiased standard deviations) for the Fenton Hill zircons.

Introduction: How Realistic are Dr. Humphreys' "Dating" Equations?

In this section, the reliability of Dr. Humphreys' "dating" equations (equations 12-14 and 16) from Humphreys *et al.*, 2003a) are evaluated by entering more realistic ranges of *a*, *b*, and Q/Q_0 values into them. The "dates" derived from Dr. Humphreys' equations reflect the validity of the underlying assumptions of his "creation" and "uniformitarian" models. As discussed below, Loechelt (2008c) uses additional arguments to show that Dr. Humphreys' models and their assumptions are very unrealistic and invalid.

The Helium Diffusion "Dates" in Tables 5 and 6 of my Original Essay

In my original essay, I entered ranges of revised *a*, *b*, and Q/Q_0 values into Dr. Humphreys' "dating" equations to produce a series of "dates" for the Fenton Hill zircons. They were listed in my Tables 5 and 6 of my original essay (refined and corrected results are in Table 4 of this essay). Humphreys (2005a) refers to these "dates" as garbage in, garbage out. I must agree with Dr. Humphreys' observation and I said as much in my original essay. However, if Dr. Humphreys doesn't like the "dates" in the tables of my original essay, he needs to realize that they were derived from **his** equations, **his** incorrect units of measure in Appendix C of Humphreys *et al.* (2003a), corrections to **his** Q/Q_0 values based on data from YEC R. V. Gentry (Gentry *et al.*, 1982b), and more realistic ranges for **his** *a* and *b* values. Dr. Humphreys could avoid a lot of problems with his equations and data, if he would simply listen to his critics and be more careful and thorough with his work.

New Helium Diffusion "Dates" from Entering Improved *a*, *b*, *D*, and Q/Q_0 Values into Dr. Humphreys' "Dating" Equations

Because of the unit error in Appendix C of Humphreys *et al.* (2003a) that I did not notice in my original essay (see above) and my increasing concerns about Dr. Humphreys' Q/Q_0 values and his other results that are required for his "dating" equations in Humphreys *et al.* (2003), I have often recalculated and refined the helium diffusion "dates" of this section in the updates of this essay. In earlier versions of this essay, I used "dating" equations 12-14, 16 and 17 from Humphreys *et al.* (2003a). However, in this latest version, I've decided that since equation 17 is simply part of equation 14, that only equations 12-14 and 16 should be used. In my calculations, the helium diffusion coefficients (*D*) for samples 3 and 5 were taken from measurements in Table III of Humphreys *et al.* (2004, p. 8). The *D* values for samples 1 and 6 were estimated from

Table II of Humphreys *et al.* (2004, p. 6) based on temperatures from Table I (p. 3) of the same document.

As discussed above, Humphreys *et al.* (2003a) and Humphreys *et al.* (2004) failed to properly estimate their *a* values and never provided any suitable standard deviations. Based on the descriptions in Humphreys *et al.* (2003a), Humphreys *et al.* (2004), Gentry *et al.* (1982a), and numerous measurements of Fenton Hill zircons in Heimlich (1976) (see my Appendix B), the best estimates of *a* for any 50-75 microns long zircons in samples 1-6, 2002 and 2003 are probably around 20-30 microns. Loechelt (2008c) further argues that *a* is probably closer to 20 microns. Because no standard deviations are given for the single average *b* value in Humphreys *et al.* (2003a, p. 8) and because the sizes of the biotite grains (*b* values) in the igneous and metamorphic rocks of the Fenton Hill cores could be radically different than the single average measurement provided by Humphreys *et al.*, alternative *b* values of 0.05 cm and 0.30 cm were also used in the equations. In my calculations with equation 14a-c, the *a* values were paired with *b* values in such a way as to obtain a maximum range of possible "dates."

As discussed earlier and as shown in my Appendix B, the Q/Q_0 values in Gentry *et al.* (1982a), Humphreys *et al.* (2003a), and Humphreys *et al.* (2004) are unreliable. Because of the assumptions underlying his Q_0, I also believe that the Q/Q_0 values in Loechelt (2008c) are questionable. Considering the invalidity of the assumptions in Gentry *et al.* (1982a), I would further argue that the corrected Q/Q_0 values in my Appendix A are still not good enough to use. All assumptions considered, Appendix B probably lists the best available results, which only include samples 1, ~3, 5, and 6. If Humphreys (2005a) really believes that "it does not matter in the least to our results whether we call the low-temperature part of the curve a 'defect line' or not" and that my criticisms are a "ridiculous quibble," then he should be willing to allow the results for sample 1 to be entered into his equations. As discussed above, if Dr. Humphreys is willing to derive "dates" for sample 5, he has no justification for objecting to any "dates" from sample 6.

My resulting "dates" with samples 1, ~3, 5 and 6 are listed in Table 4. The average of all of the "dates" in Table 4 is a ridiculous 90,000 ± 500,000 "years" old (one significant digit with two unbiased (n-1) standard deviations) with a range of 200 to 1,700,000 years old. Considering the faulty equations and assumptions in Dr. Humphreys' "creation" and "uniformitarian" models as further shown by Loechelt (2008c), I don't think that any reliable helium diffusion dates are possible with Dr. Humphreys' approach. Furthermore, after viewing the absurd range of "dates" using Dr. Humphreys' methods, YECs have no basis for criticizing the relatively minor problems with radiometric dating.

Table 4. "Dates" for Fenton Hill zircons 1, ~3, 5 and 6 (my Table 1) derived from equations 12-14 and 16 in Humphreys *et al.* (2003a). The Q/Q_0 values are from my Appendix B and the ranges of *a* and *b* values were selected based on arguments in my text. "Dates" for samples 1 and ~3 have two significant digits. To approximate sample 3 from Gentry *et al.* (1982a), chemical data from an analysis of one zircon from Zartman (1979) was used (see my Appendix B). The Zartman zircon was collected within four meters of sample 3 and probably within the same lithology (a biotite granodiorite). Because Gentry et al. (1982a) could only approximate the helium measurements (*Q*) for the zircons of samples 5 and 6, the Q/Q_0 values and resulting "dates" for these samples are even less certain and their "dates" only have one significant digit. The averages and standard deviations also only have one significant digit. See text for details.

				Eq. 12-14a-c		Eq. 16
No.	***a*, cm**	***b*, cm**	Q/Q_0	***D* (cm^2/sec)**	**Age (years)**	**Age (years)**
1	0.002	0.05	0.33	1.00E-17	2,700	2,600
	0.002	0.1	0.33	1.00E-17	2,700	
	0.002	0.3	0.33	1.00E-17	2,700	
	0.002	0.05	0.33	1.00E-18	27,000	26,000
	0.002	0.1	0.33	1.00E-18	27,000	
	0.002	0.3	0.33	1.00E-18	27,000	
	0.002	0.05	0.011	1.00E-17	50,000	77,000
	0.002	0.1	0.011	1.00E-17	50,000	
	0.002	0.3	0.011	1.00E-17	50,000	
	0.002	0.05	0.011	1.00E-18	500,000	770,000
	0.002	0.1	0.011	1.00E-18	500,000	
	0.002	0.3	0.011	1.00E-18	500,000	
	0.003	0.05	0.33	1.00E-17	6,000	5,800
	0.003	0.1	0.33	1.00E-17	6,000	
	0.003	0.3	0.33	1.00E-17	6,000	
	0.003	0.05	0.33	1.00E-18	60,000	58,000
	0.003	0.1	0.33	1.00E-18	60,000	
	0.003	0.3	0.33	1.00E-18	60,000	
	0.003	0.05	0.011	1.00E-17	110,000	170,000
	0.003	0.1	0.011	1.00E-17	110,000	
	0.003	0.3	0.011	1.00E-17	110,000	
	0.003	0.05	0.011	1.00E-18	1,100,000	1,700,000
	0.003	0.1	0.011	1.00E-18	1,100,000	
	0.003	0.3	0.011	1.00E-18	1,100,000	

Table 4. (continued)

				Eq. 12-14a-c		Eq. 16
No.	***a*, cm**	***b*, cm**	***Q/Q₀***	***D* (cm^2/sec)**	**Age (years)**	**Age (years)**
~3	0.002	0.05	0.15	5.49E-17	1,200	1,000
	0.002	0.1	0.15	5.49E-17	1,200	
	0.002	0.3	0.15	5.49E-17	1,200	
	0.002	0.05	0.08	5.49E-17	2,100	1,900
	0.002	0.1	0.08	5.49E-17	2,100	
	0.002	0.3	0.08	5.49E-17	2,100	
	0.003	0.05	0.15	5.49E-17	2,700	2,300
	0.003	0.1	0.15	5.49E-17	2,700	
	0.003	0.3	0.15	5.49E-17	2,700	
	0.003	0.05	0.08	5.49E-17	4,700	4,300
	0.003	0.1	0.08	5.49E-17	4,700	
	0.003	0.3	0.08	5.49E-17	4,700	
5	0.002	0.05	0.003	7.97E-16	2,000	4,000
	0.002	0.1	0.003	7.97E-16	2,000	
	0.002	0.3	0.003	7.97E-16	2,000	
	0.002	0.05	0.0007	7.97E-16	4,000	20,000
	0.002	0.1	0.0007	7.97E-16	4,000	
	0.002	0.3	0.0007	7.97E-16	4,000	
	0.003	0.05	0.003	7.97E-16	3,000	8,000
	0.003	0.1	0.003	7.97E-16	3,000	
	0.003	0.3	0.003	7.97E-16	3,000	
	0.003	0.05	0.0007	7.97E-16	9,000	30,000
	0.003	0.1	0.0007	7.97E-16	9,000	
	0.003	0.3	0.0007	7.97E-16	9,000	

Table 4. (continued)

				Eq. 12-14a-c		Eq. 16
No.	***a*, cm**	***b*, cm**	***Q/Q₀***	***D* (cm^2/sec)**	**Age (years)**	**Age (years)**
6	0.002	0.05	0.002	1.00E-14	200	400
	0.002	0.1	0.002	1.00E-14	200	
	0.002	0.3	0.002	1.00E-14	200	
	0.002	0.05	0.002	1.00E-15	2,000	4,000
	0.002	0.1	0.002	1.00E-15	2,000	
	0.002	0.3	0.002	1.00E-15	2,000	
	0.002	0.05	0.0002	1.00E-14	800	4,000
	0.002	0.1	0.0002	1.00E-14	800	
	0.002	0.3	0.0002	1.00E-14	800	
	0.002	0.05	0.0002	1.00E-15	8,000	40,000
	0.002	0.1	0.0002	1.00E-15	8,000	
	0.002	0.3	0.0002	1.00E-15	8,000	
	0.003	0.05	0.002	1.00E-14	400	1,000
	0.003	0.1	0.002	1.00E-14	400	
	0.003	0.3	0.002	1.00E-14	400	
	0.003	0.05	0.002	1.00E-15	4,000	10,000
	0.003	0.1	0.002	1.00E-15	4,000	
	0.003	0.3	0.002	1.00E-15	4,000	
	0.003	0.05	0.0002	1.00E-14	2,000	9,000
	0.003	0.1	0.0002	1.00E-14	2,000	
	0.003	0.3	0.0002	1.00E-14	2,000	
	0.003	0.05	0.0002	1.00E-15	20,000	90,000
	0.003	0.1	0.0002	1.00E-15	20,000	
	0.003	0.3	0.0002	1.00E-15	20,000	
				Average (1 sig. digit)	80,000	100,000
				2 std. dev.	500,000	400,000

Average (1 significant digit) and 2 standard deviations (1 significant digit) for all "dates" in Table 4: 90,000 +/- 500,000 years.

Now, Dr. Humphreys and his allies might be tempted to view the average "date" of 90,000 years from the creation model to be close enough to support young-Earth creationism and refute "uniformitarianism." However, this value is simply an average of a diverse set of meaningless numbers resulting from Dr. Humphreys' bogus equations, unrealistic models and inappropriate data (also see Loechelt, 2008c). The "dates" in Table 4 that result from using Dr. Humphreys' equations are so poor and scattered that just one unbiased standard deviation easily exceeds the overall average "date" of 90,000 years.

THE REAL THERMAL HISTORY OF THE FENTON HILL SUBSURFACE THAT DR. HUMPHREYS' "ACTS OF GENEROSITY" CAN'T DISMISS

Harrison *et al.* (1986), Sasada (1989) and Loechelt (2008c) clearly refute another major assumption in Humphreys *et al.* (2003a, p. 8), which states that subsurface temperatures at Fenton Hill have been constant over time. I pointed out this invalid assumption in my original essay. Rather than finally dealing with the detailed issues of variable temperatures over time in the subsurface of Fenton Hill (my Figure 5), it is obvious that Dr. Humphreys never adequately read my original essay because Humphreys (2005a) simply repeats his old cliché about being "generous to the uniformitarians":

> "Henke is counting on his readers not to have read my papers carefully enough to know that I considered and discussed all the factors he mentions. I pointed out [*ICC* 2003, section 7] that, 'Our assumption of constant temperatures is generous to uniformitarians.'"

Responding to evidence that temperatures at Fenton Hill were generally lower in the past than the current values, Humphreys (2005b, p. 52) further adds:

> "Thus the long time at lower temperatures would not compensate for high losses during the few million years at higher temperatures. This makes our assumption of constant temperatures at today's values quite favorable to the uniformitarian scenario."

That is, according to Humphreys *et al.* (2003a, p. 10; 2004, p. 8) and Humphreys (2005b, p. 52), without constant temperatures, the "uniformitarian model" would be even worse.

Even ***if*** the thermal history of the Fenton Hill site was unfavorable to the retention of helium in zircons in the "uniformitarian" model, Humphreys (2005a) fails to realize that detailed accuracy is always more important than adopting obviously false assumptions to supposedly be "generous" to your opponents. As I stated in my original essay that Humphreys (2005a) ignored and that *he* is counting on *his* readers not to have read, scientists don't need or want any erroneous "acts of generosity" from him or anyone else. ***If*** Dr. Humphreys was right about a heating problem existing for the "uniformitarian" model, scientists would have had to deal with it realistically and in detail. Nevertheless, as discussed below, a number of researchers have discussed the subsurface thermal history of the Fenton Hill site and the results do not support Dr. Humphreys' YEC agenda. Dr. Loechelt actually shows that the thermal history of Fenton Hill is consistent with the zircons being 1.5 billion years old.

Using $^{40}Ar/^{39}Ar$ dates from feldspars at depths of 1130, 2620, and 2900 meters in the Fenton Hill core samples, Harrison *et al.* (1986, p. 1899, 1901) concluded that the temperatures for these samples fell below approximately 200°C about 1030 million years ago and below about 130°C around 870 million years ago. Again, the closure temperature for helium in zircons is about 200°C (Reiners *et al.*, 2002). Harrison *et al.* (1986, p.

1899) also identified a noticeable thermal event in the Fenton Hill core samples within the past few tens of thousands of years. Whitefield (2008) notes that apatite fission track ages on subsurface samples from Fenton Hill indicate that subsurface temperatures dropped below 125°C at 790 meters about 66.8 million years ago and at 1130 meters about 55.1 million years ago (Brookins *et al.* 1977). The fission tracks are only preserved in apatite once its temperature drops below about 125°C (Whitefield, 2008). Fission tracks in sphene indicate that the temperature of the rocks at depths of 743.4 meters (2439 feet) dropped below about 250°C about 1.3 billion years ago (Brookins *et al.* 1977; Whitefield, 2008).

Figure 5. Thermal history of a granodiorite at 2624 meters depth (Fenton Hill cores) and hypothetical relationships with extraneous helium (based on Figure 9 in Sasada, 1989). Sasada, (1989) does not quantify the time span on the x-axis, but it's probably tens of thousands of years (Harrison *et al.*, 1986, p. 1899). Rather than considering how the thermal and fluid history shown in this diagram might affect his models, Humphreys (2005a) argues that he can ignore this history by claiming that his assumptions were "generous" to his "uniformitarian" model (see text for details).

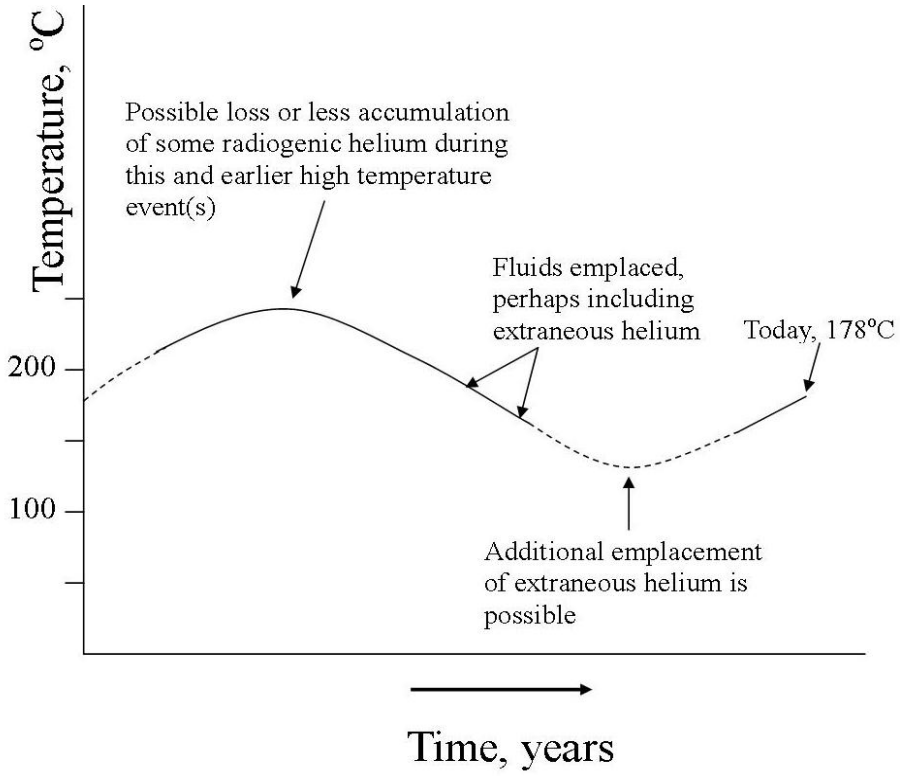


Figure 9 in Sasada (1989, p. 264) shows the variable thermal history of the GT-2 well core at a depth of 2624 meters (compare with my Figure 5). According to Sasada (1989, p. 262-265), a warm period occurred sometime ago. The warm period was followed by a **cooler** event, which included the emplacement of fluids (see my Figure 5). In particular, Sasada (1989) argues that fluids were trapped in secondary inclusions within the granodiorite at depths of 2624 meters when temperatures were at least 26°C **cooler** than present (about 152°C rather than the current value of 178°C). Sasada (1989, p. 265) does not provide any definitive dates for the heating and cooling events, but he argues:

> "The fluid inclusions in the calcite veins and those in quartz of the Precambrian crystalline rocks from the GT-2 indicate heating up to the thermal maximum, cooling and calcite veining, and heating again to the present temperature."

As discussed below, these fluids may have contained extraneous helium that could have contaminated the Fenton Hill zircons and biotites.

Loechelt (2008c) discusses the thermal history of Fenton Hill in considerable detail. By using a "generous" constant temperature for their "uniformitarian" model, Dr. Humphreys and his RATE colleagues unfairly apply higher than normal temperatures to their modeling efforts (Loechelt, 2008c, p. 8-10), which contributes to the failure of the strawperson "uniformitarian" model. Loechelt (2008c) further demonstrates that Humphreys *et al.* (2003a, p. 10) and Humphreys (2005b, p. 52-53) completely misread the time values on the graphs in Kolstad and McGetchin (1978) and Harrison *et al.*, 1986), which contributed to the flaws in Dr. Humphreys' "uniformitarian" model.

Humphreys (2008b) briefly responds to Loechelt (2008c) by claiming:

> "But even assuming (for the sake of argument) his [Loechelt's] lower temperatures, a few hundred thousand years of the laboratory leak rates would wipe out essentially all the helium from the zircons... in contrast to the high amounts observed."

Again, Humphreys (2008b) provides no detailed evidence or calculations to support his arm-waving, whereas the citations and arguments in Loechelt (2008c; 2009a) thoroughly dispute Dr. Humphreys' belief. In particular, Harrison *et al.* (1986) argue that the recent heating event at Fenton Hill lasted for tens of thousands of years, which are an order of magnitude too short for the "few hundred thousand years" in the Humphreys (2008b) speculation. As discussed by Loechelt (2008c), this is further evidence that Dr. Humphreys never carefully read Harrison *et al.*, (1986). Finally, the great uncertainties in the Q/Q_0 values of the Fenton Hill zircons (see above) and the large variations in the uranium and thorium concentrations of the zircons in Gentry *et al.* (1982b) raise serious questions about whether any more than a few percent of the radiogenic helium was actually retained by the zircons (for example, additional zircon analyses might demonstrate that the overall Q/Q_0 value of sample 1 is closer to 0.011 than 0.58 or 0.33). Until Dr. Humphreys and his YEC allies can provide better Q/Q_0 values and thoroughly refute the old-Earth multi-domain models in Loechelt (2008c), which explicitly consider

the thermal history of the Fenton Hill rocks, they have no rational grounds for promoting bogus "uniformitarian" models that are based on unrealistic assumptions.

THE POSSIBILITY OF EXTRANEOUS HELIUM AND DR. HUMPHREYS' INVALID LYELL UNIFORMITARIANISM

Radiogenic, Excess, Inherited and Extraneous Noble Gases

McDougall and Harrison (1999, p. 11) define and differentiate between radiogenic, excess, inherited, and extraneous argon. Using the terminology in McDougall and Harrison (1999, p. 11), analogous definitions may be derived for helium. Radiogenic argon is argon that forms from the decay of ^{40}K and remains in its host mineral or rock. Similarly, radiogenic helium is ^{4}He that results from the decay of uranium or thorium, and remains in its host rock or mineral, including zircons. Excluding any contamination from the atmosphere, excess argon is that component of the gas that has been incorporated into a rock or mineral by processes other than *in-situ* radioactive decay of ^{40}K (McDougall and Harrison, 1999, p. 11). By analogy, excess helium refers to non-atmospheric ^{3}He or ^{4}He that enters a host rock or mineral rather than originating in it. Inherited helium is any radiogenic helium that might somehow remain in a rock or mineral after it has been recrystallized by igneous or metamorphic processes. Extraneous helium is the sum of a mineral's or rock's excess and any inherited helium.

^{4}He largely results from the decay of thorium or uranium isotopes in terrestrial rocks and minerals. Meteorites may also contain trace amounts of helium. Most ^{3}He is primordial and ultimately originates from the mantle. Radioactive tritium is very rare in the Earth's crust, but if it's present, it would decay and produce very small amounts of ^{3}He.

Complications to Helium Diffusion Models if Extraneous Helium is Present

Throughout his documents, Dr. Humphreys claims that the Fenton Hill zircons contain too much helium to be 1.5 billion years old. In response, Loechelt (2008c; 2009a) states that his multi-domain models indicate that Dr. Humphreys' helium diffusion measurements are consistent with the zircons being about 1.5 billion years old. Both Drs. Loechelt's and Humphreys' models, however, would be invalidated if significant extraneous ^{3}He and ^{4}He were present in the zircons. Just as there are methods to detect and correct for the presence of extraneous argon, Dr. Humphreys should be able to detect any significant extraneous helium in his zircons and develop techniques to correct for it (see discussions below).

YECs Only Invoke the Presence of Extraneous Inert Gases When it Benefits Their Agenda

YECs readily accept the existence of extraneous argon in igneous and metamorphic minerals because they improperly believe (see response here) that "undetected excess"

argon nullifies K-Ar and Ar-Ar dating. Because helium atoms are much smaller than argon atoms, helium would tend to more readily move in and out of most minerals than argon. So, if YECs enthusiastically accept the existence of extraneous argon, why shouldn't they acknowledge that subsurface minerals (including zircons) could be substantially contaminated with extraneous helium? The answer is obvious. Extraneous helium is one of many factors that could completely nullify the YEC conclusions of Dr. Humphreys' Fenton Hill zircon study.

Important Comments from R. V. Gentry about Helium Sources

Although Humphreys *et al.* (2003a, p. 3) claim that Gentry *et al.* (1982a) measured the amount of ^{4}He in their samples, Gentry *et al.* clearly give no indication that they distinguished extraneous ^{3}He and ^{4}He from radiogenic ^{4}He in any of their analyses. Simply because of how zircons from samples 1-4 degassed, and especially two groups from sample 4 with relatively large (150-250 microns) specimens, Gentry *et al.* (1982a, p. 1130) thought that **some** of the helium in samples 1-4 (Table 1) was radiogenic:

> "That is, in the two deepest zircon groups (3930 and 4310 m [samples 5 and 6]), we observed only short bursts of He (~1-2 sec) in contrast to the prolonged 20 sec or more evolution of He which was typical of He liberation from zircon groups down to and including 3502 m [samples 1-4]. In fact, it was this prolonged He liberation profile seen in two 150-250 micron size zircon groups from 3502 m [sample 4] which convinces us that **some** residual He is still trapped in the zircons down to that depth (239°C)." [my emphasis]

Clearly, these degassing profiles did not quantify and eliminate the possible presence of extraneous helium in the relatively small (50-75 microns) zircons in samples 1-4, which were used to derive Gentry *et al.*'s Q/Q_0 values. As mentioned earlier, Gentry *et al.* (1982a, p. 1130) even admit that samples 5 and 6 may not contain radiogenic ^{4}He:

> "In fact, at present we are **not** certain whether the minute amounts of He recorded from the deepest zircons (3930 and 4310 m [samples 5 and 6]) are actually residual He in the zircons **or derived from some other source**." [my emphasis]

Again, "derived from some other source" could mean extraneous helium or possibly interferences from the analytical equipment.

Evidence of Open Systems in the Fenton Hill Zircons

Zircons from a biotite granodiorite (Zartman, 1979) and overlying gneisses in the Fenton Hill core (Appendix A in Humphreys *et al.*, 2003a) have discordant U/Pb dates, which indicate open system behavior for lead and/or uranium, and no doubt helium. Open systems not only mean that helium may flow out of zircons, but extraneous helium may periodically flow **into** them. To enter a zircon, extraneous helium need not actually dissolve into the zircon crystalline structure or migrate across the boundary (interface) between a biotite and zircon crystal. The helium could have entered and become trapped

in small fractures, permeable metamict areas and other voids in the zircons that were open even under high subsurface pressures.

Dr. Humphreys' Proposed Field Studies are Unnecessary and his Magmas aren't Needed to Produce Extraneous Helium

Humphreys (2005a) mistakenly believes that any contamination of zircons with extraneous helium would require high temperature "magmatic fluids" and in particular "basaltic magmatic fluids." That is, Humphreys (2005a) erroneously claims that if I want to demonstrate the presence of extraneous helium in the Fenton Hill cores, I need to find "geological evidence that conduits of basalt (solidified volcanic magma [*sic*, by definition, magma is not extrusive]) presently exist within that distance of the borehole." Dr. Humphreys' proposed field studies are completely unnecessary and Humphreys (2005a) wouldn't be making these statements if he had bothered to read the relevant literature and my proposed laboratory studies for detecting extraneous helium in his zircons, which were introduced in my original essay. While magmas can certainly release extraneous helium, extraneous ^{3}He and ^{4}He may also originate from the massive portions of the mantle that are not molten (Goff and Gardner, 1994, p. 1816). Both extraneous ^{3}He and ^{4}He can accumulate in minerals in the upper crust and perhaps eventually escape into the atmosphere (also see Baxter, 2003). Specifically, Manning (2008, p. 1, 65-66) argues that helium in the groundwaters of the Española Basin, which is located just east of Fenton Hill, probably originated from uranium-rich minerals in the northeastern part of the basin and from deeper crustal- and mantle sources. Extraneous helium may further accumulate in hydrothermal ("hot water", but not magmatic) fluids through the leaching of helium from surrounding Precambrian rocks (Truesdell and Janik, 1986, p. 1827). These fluids need not be as hot as magmatic temperatures, which are typically 650°C and higher. For example, the Valles Caldera, which is only a few kilometers from Fenton Hill, **currently** has helium-bearing fluids that are only 260-295°C (Goff and Gardner, 1994, p. 1816). Hydrothermal fluids may also deposit uranium-rich materials in rock fractures, which can be locally important sources of extraneous helium. West and Laughlin (1976, p. 618) even detected uranium deposits in fractures of a biotite granodiorite in the GT-2 core, which might have released extraneous helium into Dr. Humphreys' nearby gneisses.

Even if field studies were to locate evidence of extraneous helium at Fenton Hill, Dr. Humphreys would probably invoke some vague arm-waving excuses to reject the evidence just as he has done with the vast amounts of previous criticism of his work. Instead of proposing superfluous field work, I stated in previous versions of this essay that Dr. Humphreys should have analyzed his zircons for ^{3}He and surrounding low-uranium quartz grains for ^{4}He (also see discussions below). These analyses could quickly determine whether or not his samples are likely to contain extraneous helium.

The Consequences of Extraneous Helium in the Nearby Valles Caldera

Helium-rich gas deposits and groundwaters occur in many areas of New Mexico, including in the Española Basin, which is located just east of Fenton Hill (Manning, 2008). Some of the New Mexico gas deposits have such high concentrations of helium that they are valuable reserves. In the Valles Caldera, which is only a few kilometers away from the Fenton Hill site, fluid samples collected in the 1980s from the Baca test wells contained significant extraneous helium. In 1982, extraneous ^{4}He ranged from 0.0183 cc/kg for Baca-15 to 0.1173 cc/kg for Baca-4 (or 0.0183 to 0.1173 ncc STP/μg) (Smith and Kennedy, 1985, p. 897). According to Goff and Gardner (1994, p. 1816), wells Baca-15 and Baca-4 are greater than 1,000 meters deep and have bottom temperatures of 267°C and 295°C, respectively. In a later article, Truesdell and Janik (1986, their Table 8, p. 1831) report somewhat higher helium concentrations (about 0.2 ncc STP/μg) in Baca wells 13 and 4. The extraneous helium concentrations in at least the Baca 4 well approached or exceeded the helium concentrations that Humphreys *et al.* (2004) list for the zircons in samples 4-6 (my Table 1).

Unless Humphreys *et al.* can thoroughly identify and subtract out any extraneous helium in their zircons and correct the other numerous problems with their work, no one should expect realistic results from their "creation" and "uniformitarian" models. For example, the extremely small Q/Q_0 values predicted by the "uniformitarian" model in Table 5 of Humphreys *et al.* (2003a, p. 12) could be easily masked by extraneous helium concentrations of only 0.01 ncc STP/μg.

Dr. Humphreys' Invalid Lyell Uniformitarianism

In response to the possibility of extraneous helium in the Fenton Hill zircons and biotites, Humphreys *et al.* (2003a, p. 13) states:

> "A second uniformitarian line of defense might be to claim that the helium 4 concentration in the biotite or surrounding rock is presently about the same as it is in the zircons. (Such a scenario would be very unusual, because the major source of ^{4}He is U or Th series radioactivity in zircons or a few other minerals like titanite or apatite, but not biotite.) The scenario would mean that essentially no diffusion into or out of the zircons is taking place. However, our measurements (Appendix B) show that except for possibly samples 5 and 6, the concentration of helium in the biotite [sect. 6, between eqs. (7) and (8)] is much lower than in the zircons. Diffusion always flows from greater to lesser concentrations. Thus helium must be diffusing out of the zircons and into the surrounding biotite."

Humphreys (2005a) makes a similar claim:

> "First, if the helium in the zircons were 'excess' and came from outside them, it would have had to come through the biotite. As I pointed out on p. 9 of *CRSQ* 2004, the helium concentration in the biotite is two hundred times lower than the concentration in the zircon. That means, according to the laws of diffusion, that

> the helium is presently leaking *out* of the zircons *into* the biotite, not the other way around. Also, as I pointed out, the total amount of helium in the biotite is roughly the same as the helium lost from the zircon."

Obviously, Dr. Humphreys has an invalid Lyell uniformitarian mindset that YECs so often accuse scientists of possessing. That is, Dr. Humphreys falsely believes that if the helium concentrations in surrounding biotites are **now** relatively low, then these concentrations must have **always** been low in the past. Dr. Humphreys simply fails to realize that the zircons may have been contaminated with extraneous helium over a prolonged period long ago. While abundant cleavage planes could have allowed extraneous helium to eventually dissipate from biotites in the distant past, the extraneous helium could substantially remain in the relatively impermeable zircons (see further discussions below).

After repeating Dr. Humphreys' invalid Lyell uniformitarian argument, the CreationWiki author(s) adds the following statement:

> "In addition to this Q/Q_0 decreases with depth as predicted by the zircons being the source, on the other hand, contamination would tend to produce the opposite pattern since the deeper zircons would have higher diffusion rates, it would tend to accumulate quicker in those at shallower depths."

This simplistic statement might be true if a homogenous extraneous helium plume ("contamination") rose from the mantle, passed through all of the Fenton Hill rocks and if Humphreys Q/Q_0 values were actually reliable (but, as discussed above, they're not). On the other hand, contrary to this speculation by the CreationWiki author(s), at least the groundwaters at depths down to 700 meters in the Española Basin around Santa Fe, New Mexico, show the opposite helium distribution. Extraneous helium probably originating from the deep crust or mantle is more abundant in the deeper groundwaters (Manning, 2008, his Figure 56, p. 60, along his Table 14, p. 58). Furthermore, as previously discussed, extraneous helium can also originate from uranium deposits that are known to locally occur in fractures within the Fenton Hill rocks (West and Laughlin, 1976, p. 618). So, not all zircons may be equally exposed to extraneous helium and not all of the helium in the zircons may be extraneous. Besides the presence of extraneous helium from deep crustal and mantle sources, the amount of extraneous helium in any zircons would also depend on the uranium and thorium contents, mineralogy and permeability of their host and surrounding rocks. Until Dr. Humphreys becomes responsible and actually measures the $^3He/^4He$ ratios of his zircons and looks for extraneous ^4He in quartz and other low uranium and thorium minerals (see below), no one will know if this is a significant problem or not.

The Wet History of the Fenton Hill Rocks Refutes Dr. Humphreys' Dry Lyell Uniformitarian Thinking

Humphreys (2005a) also improperly believes that fluids that might contain extraneous helium could not have flowed through the rocks of the Fenton Hill cores **in the past**

because they're **currently** "dry and well-consolidated." Once again, Dr. Humphreys shows his fallacious Lyell uniformitarian thinking (that is, because the rocks are **now** dry and impermeable, they must **always** have been dry and impermeable in the past). However, if fluids did not migrate through the Fenton Hill cores sometime **in the past**, why are their fractures often filled with hydrothermal ("hot water") minerals (for example, Sasada, 1989), including uranium-rich materials that can produce extraneous ^{4}He (West and Laughlin, 1976, p. 618)? How did these hydrothermal minerals form under dry conditions? If the subsurface of Fenton Hill was as dry as Dr. Humphreys claims, why are liquid-rich inclusions present in calcite veins at depths of 2624 meters (Sasada, 1989, p. 259)? Also, why do Laney *et al.* (1981) and Laughlin and Eddy (1977, p. 28) admit that the cores were substantially altered by fluids? How does the presence of fluid-altered grains support the undocumented proclamation in Humphreys (2005a) that fluids could not have traveled very far in the Fenton Hill Precambrian rocks because "the interface widths between minerals would be microscopic, perhaps only an Angstrom (the diameter of a hydrogen atom) or so"? Where are Dr. Humphreys' measurements that indicate that these interface widths are this narrow? Even if these widths are currently extremely narrow, how does he know that they were this narrow in the past and how does he explain that presence of fluid-altered minerals in the cores? If the subsurface Precambrian rocks at Fenton Hill were under too much pressure to allow for the diffusion of extraneous helium, why did Manning (2008, p. 1, 65-66) conclude that faults within similar "basement" rocks of the nearby Española Basin could have been suitable conduits for extraneous helium? Also, if Dr. Humphreys is willing to claim that subsurface pressures and "Ångström-wide interface widths" would hinder the flow of extraneous helium, why won't he consider the possibility that these subsurface conditions might also hinder the diffusion of helium from his zircons? To answer these critical and often conflicting questions, Dr. Humphreys needs to stop the arm-waving speculations and actually perform some high-pressure experiments and measure his zircons for possible extraneous helium.

An Extraneous Helium Hypothesis and How to Test It

Dr. Humphreys simply fails to realize that the zircons may have been contaminated with extraneous helium many thousands of years ago. Extraneous helium from the lower crust or mantle may have periodically passed through Fenton Hill in the past just as the gas is currently passing through the nearby Valles Caldera (Smith and Kennedy, 1985; Truesdell and Janik, 1986), parts of the Española Basin (Manning, 2008), and in many other areas of New Mexico (Broadhead, 2006). The presence of uranium deposits in at least part of the GT-2 Fenton Hill core (West and Laughlin, 1976, p. 618) is another potential source of extraneous helium and indicates that at least at one time the Fenton Hill subsurface rocks were far more permeable for uranium-bearing fluids than what Humphreys (2005a) realizes.

Again, Sasada (1989) argues that the Fenton Hill rocks were mineralized by fluids during a relatively **cool** period in the recent past (my Figure 5). During prolonged exposure, extraneous helium could have contaminated biotites, zircons and other minerals. Also rather than always penetrating the zircons, helium pressures surrounding the minerals

may have been periodically high enough **in the past** to temporarily prevent or extensively slow down the escape of any helium from the zircons.

According to Sasada (1989), the cooling event in the subsurface of Fenton Hill was followed by reheating to present temperatures (my Figure 5). During this current reheating event, the cleavage planes in biotites and other micas would have provided excellent pathways for any extraneous helium to largely dissipate as background helium concentrations in the regional crust declined. However, the relatively impermeable zircons could have retained any extraneous helium for a longer period of time, perhaps up to the present. Therefore, instead of just observing the remnants of radiogenic helium in zircons from 1.5 billion years' worth of uranium and thorium decay, Humphreys *et al.* (2003a, 2003b; 2004) might be analyzing significant remaining extraneous helium that contaminated the Fenton Hill subsurface rocks during the relatively cool period in the recent past.

If substantial extraneous helium is present in the Fenton Hill zircons, at least ^{3}He might be identified and appropriate corrections could be made. There are techniques for identifying extraneous ("excess") argon (Hanes, 1991; McDougall and Harrison, 1999, p. 114-130) and analogous methods might be able to identify extraneous ^{4}He. For example, quartz and other impermeable and low-uranium/thorium minerals in his rock samples should be analyzed for extraneous ^{4}He. If extraneous helium occurs in quartz, it's probably also present in adjacent zircons. Dr. Humphreys should also determine the $^{3}He/^{4}He$ ratios of all of the zircons from his and R. V. Gentry's samples. So, before Dr. Humphreys can use his "studies" to promote a religious agenda and overthrow nuclear physics and geochronology, he clearly needs to measure the ^{3}He and ^{4}He values on preferably fresh (not >30 years old) minerals and eliminate any possible effects from extraneous helium.

Rather than seriously considering the presence of extraneous helium and its possible ramifications to his samples, Humphreys (2005a) prematurely concludes:

> "Henke's scenario is pure conjecture. It depends on unknown factors to produce improbable coincidences. Even though this is his best shot (that's why I've spent some time on it), it falls far short of credibility."

Considering the current presence of extraneous helium in the nearby Valles Caldera and the presence of uranium-rich deposits in the Fenton Hill cores, **past** contamination of the Fenton Hill zircons with extraneous helium is certainly not an outrageous hypothesis. It certainly makes more sense than invoking religious miracles to accelerate radioactive decay rates and then relying on even more groundless magic to keep the Earth from melting (see below). So, Dr. Humphreys, I would argue that your miraculous accelerated radioactive decay scenario is pure conjecture. It depends on unknown factors; that is, groundless miracles and bad data. Even though this is your best shot (that's why I've spent some time on it), your reliance on magic falls far short of credibility.

POSSIBLE PRESSURE EFFECTS ON "HARD" SILICATES

Dr. Humphreys Inconsistent Views of Temperature and Pressure

In Humphreys (2005a) and his other documents, Dr. Humphreys frequently invokes the following strawperson argument involving temperature:

> "I further pointed out that the zircons would have to be **colder than dry ice** [Humphreys *et al.*, 2004, p. 9] for most of their history in order to save the 1.5 billion year scenario, and no geologist would consider such a low temperature to be in the realm of possibility." [Dr. Humphreys' emphasis]

Humphreys *et al.* (2003b) base this argument on an extension of their defect curve, whereas the intrinsic curve is more likely to better represent diffusion under subsurface conditions (see below). Rather than attack strawperson fallacies based on unrealistic temperatures, Dr. Humphreys needs to verify that his defect curve and its vacuum helium diffusion data, which he uses in his modeling efforts, accurately represent high-pressure subsurface conditions at Fenton Hill (Figure B). Just as temperatures colder than dry ice do not represent natural conditions on Earth, neither do laboratory vacuums.

Dr. Humphreys' Pressure Assumption Should be Tested

The diffusion results in Dr. Humphreys' studies were obtained in a vacuum of a quadrupole mass spectrometer. These instruments typically operate at vacuums of no more than 0.004 torr or less than about 5×10^{-6} bar. Therefore, the vacuum that was used to produce Dr. Humphreys' results was **at least** 8 orders of magnitude lower than the natural pressures that his zircons experienced in the subsurface of Fenton Hill (depths of 750 - 4310 meters or about 200 to 1,200 bars of pressure; Winkler, 1979, p. 5). A major assumption of Humphreys *et al.*'s work is that helium diffusion measurements obtained under a laboratory vacuum (for example, Appendix C of Humphreys *et al.*, 2003a) are essentially the same as natural diffusion coefficients for the zircons when they were in the subsurface of Fenton Hill. This assumption appears valid in at least some circumstances (*e.g.*, Wolfe and Stockli, 2010), especially if a pronounced defect curve is not involved. However, Dr. Humphreys' modeling efforts are based on defect curves. Dr. Humphreys is assuming that natural pressures of 200-1,200 bars would not have closed or narrowed a significant number of fractures or other defects in his zircons, thereby decreasing the permeability of the zircons and lowering the zircon defect curve in his graph away from his "creation model" and towards his "uniformitarian model" (see my Figure B). McDougall and Harrison (1999, p. 144) remind us:

> "Diffusivity is predicted to decrease as pressure increases as a result of both a drop in number of vacancies in response to the crystal relieving internal pressure and the extra work diffusing atoms must perform against the confining pressure to distort the lattice to make a diffusion jump."

Dr. Humphreys has the burden of proof to demonstrate that his laboratory vacuum diffusion data and the associated defect curve accurately represent helium diffusion under the subsurface pressures of Fenton Hill. That is, Dr. Humphreys needs to stop his arm waving and actually perform some high pressure experiments to justify his assumptions.

The Information in Dunai and Roselieb (1996) that Dr. Humphreys Doesn't Want You to See: High Pressure Experiments Indicate that Helium in "Hard" Garnets Takes 10,000,000's to 100,000,000's of Years to Diffuse Even at Temperatures as High as 700°C

The extensive effects of pressure on helium and argon diffusion in micas and other phyllosilicate minerals are well known in the literature (*e.g.*, McDougall and Harrison, 1999, p. 154 and Dalrymple and Lanphere, 1969, p. 155). Humphreys (2006) attempts to dismiss the relevance of these studies by claiming that micas and other phyllosilicates are not "hard" minerals and that "hard" minerals, like zircon, are incompressible and would not be significantly affected by pressure. However, Dr. Humphreys should not be too quick to dismiss the effects of biotite and other mica minerals on his helium diffusion studies. Whitefield (2008) suggests that very small mineral inclusions and surface coverings were not effectively removed from Dr. Humphreys' zircons and that the helium associated with Dr. Humphreys' defect curve may not have primarily originated from the zircons, but from very small amounts of biotite and perhaps other mineral impurities associated with the zircons.

Even if Dr. Whitefield's hypothesis is an insignificant factor, Dr. Humphreys fails to mention some important results in Dunai and Roselieb (1996). Dunai and Roselieb (1996) concluded that at high pressures of 250 bars, helium would take ***TENS* to *HUNDREDS OF MILLIONS OF YEARS*** even at high temperatures (700°C) ***TO PARTIALLY DIFFUSE*** out of garnets. **Like zircons, garnets are "hard" silicate minerals.** If it takes many millions of years for helium to just partially diffuse out of "hard" garnets at 700°C and pressures of 250 bars, what makes Dr. Humphreys believe that 200-1,200 bars of pressure might not significantly lower the diffusion of helium out of his "hard" zircons? It doesn't take much thought to realize that helium diffusion could be much greater from a rapidly heated, bare and fractured zircon in a laboratory vacuum than a zircon 750 to 4,310 meters in the subsurface encased in other minerals and possibly bathed in extraneous helium over long periods of time.

Dr. Humphreys Initially Ignored Potential Pressure Problems

Despite the clear warnings in my original March, 2005 essay, I had to place the pressure issue prominently in a figure in the abstract of my November, 2005 essay (also Figure B in this version of my essay) before Dr. Humphreys (2006) even took notice. Again, this demonstrates that Dr. Humphreys does not carefully and appropriately consider scientific evidence and discussions from his critics. Instead, he obviously just skims the abstract and prefers insults, flippant "answers," and groundless *ad hominem* innuendo about my former religious beliefs (*i.e.*, Humphreys, 2005a).

In my original March, 2005 essay, I quoted Farley (2002) and Lippolt and Weigel (1988, p. 1454), and I warned Dr. Humphreys that vacuums and other laboratory conditions may not appropriately model the natural subsurface environments of the Fenton Hill site and that he should perform high-pressure laboratory studies that better represent the subsurface pressure conditions at Fenton Hill. In particular, Farley (2002, p. 822) warns that laboratory diffusion data must be carefully applied to natural situations:

> "It is important to note that such laboratory measurements **may not apply** under natural conditions. For example, diffusion coefficients are commonly measured at temperatures far higher than are relevant in nature, so large and potentially inaccurate extrapolations are often necessary. Similarly, some minerals undergo chemical or structural transformations and possibly defect annealing during vacuum heating; extrapolation of laboratory data from these modified phases to natural conditions **may lead to erroneous predictions**." [my emphasis]

Vacuums may also decompose minerals (such as biotites and other micas, and perhaps mica inclusions in zircons) or open fractures, which would allow helium to more readily escape than under natural subsurface conditions. In particular, Lippolt and Weigel (1988, p. 1451) question whether laboratory vacuum experiments adequately model the degassing behavior of certain minerals under natural conditions. These issues must be kept in mind when evaluating Humphreys *et al.*'s models, especially with their biotite data.

Lack of Pressure Data in the Noble Gas Diffusion Literature

Humphreys (2006) believes that the lack of high-pressure noble gas diffusion studies in the literature somehow indicates that pressure is an unimportant variable in helium diffusion. While Wolfe and Stockli (2010) recently demonstrated that helium diffusivities determined by measurements in a laboratory vacuum were able to suitably predict helium retention in their zircons, Dr. Humphreys has yet to show similar results for his defect curve and its associated creation model (my Figure B).

While pressure experiments can be very important in modeling subsurface environments, the literature reminds us that these experiments can be expensive, technically difficult to perform and single runs can take long periods of time to complete. That is, high pressures may slow down diffusion so significantly that it may take weeks or months just to perform one measurement. For example, when Humphreys (2006) refers to the high pressure results in Table 2 of p. 160 of Carroll (1991), he never mentions that some of the runs took almost 65 days to perform. Furthermore, some of the runs performed by Dunai and Roselieb (1996) lasted for 500 hours or nearly three weeks. Dunai and Roselieb (1996, p. 413) also noted that their platinum sample capsules were unable to withstand pressures above 250 bars. Although long-term high-pressure diffusion experiments are difficult to perform, time-consuming and possibly expensive, how else is Dr. Humphreys going to definitively determine whether or not pressure is a relevant parameter in modeling the subsurface conditions at Fenton Hill? Dr. Humphreys must either find some way of properly performing these difficult and potentially expensive experiments or

abandon (at least for now) any claims that he has adequately modeled the diffusion of helium under natural conditions in the subsurface of Fenton Hill.

Humphreys (2006) is on the Wrong Side of the Carroll (1991) Curve

Considering the results of the high pressure helium diffusion studies on "hard" garnets in Dunai and Roselieb (1996), Dr. Humphreys needs to evaluate and discuss how subsurface pressures and long-term exposure to extraneous helium might affect the vacuum-generated **defect** curve that coincides with his creation model (my Figure B). Instead of taking this responsible approach, Humphreys (2006) simply cites some information from a small number of articles that either have absolutely nothing to do with the diffusion of noble gases (helium and argon) in silicate minerals (*i.e.*, self-diffusion of lead in Hudson and Hoffman, 1961) or only apply to noble gas diffusion on high-temperature **intrinsic** curves, which are not relevant to the low-temperature **defect** curve of his zircons and creation model. For example, when Humphreys (2006) refers to the diffusion of argon in the glasses of Carroll (1991, p. 160), he forgets that this reference is dealing with argon diffusion over a relatively small pressure range of 1179 to 3725 bars on **an intrinsic curve**. Unlike Dr. Humphreys' zircons, the bubble-free rhyolitic glass in Figure 4 of Carroll (1991, p. 161) shows no defect curve. Considering the relatively small pressure range and that the glass was free of bubbles and other defects, it's not surprising that the pressure effects in Carroll (1991) are minor, only involve an intrinsic curve, and provide nothing to support Dr. Humphreys' YEC agenda.

Exponential Effects of Pressure and Activation Energy on Diffusion

McDougall and Harrison (1999, p. 144) list the following equation to show the relationships between pressure (P), activation energy (E), and the diffusivity of noble gases in minerals:

$$D = D_0 \, e^{[-(E+PV^*)/RT]}$$

where:

V^* = activation volume
P = pressure
E = activation energy
D = Diffusion coefficient
D_0 = Frequency factor

(Because the diffusivities of Dr. Humphreys' zircons were measured in a vacuum (P~0), the above equation reduces to equation #2 in Humphreys *et al.* (2003a, p. 5): $D = D_0 \, e^{[-(E/RT)]}$).

Pressure-induced strain on zircons could change their activation energies. Notice that because pressure (P) and activation energy (E) are in the exponent of the above equation, even relatively small changes in these variables could lead to huge changes in diffusion

coefficients (D). This is why mathematically enormous changes in diffusivity were seen when Humphreys *et al.* (2003a, Fig. 5, p. 6) fudged the units of measure on the y-axis of the Magomedov (1970) graph from natural to base 10 log (see discussions above). The activation energy nearly tripled to ~40 kcal from Magomedov's listed value of 15 kcal, but the mathematical effects on the diffusion coefficients were even more profound and changed by five orders of magnitude. So, even relatively small or moderate changes in activation energy could lead to orders of magnitude changes in diffusion. Furthermore, when Humphreys (2006) cited Carroll (1991) in his attempts to belittle the importance of pressure, Dr. Humphreys failed to mention that Carroll (1991, p. 161) admitted that his pressure range was **not** sufficiently great to determine how pressure might affect the activation energy of his glasses. Rather than hoping that any pressure-induced changes in the activation energies of his zircons are inconsequential, Dr. Humphreys actually needs to perform high pressure experiments to verify his hopes and defend his creation model.

MORE REALISTIC HELIUM DIFFUSION MODELS IN LOECHELT (2008c) SUPPORT AN ANCIENT EARTH AND RUFUTE YOUNG-EARTH CREATIONISM

Even if pressure and extraneous helium have no significant effect on Dr. Humphreys' results, materials engineer Dr. Gary H. Loechelt (Loechelt, 2008a; 2008b; 2008c; 2009a; 2009b) has recently shown that multi-domain helium diffusion models, which are far more realistic than the "creation" and "uniformitarian" models presented by Humphreys *et al.* (2003a), indicate that the Fenton Hill zircons are about 1.5 billion years old. As mentioned earlier, the values for Q/Q_0, a, and other parameters in Dr. Humphreys' manuscripts are highly questionable and are often known to be erroneous. After recognizing the severe problems with Dr. Humphreys' parameters, Loechelt (2008c) derived his own a and Q/Q_0 values. Although Dr. Loechelt's a of 20 microns seems reasonable, as discussed above, the assumptions underlying his Q_0 values, like those of Gentry, Humphreys, and my appendices, are still questionable. Loechelt (2008c, p. 15) entered his values into four possible helium diffusion models, which included: 1) an old-Earth (1.44 billion years) multi-domain model with a = 20 microns, 2) a RATE-based young-Earth (6,000 years) model, where the conditions used by Dr. Humphreys were applied, including: a single-domain with a = 30 microns for a spherical zircon surrounded by a biotite shell with identical helium diffusion properties, 3) a revised single-domain young-Earth model that contains more realistic parameters, including a = 20 microns, and 4) a multi-domain young-Earth model that also contains more realistic parameters, including a = 20 microns. His results (Figures 7-10 on p. 16 of Loechelt, 2008c; also see the modified graph in Figure A at the top of this essay) show that the old-Earth multi-domain model more accurately matches his Q/Q_0 values and the actual thermal history of the Fenton Hill rocks than any of the three young-Earth models. Loechelt (2008c, p. 15) comments on the results for the first three models:

> "The old-earth model by far has the best agreement to the revised data. The RATE young-earth model seriously *over-predicts* the helium retention at all

> depths. However, once the artificial helium retaining effects of the over-sized geometry and zircon/biotite interface conditions are removed, the revised young-earth model seriously *under-predicts* the helium retention. This observation offers a possible insight into why Humphreys might have revised his value for the spherical radius up from his earlier estimate of 22 microns, and why an unrealistic zircon/biotite interface condition was chosen which was not supported by the diffusion data. A realistic young-earth model has serious difficulties matching the measured data without some artificial assistance." [Dr. Loechelt's emphasis]

By using the words "some artificial assistance", Dr. Loechelt strongly implies that Dr. Humphreys manipulated his models and data to favor young-Earth creationism. Based on how I've seen Dr. Humphreys manipulate the data in Magomedov (1970), his inability to explain the origin of his Q_0 value and his dodging about revealing the details of how and when the "corrections" were made to the Q values from Gentry *et al.* (1982a) (see details above), I think that Dr. Loechelt has a point. There are more than enough suspicious claims, invalid assumptions, and outright errors in Dr. Humphreys' work to reject all of it and to insist that the project be redone from scratch by qualified personnel.

The young-Earth multi-domain model (#4) also overestimates the Q/Q_0 values for the Fenton Hill samples. Loechelt (2008c, p. 17) comments on his young-Earth multi-domain model (#4):

> "With the more retentive multi-domain diffusion model, there is now insufficient thermal budget in a young earth to cause *enough* helium loss. The combination that best fits the measured data is a multi-domain diffusion model in the context of an old earth with multiple thermal events occurring over the last 1.44 billion years."

Although Gentry *et al.* (1982a) and Dr. Humphreys fail to provide adequate a, b, Q, and Q_0 values for **any** modeling efforts, Dr. Loechelt's work demonstrates that the available data do not support young-Earth creationism and are most compatible with the zircons being 1.5 billion years old. Who knows if these diffusion results might change if better data were available? Meanwhile, rather than discussing the results and diagrams in Loechelt (2008c) in any detail, Humphreys (2008b) simply points to his old discredited diagram and spouts his hypocrisy about "peer-review."

HEAT PROBLEMS AND ANY HELIUM IN ZIRCONS REFUTE "ACCELERATED" RADIOACTIVE DECAY

Dr. Humphreys and his allies argue that his helium in zircons study is "evidence" of "accelerated" radioactive decay, presumably during the "Creation Week" and/or "Noah's Flood." Besides vaporizing the zircons and releasing their helium, any acceleration of radioactive decay would release an enormous amount of heat and other radiation that would have fried Noah or created a molten Earth that would have been too hot to plant

the "Garden of Eden" until long after the "Creation Week." Therefore, the very presence of any helium in zircons is incontrovertible evidence that accelerate radioactive decay as advocated by Dr. Humphreys and his allies **never** occurred. Whitefield (2008) notes that even the RATE YECs admit that the terrestrial temperature increase from their "accelerated" radioactive decay would have been about 20,400 degrees Kelvin, or more than three times hotter than the surface of the Sun. Morton and Murphy (2004) also quote YECs that readily admit that they have a "heat problem", including Dr. Humphreys. Humphreys (2000) and Humphreys (2005b) speculated on a "solution" to the YEC heat problem that involve the expansion of space. Morton and Murphy (2004) and Pitts (2009) show that Dr. Humphreys' "solution" is refuted by scientific observations.

DR. HUMPHREYS' OVERRELIANCE ON HIS PRETTY FIGURE AND THE NEW COMPETITION FROM DR. LOECHELT

Instead of adequately dealing with the numerous problems associated with his creation model and the heat that would have been released by the RATE-proposed "accelerated" radioactive decay event(s), Dr. Humphreys simply keeps referring to his pretty diagram (*e.g.*, Figure 2 in Humphreys, 2005a, Figure 3 in Humphreys, 2008b, etc.) and emphasizing the "consistency" between his creation model and his laboratory vacuum helium diffusion measurements. For example, Humphreys (2005a) states:

> "Finally, if I used such poor judgment in choosing the simplifying assumptions for my "6,000 year" model, how did it happen to anticipate the data in Figure 2 so exactly?"

Again referring to his Figure 2, Humphreys (2005a) further issues this irrelevant challenge to me and other critics:

> "This sequence of events places the burden of disproof on the critics, because they must explain how, if there is no truth to our model, the data 'accidentally by sheer coincidence just happened by blind chance' to fall right on the predictions of our model."

Humphreys (2008b) again repeats this tiresome mantra. The YECs of RATE clearly want us to replace a coherent history of the Solar System based on countless valid and consistent radiometric dates and other data, and everything that we know about radiometric decay with Dr. Humphreys' untenable creation model and a good dose of superficial "God did it!s."

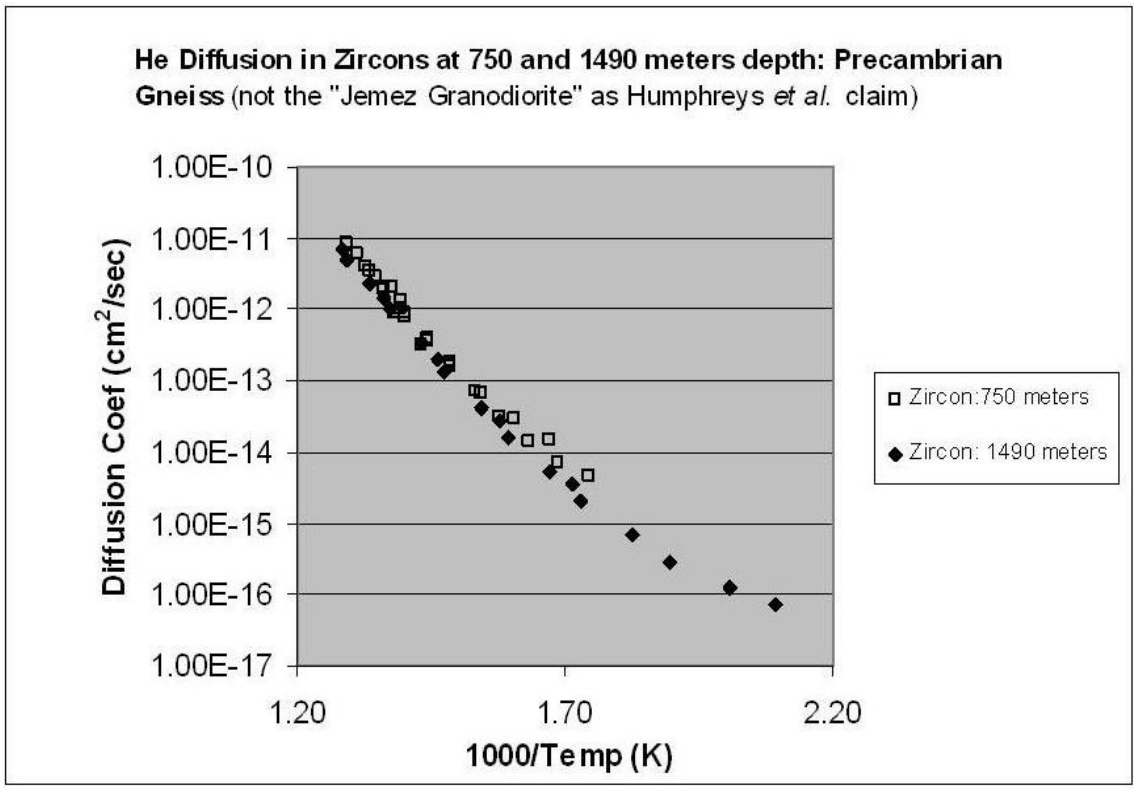
Humphreys (2008b) falsely accuses his critics (including me) of "ignoring" his pretty diagram. In my earlier Talkorigins essays, I repeatedly commented on the errors and oversimplifications of his diagram. It's clearly obvious that Dr. Humphreys can't deal with the numerous questions from his critics (*e.g.*, my Appendix C) and that **his figure is**

the only card that he has left to play. While Dr. Humphreys is obviously overawed by his pretty figure, wise individuals will easily see through this ploy. My Figure 6 shows the actual data (Humphreys *et al.*. 2003a, Humphreys *et al.*, 2004) that was available to Dr. Humphreys. Notice that there is **nothing** in this Arrhenius plot that provides a date for the Fenton Hill zircons. The **only measured** dates for the Humphreys *et al.* zircons are the ancient U/Pb results in Appendix A of Humphreys *et al.* (2003a). The question then becomes why does Dr. Humphreys' creation model happen to align with the vacuum diffusion data? Loechelt (2008c; 2009a) argues that some biased "tuning" was done on the creation model to make it conform to the helium diffusion data, which means that Dr. Humphreys' figure is not the stunning result as proclaimed by Humphreys (2005a) and Humphreys (2008b). Nevertheless, Dr. Humphreys' "6,000 year old date" is based on a lot of questionable and outright erroneous data, including Q/Q_0 values that are often too high, a and b values that are based on missing or inappropriate measurements, improper modeling assumptions as shown in Loechelt (2008c; 2009a), etc. Besides bad data, invalid models and possible manipulation, the consistency between Dr. Humphreys' creation model and the diffusion data may also be partially accidental. John Woodmorappe in Woodmorappe (1999) and other YECs have falsely argued that the numerous consistent radiometric dates in the literature may be nothing more than products of "chance." While the science and economics of J. Woodmorappe's accusations are completely untenable, he might have better luck looking for data manipulation and coincidental relationships in Humphreys (2005a) and associated documents.

Just because his invalid equations and inaccurate data happened to spit out a meaningless number that he likes (6,000), Dr. Humphreys has convinced himself that his conclusions must be "gospel" and he is more than willing to ignore and inappropriately dismiss **any** data or criticisms that expose the fraudulent nature of his "creation date." After all, why should he check his math and assumptions when he thinks that the Bible is telling him that he got the "right and final answer"? This type of blind dogmatism utilized by Dr. Humphreys and his allies is exactly why young-Earth creationism has no place in the laboratory, field research or science classroom. If they happen to pull a number out of the ether that they believe confirms their Biblical interpretations, too many of them (but fortunately not all YECs) will shut their ears to all criticism and nothing can be done to correct them. If Dr. Humphreys is impressed with pretty diagrams, he needs to look at the better figures in Loechelt (2008c, p. 16; also see Figure A at the beginning of this essay), which clearly support an ancient Earth and rival anything that Dr. Humphreys has to offer. Dr. Loechelt's models are thorough and their underlying assumptions are definitely superior to anything that Dr. Humphreys has to offer. Nevertheless, as discussed earlier, possible effects from extraneous helium and high subsurface pressures as well as the unreliable a, b, Q/Q_0 values, and other data that went into both Dr. Humphreys' and Dr. Loechelt's models do not allow **anyone** at this time to definitively determine the age of the zircons on the basis of helium diffusion. However, using Dr. Humphreys' approach to research, an individual could point to Dr. Loechelt's figures and also proclaim: "Finally, if Dr. Loechelt used such poor judgment in choosing the assumptions for his old-Earth model, how did it happen to anticipate the data in his figures so exactly?" and "This sequence of events places the burden of disproof on the

YECs, because they must explain how, if there is no truth to Dr. Loechelt's old-Earth model, the data 'accidentally by sheer coincidence just happened by blind chance' to fall right on the predictions of his model."

Figure 6. An Arrhenius plot of the vacuum helium diffusion measurements from Humphreys *et al.* (2003a) and Humphreys *et al.* (2004). Notice that these data do not provide an age for the zircons. Dr. Humphreys had to develop invalid and oversimplified models based on questionable and erroneous *a*, *b*, and Q/Q_0 values to derive his YEC arguments.



The lesson is clear, depending upon the initial assumptions and how one uses (or misuses) data, anyone can produce a pretty graph to "prove" anything. Contrary to Dr. Humphreys' approach, detailed scientific evidence and not lines on a graph determine the verdict. The evidence presented in this essay, Loechelt (2008c; 2009a) and elsewhere indicates that through biased manipulation, bad assumptions, sloppy *a*, *b*, and Q/Q_0 values, and probably some shear luck, Dr. Humphreys derived a bogus creation model that fits his vacuum helium diffusion data. No one should be awed by Dr. Humphreys' pretty diagram and accept anything he says on faith. Using Dr. Humphreys' own words in Humphreys (2005a), the proper description of Figure 2 in Humphreys (2005a) is "garbage in, garbage out."

DR. HUMPHREYS MISUNDERSTANDS AND MISUSES SCIENCE

Contrary to the misconceptions in Humphreys (2005a), the scientific method is not a matter of my tastes or his, but is based on a set of rules that Dr. Humphreys simply does not understand or follow. All professions have rules and anyone that claims to be a scientist must follow the rules of science. The rules of the scientific method do not allow individuals to manipulate data from the literature and invoke miracles to eliminate scientific data (*e.g.*, U/Pb dates) and questions that they don't like. Because miracles by definition don't obey natural law, are only limited by an individual's subjective imagination, and can be readily remolded to explain away any failures, any models based on the supernatural can never be scientifically evaluated for accuracy or predictability.

Scientists have made enormous advances in the past few centuries. These advances did not develop because scientists relied on miracles or other flimsy excuses to cover up problems and promote religious or political agendas. How much expertise does it take to say "God did it!"? Proclaiming "God did it!" is clearly not an acceptable answer in criminal forensics, weather forecasting, or any other scientific pursuit. Would the invoking of miracles ever be tolerated in a court room, medical school or anywhere else outside of a religious forum? If psychologists don't blame demons for causing manic depression, car mechanics don't blame gremlins for engine problems, and forensics scientists don't invoke witchcraft to solve unwitnessed crimes, what makes Dr. Humphreys believe that geologists should use the supernatural to explain the origin of a rock?

As explained in this essay, it's totally irrational for Dr. Humphreys to rely on faulty data, equations, and magic just to promote a religious agenda to his liking. Because too many YECs are willing to "resolve" any problems or prop up any of their religious ideas with unfalsifiable *ad hoc* miracles, they really don't produce scientific results or models. To be exact, Dr. Humphreys' "accelerated nuclear decay event" is nothing more than an example of the infamous Gosse (Omphalos) and "god of the gaps" fallacies.

Obviously, there are major and critical differences between many YEC "researchers" and real scientists. Real scientists (which include some YECs) pay attention to details in their research, evaluate multiple natural hypotheses, see where their research takes them, and ignore any pronouncements from the *Humanist Manifesto*, the *Bible*, the *Book of Mormon*, or the *Koran*. Just as real Christians would never sign a declaration denying Christ, real scientists would never betray their profession by signing loyalty oaths to the Bible or a set of religious or political doctrines. Authentic scientists also would never allow dogmatic religious or political commissars to dictate to them which of their results are "acceptable" and which are not, yet Dr. Humphreys and other RATE committee members had a Hebrew language scholar looking over the shoulders to make sure that the members "stay on course" (Morris, 2000, p. viii). Unlike authentic scientists, the RATE committee already had their conclusions for an approximately 6,000 year old Earth firmly established in their minds even before their "research" began.

Because YECs like Dr. Humphreys already believe that they have "The Answers" in their Bibles, they tend to take an "ends justify the means" approach to "research," which often leads to careless actions, including: taking unjustified shortcuts (such as, assuming isotropic diffusion in biotites), being inexcusably careless with data (for example, listing the wrong units of measure in Appendix C of Humphreys *et al.*, 2003a), and overlooking alternative natural explanations that conflict with their agenda (as examples, Dr. Loechelt's models or significantly slower helium diffusion under subsurface pressures). After all, getting the "biblically correct answers" and protecting their faith are paramount for these YECs, and Dr. Humphreys proves his sloppy "the ends justify the means" approach every time he flashes his Figure 2 from Humphreys (2005a) and claims that it supports a 6,000 year old Earth.

DR. HUMPHREYS' ACTIONS ARE RELIGIOUS AND NOT SCIENTIFIC

In reaction to my criticisms that he is trying to eliminate sound U/Pb radiometric dates with groundless miracles, Humphreys (2005a) attempts to minimize his religious agenda by claiming that he only spent a few paragraphs in his documents arguing that God miraculously altered radioactive decay rates. That is, Humphreys (2005a) claims that his data are the "main subject" of his work and not the supposed miraculous role of God in accelerating radioactive decay rates. If Dr. Humphreys had really proven that accelerated radioactive decay was a reality, as a scientist, he would be making this discovery the main focus of his work. He would be vigorously confirming his results for a Nobel Prize and looking forward to becoming one of the greatest scientists in modern history. Yet, it is obvious, that he and the other RATE members are only interested in finding enough arguments to confirm their religious convictions and start a revolution in science that could ultimately exalt young-Earth creationism within the scientific community. Furthermore, not even the YEC public buys into Dr. Humphreys' claim that the "main subject" of his work is his data and not the supposed miraculous role of God in accelerating radioactive decay rates. The YEC public doesn't care about the esoteric calculus equations or the long lists of measurements in Humphreys *et al.* (2003a). It's obvious from the countless fundamentalist Christian Internet sites that uncritically cite Humphreys *et al.*'s work that YECs consider the few accelerated radioactive decay paragraphs to represent the very foundation and the most important part of Humphreys *et al.*'s work. The YEC public wants converts and not technical details. They're hoping that Dr. Humphreys' "evidence" for "accelerated" radioactive decay will destroy all radiometric dating methods and bring millions of people streaming to church altars in repentance. Finally, if his data are paramount and the role of God in supposedly accelerating radioactive decay rates is not the "main subject" of his work as Humphreys (2005a) claims, why didn't he first publish a full article in a secular peer-reviewed science **journal** instead of one brief poster at a secular conference and the rest of his writings on this topic being in YEC pamphlets, Sunday school materials, books and magazines that have no widespread respect in the scientific community (*e.g.*, Humphreys, 2003; Humphreys *et al.*, 2004)?

DR. HUMPHREYS' PEER-REVIEW PLOY AND HIS INAPPROPRIATE CHALLENGE: IT'S NOT MY RESPONSIBILITY TO DO DR. HUMPHREYS' WORK FOR HIM

Dr. Humphreys' Peer-review Hypocrisy

Humphreys (2005a) repeatedly challenges me to publish my criticisms of his work in a peer-reviewed scientific journal. In Humphreys (2008b), he repeats his claims against his other critics and me. Contrary to his hopes, the publications of ICR, CRS, AiG, and other YEC organizations have earned no respect in the scientific community. Although YECs consider it unfair, authentic science journals are no more likely to accept a critique of Dr. Humphreys' *Creation Research Society Quarterly* (*CRSQ*) article (Humphreys *et al.*, 2004) than a rebuttal of the astrology columns and Big Foot articles in the *National Enquirer*. Loechelt (2009a) even states that he was told that the *Journal of Chemical Geology* would not accept any manuscript that merely cites articles in the *CRSQ* regardless of the position it takes. Furthermore, before Dr. Humphreys hypocritically screams about the importance of peer-review, he needs to follow his own advice. He needs to openly publish his work and conclusions in a **full article in a legitimate peer-reviewed science journal** (such as *Geochimica et Cosmochimica Acta* or *American Mineralogist*). Since Humphreys (2005a) claims that his data are the "main subject" of his work and not the supposed role of God in "accelerating" radioactive decay rates, he certainly should be able to publish his data and some conservative interpretations in at least one secular peer-reviewed science journal. Peer-review documents don't include YEC publications that are edited by RATE members and other YECs that are more than willing to rubber stamp Dr. Humphreys' manuscripts or to endorse articles that invoke magic to "explain away" scientific problems and questions. Concerning Dr. Humphreys' sole Fenton Hill publication in a secular forum (*i.e.*, Humphreys *et al.*, 2003b), Loechelt (2009a) concludes the following:

> "We are left with the one publication in a truly public forum, the fall meeting of the American Geophysical Union [Humphreys *et al.* 2003b]. 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."

It's obvious from Dr. Humphreys' publication record on this topic (*i.e.*, Humphreys *et al.*, 2003a; 2003b; Humphreys, 2003; Humphreys *et al.*, 2004, etc.) that he has no real interest in fully presenting his ideas as an article in a real peer-reviewed science **journal**, where they can be critically scrutinized by some of the world's authorities on zircon and helium chemistry, and where journal editors won't accept his evasions to serious questions. So, if Dr. Humphreys is really sincere about his devotion to peer-review, let

him wean himself off the reliance on miracles for his "accelerated radioactive decay process", honestly recognize and correct his numerous and serious mistakes, and publish what's left in an authentic peer-reviewed science journal.

Even if I could get a critique of Dr. Humphreys' work published in a peer-reviewed science journal, I would have no interest in doing so. If I did, the editors of the journal would be obligated to provide a forum for Dr. Humphreys to reply. Why should I help Dr. Humphreys get free space in a prestigious journal so that he could simply repeat the errors, insults and evasions of Humphreys (2005a), Humphreys (2006), Humphreys (2008a), Humphreys (2008b), and Humphreys (2010)? That is, why shouldn't I expect Dr. Humphreys to misrepresent many of my criticisms, try to trivialize his serious mistakes, invoke more groundless fantasies like he did with sample #5 and the Magomedov (1970) data, make a couple of minor corrections here and there but otherwise ignore the details, inconsistently pick and choose values from Gentry *et al.* (1982a), attempt to mesmerize readers with his Figure 2 from Humphreys (2005a), emptily promise better answers sometime in the future, and finally hope that his readers swallow his proclamations without wanting to see his math? Also, why should I help Dr. Humphreys add a publication in a prestigious journal to his list of accomplishments and give his work an air of honor and acceptability that it does not deserve?

Talkorigins is Popular and Mainstream

In contrast to peer-reviewed technical journals that have relatively few readers and little space for adequately detailed discussions and calculations, Talkorigins provides a **peer-reviewed** science forum that has a potential audience of millions and no page limits. The readership of Talkorigins is also probably greater than most YEC literature, including *CRSQ*. So, contrary to the claims in Humphreys (2006), the science essays at Talkorigins are extensively read, reviewed and cited, and are not in a "dark corner of the Internet." Many of the peer-reviewers of this essay are professional scientists and some of their names are listed in the Acknowledgements below.

Phony "Peer Review" and Tabloid Quality of the *Creation Research Society Quarterly* and Humphreys *et al*. (2004)

Humphreys *et al*. (2004) was published in the *Creation Research Society Quarterly* (*CRSQ*), a so-called "peer-reviewed" YEC "journal." A careful review of Humphreys *et al*. (2004) shows that Dr. Humphreys and his coauthors extensively responded to criticism from an unknown individual. So, why wasn't this critic's manuscript published in the *CRSQ* or at least referenced if it was published elsewhere? What authentic peer-reviewed science journal accepts articles that respond to unknown, unpublished or unreferenced sources? Why were Dr. Humphreys' responses published, but not the original criticisms? How can any reader really understand and evaluate the validity of Dr. Humphreys' responses without seeing the criticisms? What were the editors of the *CRSQ* afraid of?

It should also be noted that a summary of the final conclusions in Humphreys *et al*. (2004) were previously published in an ICR evangelism flier, Humphreys (2003). There is no indication in Humphreys (2003) that Humphreys *et al*. (2004) had been peer-reviewed and was in-press. Indeed, Humphreys (2003) simply mentions that Dr. Humphreys would be submitting (future tense) an article to the Creation Research Society, which was presumably Humphreys *et al*. (2004). What author publishes a summary of the key findings of his "research" in a layperson's Sunday school publication before it's published by a "peer-reviewed science journal"? What's the point of "peer reviewing" the conclusions in Humphreys *et al*. (2004) when they've already been published in Humphreys (2003)? What would the ICR and Dr. Humphreys have done about Humphreys (2003) if the peer-reviewers of Humphreys *et al*. (2004) had come to the realization that his conclusions were flawed and unjustified? Perhaps, Dr. Humphreys had enough confidence that the editor of *CRSQ* would rubber stamp his results that he didn't worry about an early release of his conclusions in Humphreys (2003). Interestingly, the chief physics editor of *CRSQ*, Dr. Eugene F. Chaffin, also served on the RATE committee. Now, I understand that there are probably not enough YECs with physics PhD's to avoid duplications between the RATE committee and the *CRSQ* editorial staff. Nevertheless, this is a clearly unethical conflict of interest. Questions involving ethics automatically arise about whether Dr. Chaffin accepted Humphreys *et al*. (2004) for publication because he thought that the article was worthy of publication (despite apparently strong criticism from an unknown individual) or whether he did it to promote RATE.

The quality of the *CRSQ* and its "peer-review" system is so bad that even YECs are complaining. Whitmore *et al.* (2007) is a strongly worded letter by YECs from the Creation Research Science Education Foundation (CRSEF). The letter condemns the quality of two articles by Miller *et al.* in the *CRSQ* and the overall quality and "peer-review" system of the "journal." Whitmore *et al.* (2007, p. 268) states:

> "This letter has two objectives. First, CRSEF wishes to distance ourselves from the poor science and apparent association that Miller et al. has to us. Second, we hope that criticism like this will eventually lead to improvement of the quality of articles that appear in the *Quarterly*. It appeared to us that the articles may have been printed without any editing or careful consideration of the contents at all. We realize thAT [*sic*] Miller et al. may have corrections and responses to all of our remarks below, but why weren't these issues caught in the review process?"

On p. 268, the CRSEF authors also noticed that young-Earth creationist authors too often rely on obscure and difficult to obtain references:

> "Furthermore, the major cited work (Kosmowska-Ceranowicz et al., 2001) is in an obscure foreign journal, so it is nearly impossible to check the data's credibility."

Besides the mysterious manuscript from Dr. Humphreys' critic that is unreferenced in Humphreys *et al*. (2004), Humphreys (2008b) is also guilty of citing obscure sources that

are not available to the public, including a critical oral presentation given in Australia by a YEC physicist and a critical and confidential December, 2003 manuscript by an unknown critic (perhaps, the critic mentioned in Humphreys *et al*., 2004). If this December, 2003 manuscript is from the critic referred to in Humphreys *et al*. (2004), then Dr. Humphreys received this critical manuscript in the same month that Humphreys (2003) with its glowing and confident YEC conclusions came out! How is this appropriate science or good peer-review?

Also, on p. 268, Whitmore *et al.* (2007) state:

> "In the authors' argument for global catastrophism and rapid deposition of amber on page 89, they quote Gary Gordon from an unavailable home video. Thus, the reader can make no conclusions about the validity of the remarks."
>
> *In fact, the article has no 'methods' section at all to show what techniques were used.*" [authors' original emphasis]

Again, what legitimate scientific journal contains references to home videos and allows authors to omit discussions of their methodology?

On p. 269, Whitmore *et al.* (2007) further complain:

> "The authors do give brief acknowledgement to CRSEF in the 'Acknowledgements' *but no member of CRSEF ever was aware that Hugh Miller et al. wrote this article, or that the article was 'in press.' None of us ever had a chance to review it. This would only seem appropriate since a significant portion of the funds were supplied by us.*" [authors' original emphasis]
>
> "We believe the Flood was responsible for the Wyoming deposit being studied. However, we are interested in seeing the quality of the *Quarterly* greatly improved. If the quality is going to be improved, articles like this need to be promptly rejected. Only quality research should ever be considered for review and/or publication. If this doesn't happen, the anti-creationists are going to continue to have a heyday with articles like this, and rightfully so."

While Humphreys (2008b) is quick to claim that his critics "avoid" peer-review (including Dr. Gary Loechelt and me), he continues to ignore that fact that my essay at Talkorigins was peer-reviewed by a number of scientists (see the discussions below and the Acknowledgements of this essay for the names of some of the peer-reviewers). In contrast, after being rubber stamped by various YEC publications, the countless flaws in Dr. Humphreys' work are only now being publicly exposed by critical peer reviews from numerous scientists. Rather than providing the necessary details and calculations to defend his claims, Dr. Humphreys continues to dodge criticisms and questions from scientists with insults, shallow one-liners, and empty promises to provide further details in the future (*e.g.*, Humphreys, 2005a).

Humphreys (2008b) is also reluctant to admit that Dr. Loechelt tried to publish his criticisms of Dr. Humphreys' helium in zircon study in the *CRSQ*. Loechelt (2008c, p. 35) openly states his frustration with the *CRSQ* "peer-review" system after submitting an article to them that was critical of Dr. Humphreys' work and claims:

> "Therefore, in an attempt to determine how open their forums are to outside criticism, I submitted an earlier version of this work to one of their own journals: the Creation Research Society Quarterly. Unfortunately, it was ultimately rejected by the physics section editor after a painful, and in my opinion, unfair review process."

Now, an individual might argue that Dr. Loechelt is just crying "sour grapes" because of the rejection of his paper. However, the serious problems with the *CRSQ* "peer-review" system, which include statements by YECs in Whitmore *et al.* (2007), indicate that Dr. Loechelt's protests are valid.

Dr. Humphreys' Inappropriate Challenge to Me

Dr. Humphreys has wasted a lot of time and money to create his mess and he has yet to present any conclusive evidence to support his "creation model." Humphreys (2006) has challenged me to drop my current research projects and perform studies on the Fenton Hill zircons, studies that he should be doing. Dr. Humphreys doesn't seem to realize that he, and not me, has the responsibility to perform **all** of the essential studies (including realistic high-pressure diffusion experiments) before he can promote his "creation model" and make the radical claim that he has overthrown the validity of radiometric dating. Furthermore, as I've repeatedly stated in my previous versions of this essay, all of his mistakes, invalid assumptions, and mystery math must be explained and corrected before any of his claims can be taken seriously by scientists (my Appendix C). Dr. Humphreys has no moral or scientific authority to challenge anyone to perform or publish experiments on this topic until he cleans up his own sloppy data.

In response to Humphreys (2006), it's time for Dr. Humphreys' to remove his claims from the dark corner of young-Earth creationism and into the light of real science, where his work can be critically examined without any protection from dogmatic YEC publishers that suppress criticism and hide or omit the references of critics (*e.g.*, Humphreys *et al.*, 2004; also see Loechelt, 2008c, p. 35). Rather than me seeking any "glory" by doing his work for him, it's more important that Dr. Humphreys overcome his denials, and soberly and responsibly deal with the numerous bad assumptions and errors in his work, which are well documented in my previous essays and summarized in my Appendix C. He can start by finally studying Dunai and Roselieb (1996) and maybe he'll get some ideas on how to measure helium diffusion in zircons at high pressure.

MISCELLANEOUS ISSUES: MORE EXAMPLES OF DR. HUMPHREYS MISUSING SCIENCE AND THE BIBLE

Several other topics are briefly mentioned in Humphreys *et al.* (2003a). Like their other discussions, Humphreys *et al.* make a number of statements that are based on questionable claims and outright errors. Humphreys (2005a) either ignores my criticisms of his earlier claims or replies with further logical fallacies and scientifically and historically inaccurate claims. Some of these topics are discussed below.

Atmospheric Helium: Dr. Vardiman Abandons His YEC Argument

Humphreys *et al.* (2003a, p. 2) briefly discuss the YEC atmospheric helium argument. For years, Vardiman (1990), Melvin Cook (see Cook, 1957) and other YECs have argued that the Earth's atmosphere has too little helium to be billions of years old. However, a review of Vardiman (1990) and other YEC documents show that their arguments are largely based on selective quoting of outdated references from the 1960s and 1970s.

Dalrymple (1984, p. 112) concisely challenged many of the YEC atmospheric helium arguments. He showed that YECs often omitted critical details on various atmospheric helium escape mechanisms, such photoionization. YECs also tended to forget about the impacts of 20th century helium pollution on any attempts to evaluate atmospheric escape mechanisms.

For many YECs, Vardiman (1990) was the authoritative YEC document on atmospheric helium "dating." Even though Vardiman (1990) was written six years **after** Dalrymple's report, significant portions of this 1990 document simply repeated old YEC arguments that had been refuted earlier by Dalrymple. For example, carefully compare the statements in Dalrymple, 1984 (p. 112) with Vardiman (1990, p. 24-25).

More recent studies (such as LieSvendsen and Rees, 1996; Shizgal and Arkos, 1996) provide additional information on helium escape mechanisms, which further undermined YEC arguments on this issue. Nevertheless, the final nail in the coffin of the YEC atmospheric helium argument occurred when NASA satellite images showed helium and other gases being **swept** from the Earth's atmosphere into deep space. One event occurred on September 24-25, 1998 after a solar coronal mass emission (see Solar Wind Blows Some of the Earth's Atmosphere into Space and Solar Wind Squeezes Some of the Earth's Atmosphere into Space).

In response to these recent observations, Humphreys *et al.* (2003a, p. 2) only mentioned that YECs need to review the new data. Dr. Vardiman has obviously reviewed the data because he no longer accepts his YEC atmospheric helium argument. To his credit, Vardiman (2005) states:

> "For several years before the magnitude of the polar wind was determined Vardiman (1990) reported that the lack of helium in the atmosphere argued for a

> young earth. That argument is no longer valid based on the measured and computed escape rate of helium to space in the polar wind."

Vardiman (1990, p. 28-29) also correctly states that YECs should study the atmospheric residence times of heavier gases, like argon, which are less likely to escape into space. However, it is doubtful that such studies would support their Genesis agenda. When compared with ^{36}Ar, the Earth's atmosphere has excess ^{40}Ar (Faure, 1986, p. 66), which is compatible with billions of years of ^{40}K decay in terrestrial rocks (Dalrymple, 1984, p. 83; also see Tolstikhin and Marty, 1998). In contrast, stellar atmospheres have more ^{36}Ar than ^{40}Ar (Krauskopf and Bird, 1995, p. 576), which is consistent with stellar evolution (Faure, 1998, p. 18).

Dr. Humphreys Quoting 2 Peter 3:4 and Refusing to Recognize that Even Some Early Church Fathers and Many Modern Experts Consider it to be a Forgery

Contrary to overwhelming historical and textural evidence, YECs generally assume that all of the books of the Protestant Bible are the infallible "word of God" in at least their original languages. Some of the favorite Bible verses of YECs are the following statements from 2 Peter 3:3-7, which state in the King James Version:

> "**3:3** Knowing this first, that there shall come in the last days scoffers, walking after their own lusts, **3:4** And saying, Where is the promise of his coming? for since the fathers fell asleep, all things continue as they were from the beginning of the creation. **3:5** For this they willingly are ignorant of, that by the word of God the heavens were of old, and the earth standing out of the water and in the water: **3:6** Whereby the world that then was, being overflowed with water, perished: **3:7** But the heavens and the earth, which are now, by the same word are kept in store, reserved unto fire against the day of judgment and perdition of ungodly men."

YECs feel that this is a "prophecy" against "uniformitarianism", despite arguments from old-Earth creationist or more liberal Christian theologians to the contrary. Not surprisingly, Humphreys *et al.* (2003a, p. 4) follow the YEC crowd and also cite 2 Peter 3:4,5-6 as part of their discussions. In reality, however, 2 Peter is probably a forgery written by a 2nd century Christian in response to widespread criticism from non-Christians about the delayed Second Coming of Christ, which was supposed to have been "soon", as claimed in Revelation 1:3, 1 Peter 4:7 and elsewhere in the New Testament. Even if 2 Peter was not a forgery, it does not excuse Dr. Humphreys' illegitimate and sloppy "science."

Rather than discussing the long history of skepticism of 2 Peter, Humphreys (2005a) attacks me with some illegitimate pop psychology and makes the following historically naive statements about criticisms of this New Testament book:

> "The allergy shows itself in his strong objection (just before his conclusion) to my citation of 2 Peter 3:3-7 as a prophecy condemning uniformitarianism. The medication he takes for that malady is (foolishly) to swallow the claim of

> theologically liberal 'higher critics' that 2 Peter is 'probably a 2nd century forgery.' He doesn't seem to see that their reasons for claiming that are specious, motivated by a desire to do away with all the supernatural events of Scripture, such as the virgin birth of Christ. We should not naively accept claims from people (such as Henke himself) with such motives."

Certainly, many skeptics of 2 Peter are liberal Christians or non-Christians. However, Dr. Humphreys' views of the Bible and Church history are no more accurate than his understanding of science or his use of pop psychology to "probe" my motives for opposing his nonsense (see the discussions below). Dr. Humphreys simply handles the Bible and Church history in a simplistic and careless manner. First of all, he incorrectly makes a bogus correlation between rejecting the authority of 2 Peter and opposing the supernatural, including the doctrine of the Virgin Birth. If this correlation is true, why did a significant number of Church Fathers that endorsed the Virgin Birth raise questions about the authenticity of 2 Peter? For example, why did Origen mention doubts about its authenticity? Why did Eusebius (263-339 AD) put 2 Peter on his list of "disputed" New Testament books? Why do many modern Roman Catholic theologians enthusiastically embrace the eternal virginity of the Virgin Mary, but consider 2 Peter to be fraudulent? In particular, the Roman Catholic *The New Jerusalem Bible* endorses the Virgin Birth, but refers to the Petrine authorship of 2 Peter as "doubtful," a "forgery," and its contents "suggest a later date" (p. 1995). As usual, Dr. Humphreys' simplistic black and white view of the world does not represent reality.

Dr. Humphreys and his YEC allies so desire to get as much mileage out of their false interpretations of this forged New Testament book that they can't afford to let the real context or historical facts about the book get in their way. Rather than quoting a spurious book and accusing others of illegitimate uniformitarianism, Dr. Humphreys needs to deal with and not ignore his own Lyell uniformitarian blind spots, which I documented above. He clearly fails to realize that geologists abandoned Lyell uniformitarianism for actualism long ago and that a few sentences from a fraudulent 2nd century manuscript had nothing to do with it. In context, the verses of 2 Peter have nothing to do with geology and because they're probably the words of a forger, not even YECs should take them seriously.

Dr. Humphreys' Aquatic Alchemy and Planetary Magnetic Fields

Humphreys (2005a) continues with his wild interpretations of 2 Peter by making the following proclamation:

> "Last, Henke would not like to hear that I have based a theory on the creation of planetary magnetic fields (Humphreys, 1984) - on part of the passage (2 Peter 3:5) he disparages, and that NASA spacecraft have confirmed the scientific predictions of that theory (Humphreys, 1990)."

Actually, I am very familiar with Dr. Humphreys' "predictions", which do not meet the criteria of scientific theories. Rather than finding his magnetic field "prediction"

disturbing, I find it superficial and ridiculous. The part of 2 Peter 3:5 utilized by Humphreys (1990) states:

> "...the earth was formed out of water and by water."

For Dr. Humphreys, God had no problem invoking a few miracles to keep water liquefied in the vacuum of space and then converting it into everything from hydrogen to uranium. However, does Dr. Humphreys have **any** scientific evidence to support these miraculous claims? Does he have a shred of evidence that all matter in our Solar System magically came from water? As usual, the answers are no.

Humphreys (1990) makes up some stories about God interacting with water molecules and invokes some cursory and inaccurate claims about the inadequacy of modern models of magnetic dynamos. Dr. Humphreys then expects his readers to accept his claims that modern scientific models should be replaced with his magical fantasies. Unlike Dr. Humphreys' aquatic alchemy, Van Allen and Bagenal (1999) present far more coherent and realistic views of what is known and unknown about planetary magnetic fields. Chapter 2 in Faure (1998) and Delsemme (1998) further demonstrate that the distribution of elements in the Universe (including the Oddo-Harkins rule and H/He ratios) is consistent with the Big Bang and nuclear fusion reactions in stars. That is, nuclear fusion reactions in stars, and not Dr. Humphreys' aquatic myths, explain why oxygen-16 is more abundant in nature than oxygen-17 or oxygen-18.

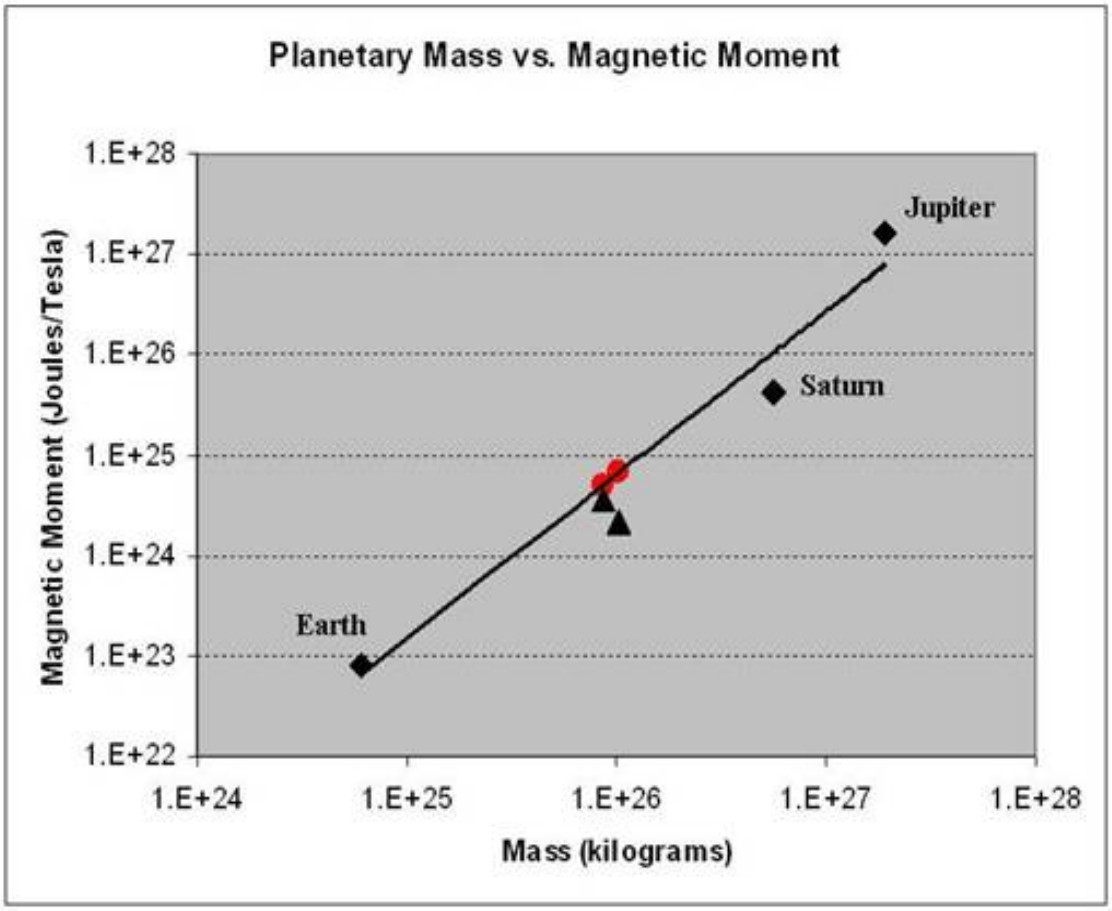
Besides failing to properly distinguish between a scientific hypothesis and a theory, Humphreys (1990) frequently props his "science" on top of groundless and unproven miracles. For example, he states:

> "By the same laws, the currents and fields would preserve themselves with only minor losses, as God rapidly transformed the water into other materials."

In the same sentence, Humphreys (1990) combines two contradictory concepts: natural laws and a supernatural transformation of water by God. Dr. Humphreys is stuck in the old Gosse (Omphalos) Hypothesis trap. How can Dr. Humphreys' "science" ever distinguish between what is a product of nature and what is supposedly supernatural?

Physicist Tim Thompson concisely demonstrates that Dr. Humphreys' aquatic alchemy and its "predictions" amount to nothing. He shows that Dr. Humphreys' equations and variables are so plastic that they could be used to support any planetary magnetic field hypothesis. To be exact, Humphreys (1984) was only willing to "predict" that the current magnetic moments of Uranus and Neptune would be "on the order of 10^{24} J/T" (joules/tesla). These "predictions" are nothing more than obvious guesses that could have been made by anyone that recognized that the magnetic moment of a planet is often related to its mass. Because the masses of Neptune and Uranus are similar to each other and intermediate between the masses of Saturn and the Earth, we would expect the magnetic moments of Uranus and Neptune also to be similar and somewhere between those of the Earth and Saturn.

Figure 7. The magnetic moments in joules/tesla (J/T) versus the masses in kilograms of Jupiter, Saturn and the Earth are plotted as diamonds. The trend line for these three points was calculated and inserted into the graph. The predicted magnetic moments for Uranus and Neptune based on the trend line and their known masses are shown as red circles. The actual measurements are plotted as triangles. The graph shows that anyone could reasonably guess the magnetic moments of Uranus and Neptune by simply knowing their masses and using the trend line. A Bible and Dr. Humphreys' aquatic alchemy are not required.



The magnetic dipole moments in joules/tesla (J/T) and the masses in kilograms of Jupiter, Saturn and the Earth were taken from Tables I and II in Humphreys (1984) and plotted as diamonds in Figure 7. The trend line for these three points was calculated and inserted into the graph. The predicted dipole moments for Uranus and Neptune based on the trend line and their known masses are shown as red circles. According to the trend line, the predicted magnetic moment for Uranus is 5.1 x 10^{24} J/T, whereas the predicted value for Neptune is 6.7 × 10^{24} J/T. The actual measurements from Tim Thompson are very close to my predictions (3.7 × 10^{24} J/T for Uranus and 2.1 × 10^{24} J/T for Neptune) and appear as black triangles in Figure 7. The graph shows that anyone could closely estimate the magnetic moments of Uranus and Neptune by simply knowing their masses and using the trend line in Figure 7. A Bible and Dr. Humphreys' aquatic alchemy are not required.

Because the Bible contains many verses that are not scientifically literal, such as Job 38:37, which claims that God stores rainwater in bottles in the heavens, we should not be surprised when it does not explain the origins of planetary magnetic fields or zircons. As most wise theologians admit, the Bible is not a book of science.

Dr. Humphreys' Personal Attacks: The Failure of his Biblical Pop Psychology

I readily admit that throughout this and my other essays that I am very critical of the Bible and the actions and beliefs of young-Earth creationists (YECs). Humphreys (2005a) refers to my blunt criticism as "mudslinging." Of course, some individuals dismiss any and all criticism as "mudslinging." Considering that my original essay made specific recommendations to allow Dr. Humphreys to improve his work (*e.g.*, look for ^{3}He in the Fenton Hill zircons), that I thoroughly documented my calculations and statements in great detail (as examples, my Appendices A and B) and because of Dr. Humphreys' inexcusable mismanagement of his data and data from the literature (Magomedov, 1970 and Lippolt and Weigel, 1988), I would argue that my comments were justified and not mudslinging. Sometimes we have to use blunt statements when individuals, like Dr. Humphreys, inexcusably misuse data and misrepresent the literature. Nevertheless, one person's mudslinging is another's forthright critique. Unlike the scientific method, "mudslinging" is often a matter of taste or in the words of Dr. Humphreys: "There's no disputing about taste." In contrast, Dr. Humphreys thinks that he can defend his work by simply holding up his deceptive Figure 2 from Humphreys (2005a) and then write a superficial note (Humphreys, 2005a) that largely attacks my former religious beliefs rather than dealing with his mystery math, his invalid Lyell uniformitarianism and other deficiencies in his work. I would argue that if Dr. Humphreys or anyone else wants to really see spiteful mudslinging, desperation, the misusing of references, groundless arguments, and sparse linking to the webpages of opponents out of fear of their arguments, one only has to read Humphreys (2005a) and Humphreys (2006).

A lot of Humphreys (2005a) contains baseless speculation on my former beliefs and current motives for criticizing his work. Dr. Humphrey's mistreatment of science and church history is only exceeded by his outrageous and false accusations against strangers that dare to disagree with his methods and conclusions. If Dr. Humphreys had really

wanted to lecture on my motives for abandoning fundamentalism, he should have read my deconversion testimony in Ed Babinski's: *Leaving the Fold* or emailed me a list of questions. Instead, Dr. Humphreys chose to mix biblical literalism and pop psychology to completely misrepresent me, why I once embraced fundamentalist Christianity, why I abandoned it, and why I oppose his irrational agenda.

Humphreys (2005a) confesses that he relied on "a lot of guesswork" to supposedly understand my motives for criticizing his work. He then proceeds to attack my character on the sole basis of his Bible and a few sentences of my brief review of *Leaving the Fold* at Amazon.com (scroll down). Fundamentalists often build entire doctrines and imaginative scenarios from sentence fragments in the Bible, so I guess that I shouldn't be surprised with how Dr. Humphreys completely mishandled the phrase "after I read the Bible" in my Amazon.com (scroll down) review. Nevertheless, what kind of a person believes that he can know the motives of a stranger by reading a brief book review and using the Bible as a crystal ball?

Using his Bible and pop psychology, Dr. Humphreys even constructs a false biography of my life:

> "So it is possible that Henke did not have enough initial exposure to the word of God to be born "from above" (literal Greek of John 3:3) and merely made a shallow commitment to someone other than the real Jesus Christ—perhaps to a human authority figure, such as a parent, teacher, or pastor. Later on, when he encountered different authority figures, perhaps skeptic professors or persuasive friends, he then transferred his commitment to them, especially since their view was obviously the consensus."

Dr. Humphreys not only misrepresents and distorts scientific data, but he uses the same corrupt and delusional methods to libel the personal lives of his opponents. Contrary to Dr. Humphreys' fables, my conversion and deconversion were completely sincere. When I became a born-again Christian, I was an adult and knew what I was doing. I remained a sincere fundamentalist/evangelical Christian for many years, despite quite a lot of opposition from family members and other people around me. However, it was the Bible, and not any opposition or persuasion from other people that eventually killed my faith. I only abandoned fundamentalism once I read the Bible, thought for myself, and recognized that it was an error-filled book and not infallible scripture. Dr. Humphreys, **who doesn't even know me**, has absolutely no justification telling me or anyone else otherwise. This is YEC arrogance and indecency at its worst.

Obviously, Dr. Humphreys views me as a serious threat. He even refers to me as among the "worst enemies of creationism." In reality, fundamentalism is its own worst enemy because it denies reality or tries to explain it away. Too many fundamentalists don't want to hear and they don't want others to hear that their scriptures are not absolute truth and that for millions of people their religion fails to provide answers for life or deal realistically with death. So, a few frightened fundamentalists will stoop to any libel to discredit people who have honestly been devout Christians and found it wanting. These

fundamentalist extremists search their scriptures and if they find something to slander their opponents, they can always justify using it. They simply assume that whatever their scriptures say, by definition, must be true. Furthermore, when other ex-fundamentalists and I encounter this fundamentalist mindset, there is **nothing** that we can say or do to convince them of the sincerity of our past conversions and subsequent deconversions.

At the same time, I want to stress that I know YECs, old-Earth creationists and theistic evolutionists that are superb scientists and fair people. Also, while most Christians (including many of my fundamentalist friends) can separate faith from science and place love above dogma, too many YECs and other fundamentalists cannot. As an ex-fundamentalist, I know that some of them live in constant fear that if only one verse in their Bible is shown to be inaccurate or if they can't discredit the next fossil in *Science*, their entire faith will collapse. This is a pitiful way for any individual to live – full of terror and desperately seeking any excuse to smear science and the actions of anyone that dares to see the world differently. Rather than calling on people to live better and more loving lives, individuals like Dr. Humphreys set a terrible example by distorting data and the lives of former believers that now sincerely disagree with them. The spite and contempt that some YECs have for former believers demonstrate the ineffectiveness of their false religion to change the lives of these individuals for the better. They are the 21st century Pharisees that value fake science and illegitimate scriptural interpretations more than love and compassion for individuals and a search for truth and accuracy. No wonder, countless individuals that were once with them, now walk away and millions more shun them.

I did not write this or my earlier essays because of any delusional speculations in Humphreys (2005a) that I was trying to reassure myself that I was correct in rejecting the myths of the Bible many years ago. I wrote it out of anger and not fear. Dr. Humphreys and his allies have repeatedly attacked my profession and attempted to exploit geology to deceive others. Geologists work hard. We get oil and ores from the Earth to support our civilization. We develop and test technologies to cleanup soils and water. Over the past 200 years, we've developed a coherent and consistent view of Earth history. Rather than being thanked for deciphering the Earth's history, too many YECs spit in our faces and tell us that historical geology is no better than a myth. They open their Bibles and without really looking at the Earth proclaim **that they know better**. They then spread their delusions and slanders into the general population. My anger is not with the vast majority of Christians (including many fundamentalists and even some YECs) that lead morale lives, properly recognize and criticize evil when they see it, and encourage others to be loving and honest. My battle is with arrogant know-it-alls that claim to speak for God, try to tell me how to do my work, and then attempt to hijack my profession and the other sciences for their religious or political agendas. This not only includes YECs like Dr. Humphreys, but also astrologers, water witches, advocates of petroleum conspiracies, and medical quacks. Nobody likes their noble profession degraded. I'm simply tired of being told how to do my research by a bunch of ignorant individuals that have never left the 17th century and vainly attempt to use Bibles to analyze outcrops.

CONCLUSIONS

Despite enthusiastic endorsements by numerous YECs, Dr. Humphreys' "helium diffusion studies" are based on many flawed arguments, invalid assumptions, untrustworthy equations, and questionable data. Dr. Humphreys has had more than five years to make a thorough and air-tight case for his claims. As explained in this and my earlier essays, he has wasted a lot of time and money, and has utterly failed to do so.

Rather than properly deal with the numerous problems in his work, Humphreys (2006) manipulates and cites irrelevant literature. Humphreys (2005a) prefers insults to explanations and, in particular, he refers to my detailed criticisms as "a torrent of hot air." It is also obvious that Humphreys (2005a) never bothered to read or understand most of my criticisms or references because he frequently keeps making the same erroneous statements over and over again even though I thoroughly documented and refuted them in my original essay (*e.g.*, refusing to recognize the presence of gneisses in his samples, failing to recognize possible contamination of his zircons with extraneous helium during cooling and not heating episodes, ignoring my Appendix B and its more realistic Q/Q_0 results, using the wrong ("biased") equation to calculate standard deviations, etc.). Rather than providing thorough answers, Humphreys (2005a) exposes even more inadequacies in his laboratory methods (such as, trying to identify rocks by merely relying on naked-eye observations, improper naming of rock units, sloppy handling of units of measure in Appendix C of Humphreys *et al.*, 2003a, etc.). In his more recent responses in Humphreys (2008a), Humphreys (2008b) and Humphreys (2010), Dr. Humphreys still fails to explain or defend his actions. My Appendix C has a long list of questions that Dr. Humphreys needs to answer, but has not. Dr. Loechelt and other critics of Dr. Humphreys' work also have pertinent questions. Dr. Humphreys needs to answer all of these questions before his claims can be taken seriously by scientists.

While Dr. Humphreys often pontificates without providing any details and inaccurately accuses me in Humphreys (2005a) of trying "to bury truth under a mountain of minutiae," I have nothing to hide and I've documented in great detail that his studies are full of numerous holes that completely undermine the credibility of his work. In particular, I show that Dr. Humphreys' miracle-based misconceptions about the ages of the Fenton Hill rocks are probably due to him severely underestimating the Q_0 values and the amounts of uranium and thorium in his zircons, assuming that his defect curve and its creation model adequately represent the high-pressure subsurface conditions at the Fenton Hill site, and/or ignoring the possibility of extraneous helium contamination in his zircons. In response, Dr. Humphreys in Humphreys (2005a) thinks that he can just read through my abstract, throw out some insults, try to trivialize his serious mistakes, invoke a few more groundless fantasies, make a couple of corrections here and there in Gentry *et al.* (1982a) without explanation, ignore the details, promise great things in the future, repeatedly rely on his deceptive Figure 2, and then hope that his readers will just go away on faith. Well, science doesn't work that way and Dr. Humphreys should know better.

Recently, Loechelt (2008c) and other scientists and engineers have raised additional questions and uncertainties about Dr. Humphreys' "creation" and "uniformitarian

models." Using more realistic models, Loechelt (2008c) was able to show that the current diffusion-related data for the Fenton Hill zircons, although far from perfect, are consistent with the zircons being about 1.5 billion years old. Therefore, RATE has wasted its time and money since Dr. Humphreys' current data and equations provide absolutely no evidence of "accelerated radioactive decay" and scientists have no reason to throw out some fundamental laws of physics and their associated radiometric dating methods. To be exact, the U/Pb methods are still the best alternatives for dating the Fenton Hill zircons (*e.g.*, the Pb/Pb results in Appendix A of Humphreys *et al.*, 2003a).

Instead of dealing with his numerous scientific mistakes, Dr. Humphreys makes up stories in Humphreys (2005a) to attack my sincerity and personal beliefs. While I've had gracious, but frank, discussions with Dr. Guy Berthault, Dr. David Plaisted, Dr. John Baumgardner and many others that strongly disagree with my views, Humphreys (2005a) and his subsequent documents are not science and represents the worst of young-Earth creationism.

ACKNOWLEDGMENTS

Internet comments on Humphreys *et al.* (2003a), Humphreys *et al.* (2003b) and Humphreys (2003) by "WeHappyFew", Dr. Joseph G. Meert, and Jack DeBaun were exceptionally insightful. Dr. John Baumgardner kindly provided helpful information on Dr. Humphreys' samples. A number of scientists and other individuals peer-reviewed and provided valuable comments on various versions of this essay, including: Tom Baillieu, Tom Bridgman, John Brawley, Paul Heinrich, Mike Hopkins, Mark Isaak, Mark D. Kluge, Ted Lawry, Frank Lovell and several anonymous scientists and other individuals.

APPENDIX A: CALCULATION OF Q/Q_0 VALUES USING THE ASSUMPTIONS IN GENTRY *ET AL.* (1982a)

Q refers to the measured quantity of helium (presumably only radiogenic ^{4}He) in a mineral. Once a mineral is below its helium closure temperature, Q_0 is the **maximum** amount of radiogenic helium (^{4}He) that is expected to accumulate in the mineral from the radioactive decay of its uranium and thorium. A certain percentage of alpha particles (^{4}He nuclei) will escape from the host mineral during radioactive decay and this loss is normally considered when calculating the Q_0 values. Q/Q_0 would then represent the fraction of radiogenic ^{4}He (that is, presumably without any extraneous component) remaining in a sample. The Q/Q_0 value of a zircon would not only depend on its age, but also on its size, the number of fractures and metamict areas, its original uranium or thorium concentrations, subsurface temperatures and other conditions, and a number of other factors.

By making several assumptions that are no doubt inaccurate, Gentry *et al.* (1982a, p. 1129) derived one Q_0 value for the zircons in all of their lithologically diverse Precambrian samples and used this value to estimate the Q/Q_0 values of their zircons. Gentry *et al.* (1982a, p. 1129) state their assumptions in the following paragraph:

> "For the other zircons from the granite [*sic*, granodiorite] **and gneiss cores** [samples 1-6], we made the assumption that the radiogenic Pb concentration in zircons from all depths was, on the average, the same as that measured (Zartman, 1979) at 2900 m, *i.e.*, ~80 ppm with ^{206}Pb/^{207}Pb and ^{206}Pb/^{208}Pb ratios of ten (Gentry *et al.*, ...[1982b]; Zartman, 1979). Since every U and Th derived atom of ^{206}Pb, ^{207}Pb, and ^{208}Pb represents 8, 7 and 6 alpha-decays respectively, this means there should be ~7.7 atoms of He generated for every Pb atom in these zircons." [my emphasis. Also, unlike Humphreys (2005a), Gentry *et al.,* 1982a admit that the Fenton Hill cores contain gneisses.]

First of all, Gentry *et al.* (1982a) assumed that the radiogenic lead concentrations (total ^{206}Pb, ^{207}Pb, and ^{208}Pb) of the zircons from each of the six samples averaged 80 parts per million (ppm). Therefore:

80 ppm = 80 micrograms radiogenic Pb/gram zircon = 0.00008 g radiogenic Pb/g zircon

Although the overall atomic mass of Pb (207.2 amu) includes non-radiogenic ^{204}Pb, the atomic mass of radiogenic Pb is close to 207.2 amu. Therefore:

0.00008 g/g divided by 207.2 g Pb/mole Pb = 3.9×10^{-7} moles radiogenic Pb/g zircon

The concentrations of the various radiogenic lead isotopes are then represented by the following equation:

^{206}Pb + ^{207}Pb + ^{208}Pb = 3.9×10^{-7} moles total radiogenic Pb/gram zircon

Given:

$^{206}Pb/^{207}Pb = 10$. That is: $^{207}Pb = {}^{206}Pb/10$. This assumption by Gentry *et al.* (1982a) is reasonable. Actual values from Gentry *et al.* (1982b, p. 296) are about 9.6 to 11.2.

$^{206}Pb/^{208}Pb = 10$. That is: $^{208}Pb = {}^{206}Pb/10$. This assumption by Gentry *et al.* (1982a) is more questionable. Gentry *et al.* (1982b, p. 296) has actual values as low as 3.1 and as high as 14.

Combining these equations and using some algebra:

$^{206}Pb + {}^{206}Pb/10 + {}^{206}Pb/10 = 3.9 \times 10^{-7}$ moles/g

Multiplying everything by 10:

$10(^{206}Pb) + {}^{206}Pb + {}^{206}Pb = 3.9 \times 10^{-6}$ moles/g

$12\ (^{206}Pb) = 3.9 \times 10^{-6}$

$^{206}Pb = 3.25 \times 10^{-7}$ mole/g

Then: $^{207}Pb = {}^{208}Pb = 3.25 \times 10^{-8}$ mole/g

Gentry *et al.* (1982a, p. 1129) state:

> "During the decay of uranium and thorium, every ^{206}Pb, ^{207}Pb, and ^{208}Pb atom has 8, 7, and 6 alpha-decays, respectively."

Therefore:

Total radiogenic ^{4}He produced with the radiogenic Pb:

Total radiogenic $^{4}He = 8(^{206}Pb$ in moles) + $7(^{207}Pb$ in moles) + $6(^{208}Pb$ in moles)

Total radiogenic $He = 8(3.25 \times 10^{-7}) + 7(3.25 \times 10^{-8}) + 6(3.25 \times 10^{-8}) = 2.60 \times 10^{-6} + 2.275 \times 10^{-7} + 1.95 \times 10^{-7} = 3.02 \times 10^{-6}$ moles/g

There are 10^{9} nanomoles in one mole.

Total radiogenic $He = 3.02 \times 10^{-6}$ moles/g x 10^{9} nanomoles/mole = 3020 nanomoles He/gram of zircon.

Converting to Humphreys *et al.*'s scale of cubic centimeters (Standard Temperature and Pressure [STP]) of radiogenic He/microgram zircon requires the following steps:

Gas laws state that at standard atmospheric temperature and pressure (STP) 1 mole of every gas has a volume of 22.4 liters:

22.4 liters = 22,400 milliliters (ml)

1.0 ml = 1.0 cubic centimeter (cc)

Therefore: 22.4 liters = 22,400 cc

Total radiogenic He = 3020×10^{-9} moles/g × 22,400 cc STP/mole = 6.8×10^{-2} cc STP/g

There are 10^6 micrograms in one gram. Therefore:

6.8×10^{-2} cc STP/g divided by 10^6 micrograms/g = 6.8×10^{-8} cc STP/microgram

Gentry *et al.* (1982a, p. 1129-1130) argue that up to 30-40% of the radiogenic helium is lost by alpha ejection. For a 40% loss:

60% of 6.8×10^{-8} cc STP/microgram = 41×10^{-9} cc STP radiogenic He/microgram (μg) zircon = 41 nano cubic centimeters (ncc) STP/μg = Q_0

Similarly, Loechelt (2008c, p. 5) concluded that the assumptions in Gentry *et al.* (1982a) would yield a Q_0 of about 40 ncc STP/μg. This value is more than twice as large as the Q_0 value of approximately 15 ncc STP/μg endorsed by Humphreys *et al.* (2004, p. 9).

Utilizing the measured helium concentrations (Q values) listed in Humphreys *et al.* (2003a, p. 3), Table A shows the Q/Q_0 values that Humphreys *et al.* (2003a) should have obtained by correctly using the assumptions in Gentry *et al.* (1982a). The use of a 30% alpha ejection would lower the Q/Q_0 values even further. Nevertheless, chemical data in Gentry *et al.* (1982b) and Zartman (1979) indicate that the values in Table A are unreliable (compare Table A with the diverse results in my Appendix B and Table 2). The assumptions in Gentry *et al.* (1982a) are no doubt inaccurate and it is improper to apply just one Q_0 value to all of the lithologically diverse Precambrian Fenton Hill samples, especially when the chemical analyses in Gentry *et al.* (1982b) indicate highly variable uranium and thorium concentrations even within single zircons.

Rather than accepting that the assumptions in Gentry *et al.* (1982a) do not support a Q_0 value of 15 ncc STP radiogenic He/microgram zircon or his high Q/Q_0 values, Humphreys (2005a) attempts to salvage his high Q/Q_0 values by claiming that there are additional "misstated" numbers in Gentry *et al.* (1982a) related to the alpha ejection percentages:

> "In his Appendix A Henke derives his value for $Q0$, 41 ncc/μg (1 ncc = 1 "nano-cc" = 10^{-9} cm^3 at standard pressure and temperature, STP). He is in the right ball park, but he is probably using too small a value for the percentage of alpha particles (helium nuclei emitted by the nuclear decay) escaping the zircons. The

percentage came from Gentry's paper, but Gentry may have misstated what he meant by the number."

Certainly, there are plenty of questionable assumptions and unreliable numbers in Gentry *et al.* (1982a). However, if the 40% alpha ejection values of Gentry *et al.* (1982a) are too small as Humphreys (2005a) claims, why should we accept any other statements in Gentry *et al.* (1982a)? Why is Dr. Humphreys still willing to trust the Q/Q_0 values in Gentry *et al.* (1982a) after he's admitted that almost every other datum in this paper is a "typo" or "misstated" number? When will the list of errors in Gentry *et al.* (1982a) end?

Table A: Q/Q_0 values for zircons in the Precambrian Fenton Hill, New Mexico well cores as they should appear in Humphreys *et al.* (2003a, p. 3) if all of the calculations using the assumptions in Gentry *et al.* (1982a) were correctly followed.

No.	**Depth (m)**	**Revised He concentrations (*Q*) in Humphreys *et al.* (2003a) (ncc STP/microgram)**	**Humphreys *et al.*'s Q/Q_0 ±30% (using Q_0 = 15 ncc STP/microgram)**	**My calculated Q/Q_0 using the assumptions in Gentry *et al.* (1982a)**
1	960	8.6	0.58	0.21
2	2170	3.6	0.27	0.088
3	2900	2.8	0.17	0.068
4	3502	0.16	0.012	0.0039
5	3930	~0.02	~0.001	~0.0005
6	4310	~0.02	~0.001	~0.0005

APPENDIX B: CALCULATION OF MORE REALISTIC Q_0 VALUES AND ESTIMATIONS OF Q/Q_0 VALUES FOR INDIVIDUAL ZIRCONS FROM SAMPLES 1, ~3, 5 AND 6 USING CHEMICAL DATA FROM GENTRY *ET AL.* (1982b) AND ZARTMAN (1979) (CORRECTIONS MADE)

Gentry *et al.* (1982b) list chemical data for individual zircons taken from depths of 960, 3930 and 4310 meters in the Fenton Hill cores (samples 1, 5 and 6 in Gentry *et al.*, 1982a). Zartman (1979) also contains a uranium and thorium analysis on a zircon that was collected within four meters of sample 3 from Gentry *et al.* (1982b) and probably within the same rock unit (a biotite granodiorite; Table 2). These data allow the Q_0 values at the four depths to be better estimated than simply utilizing the generic values that were calculated for samples 1-6 by Gentry *et al.* (1982a) (15 ncc STP/µg according to Humphreys *et al.*, 2004, p. 9, or 41 ncc STP/µg from Appendix A of this report). The Q_0 values calculated in this appendix may then be used to roughly estimate the range of possible Q/Q_0 values for the four samples.

Zartman (1979) lists the total uranium and thorium concentrations of zircon from a depth of 2903.8 meters. The uranium concentration is 328.78 mass parts per million, or micrograms of uranium per gram of zircon (µg/g), whereas thorium is 169.42 µg/g. Thanks to an astute peer-reviewer, I now recognize that the uranium and thorium concentrations in Gentry *et al.* (1982b) are in **atomic** parts per million and not **mass** parts per million. **Therefore, the calculations in Appendix B of earlier versions of this essay are wrong.** Table B1 shows the range of uranium and thorium concentrations for seven different zircons from samples 1, 5 and 6 of Gentry *et al.* (1982b, p. 296). The letters associated with the Gentry *et al.* (1982b) sample numbers in Table B1 represent different zircon specimens that were analyzed from each depth.

Table B1: Uranium and thorium atomic parts per million concentrations of seven zircons from the Fenton Hill well cores as stated in Gentry *et al*. (1982b). The analyses for the different zircons are numbered according to the scheme in Gentry *et al.* (1982a). Letters are used to distinguish different zircons from the same depth.

Zircon ID	Depth (m)	U (atomic parts per million)	Th (atomic parts per million)
1A	960	240 - 5300	800 - 2000
1B	960	465 - 1130	220 - 750
1C	960	1250 - 3300	100 - 275
5A	3930	83 - 220	63 - 120
5B	3930	90 - 110	60 - 90
6A	4310	110 - 550	63 - 175
6B	4310	125 - 210	40 - 85

Typically, Gentry *et al.* (1982b) performed four pairs of uranium and thorium analyzes on each zircon. Gentry *et al.* (1982b) noticed that the uranium and thorium concentrations varied considerably even at different locations on the same zircon grain. When calculating the concentrations, Gentry *et al.* (1982b) assumed that the zircons were pure $ZrSiO_4$. Although zircons typically contain 1-4% hafnium (Klein, 2002, p. 498), this assumption is probably reasonable.

The calculations in this appendix were performed on a Microsoft Excel™ spreadsheet. These calculations assume no uranium or thorium addition or loss in the zircons over time. Tables B2-B7 show the calculations of the Q_0 values for the zircon from Zartman (1979). For this sample, parts-per-million (ppm) values are the same as micrograms/gram. The micrograms/gram concentrations may be divided by 1×10^6 micrograms/gram to convert them into grams of element/gram of zircon. Concentrations in moles element/gram zircon are obtained by dividing the grams/gram concentrations by the atomic weights of uranium and thorium (238.03 and 232.038 g/mole, respectively) (Table B2). Now, 99.2743% of modern natural uranium is ^{238}U and only 0.7200% is ^{235}U (Faure, 1998, p. 284). These percentages are used to determine the current concentrations in moles/g of each uranium isotope (Table B2). Next, the moles/g of ^{238}U, ^{235}U, and ^{232}Th are multiplied by Avogadro's number (6.022×10^{23} atoms/mole) to obtain the total number of atoms (N) of each isotope in every gram of zircon.

Table B2: Concentrations of uranium and thorium and the total number of ^{238}U, ^{235}U and ^{232}Th atoms in the zircons from Zartman (1979).

Element or Isotope	**Current Total Element Concentration, ppm or µg/g**	**Element, mole/g**	**Isotope, mole/g**	**N, Current Number of Atoms in Zircon, atoms/g**
Uranium	328.78	1.3813E-06		
^{238}U			1.3712E-06	8.2576E+17
^{235}U			9.9450E-09	5.9889E+15
Thorium (^{232}Th)	169.42	7.3014E-07	7.3014E-07	4.3969E+17

The following equation and constants from Faure (1998, p. 281-284) are used to calculate the number of moles of radiogenic lead and helium produced from the decay of ^{238}U, ^{235}U and ^{232}Th since the zircons described in Zartman (1979) formed:

$$D^* = N(e^{\lambda t} - 1)$$

D^* = number of radiogenic Pb atoms

N = number of uranium and thorium atoms **currently** present in the sample.

λ = decay constants:

λ for $^{238}U = 1.55125 \times 10^{-10}$ 1/year

λ for $^{235}U = 9.8485 \times 10^{-10}$ 1/year

λ for $^{232}Th = 4.9475 \times 10^{-11}$ 1/year

t = age of the sample

Zartman (1979) found the zircon at 2903.8 meters depth to be 1.500 billion years old. The number of daughter atoms (a D^* value for ^{206}Pb, ^{207}Pb, and ^{208}Pb) can now be calculated for the Zartman (1979) zircon, as shown in Table B3. For every ^{206}Pb atom produced by the decay of ^{238}U, 8 ^{4}He atoms form. The formation of a ^{207}Pb atom results in the formation of 7 ^{4}He atoms and 6 ^{4}He atoms are associated with every ^{208}Pb atom (Gentry *et al.*, 1982a, p. 1129). Table B3 also lists the number of radiogenic helium atoms that would be produced by 1.50 billion years worth of radioactive decay of ^{232}Th, ^{235}U, and ^{238}U.

Avogadro's number is used to convert the number of radiogenic helium atoms into moles of helium per gram of zircon (Table B3). The helium concentrations in moles associated with the decay of ^{238}U, ^{235}U, and ^{232}Th are then summed and provide the total amount of helium produced by the decay of uranium and thorium over 1.50 billion years (Table B4). Following the usage in Gentry *et al.* (1982a), Humphreys *et al.* (2003a), and Appendix A in this document, the moles of radiogenic helium are converted into nanocubic centimeters of helium per microgram of zircon at standard temperature and pressure (STP) (Table B4).

Table B3: The amount of lead and helium daughter products in the Zartman (1979) zircons.

Parent Isotope	**N, atoms/g of isotope**	**D*, Pb atoms**	**# He, atoms/g**	**mole He/g**
^{238}U	8.2576E+17	2.1634E+17	1.7307E+18	2.8740E-06
^{235}U	5.9889E+15	2.0248E+16	1.4174E+17	2.3537E-07
^{232}Th	4.3969E+17	3.3872E+16	2.0323E+17	3.3748E-07

Table B4: Total radiogenic helium in the Zartman (1979) zircons.

Total mole He/g	**He cc STP/µg**	**He ncc STP/µg**
3.4468E-06	7.7208E-08	77.2

Before calculating the Q/Q_0 value for the zircon results from Zartman (1979), the alpha ejection value for the zircons must be determined. The alpha ejection value refers to the percentage of helium atoms that escape from a zircon as the helium forms from the decay of uranium and thorium. Estimating the alpha ejection value involves a lot of uncertainties. Gentry *et al.* (1982a, p. 1129-1130) assumed an alpha ejection value of 30-40% for their 40-50 micron zircons:

> "Knowledge of the zircon mass and the appropriate compensation factor (to account for differences in initial He loss via near-surface α-emission) enabled us to calculate the theoretical amount of He which could have accumulated assuming negligible diffusion loss. This compensating factor is necessary because the larger (150-250 µm) zircons lost a smaller proportion of the total He generated with the crystal via near-surface α-emission than did the smaller (40-50 µm) zircons. For the smaller zircons we estimate as many as 30-40% of the α-particles (He) emitted within the crystal could have escaped initially whereas for the larger zircons we studied only 5-10% of the total He could have been lost via this mechanism."

Without providing any calculations to support his accusations, Humphreys (2005a) claims that Gentry *et al.* (1982a) somehow "misstated" these alpha ejection values. As an alternative, Loechelt (2008c, p. 5) used a method from Meesters and Dunai (2002b), where the correction for the loss of alpha particles is done during the diffusion simulations. Tagami *et al.* (2003) also contains equations that are used to estimate alpha ejections from zircons. The following equations from Tagami *et al.* (2003, p. 59) calculate the fraction of alphas **retained** by a zircon immediately after their formation from radioactive decay:

$$F_T = 1 - 4.31\beta + 4.92\beta^2$$

$$\beta = (4L + 2W)/LW$$

where:

F_T = fraction of alphas (^{4}He) retained by the mineral

L = length of the zircon in microns or cm.

W = width of the zircon in the same units as the length.

Therefore:

Alpha ejection value = 1 - F_T

Although Gentry *et al.* (1982a) described the "sizes" of their analyzed zircons as 40-50 μm, the following description in Humphreys *et al.* (2003a, p. 3), which is probably based on a personal communication with R. Gentry, indicates that the zircons of samples 1, 3, 5 and 6 were somewhat larger, at least in length:

> "At Oak Ridge, Robert Gentry, a creationist physicist, crushed the [rock] samples (without breaking the much harder zircon grains), extracted a high-density residue (because zircons have a density of 4.7 grams/cm^3), and isolated the zircons by microscopic examinations, choosing crystals about 50-75 μm long."

This account suggests that the zircons were recovered by float-sink methods and "grain picking" under a microscope. Although Humphreys (2005b, p. 43) states that zircons with lengths of 50-75 microns were also selected for the 2003 analysis (sample 2003 in my Table 1), no width data on any of the zircons are specifically listed anywhere in Gentry *et al.* (1982a) or in any of the Humphreys *et al.* documents. Without width data, a F_T cannot be accurately calculated. Although far from ideal, the only present method of estimating all of the dimensions of the zircons in Humphreys *et al.* (2003a, 2004) and Gentry *et al.* (1982a) is to use information from Heimlich (1976). Heimlich (1976) performed a detailed zircon study on nine samples from the Fenton Hill GT-2 core, which included measuring the lengths and widths of zircons that were collected close to samples 1, 2003, 2, and 3 of Gentry *et al.* (1982a) and Humphreys *et al.* (2004) (my Table 1). Specifically, Heimlich (1976) sampled zircons at a depth of 2902 meters, whereas the zircons from Zartman (1979) probably came from the same granodiorite at a depth of 2903.8 meters. The sample 3 zircons from Gentry *et al.* (1982a) also came from a depth of about 2900 meters. Some relevant parameters from Heimlich (1976) are shown in Table B5.

Notice that the mean lengths of the zircons in Heimlich (1976) are often much longer than the 50-75 microns listed in Gentry *et al.* (1982a). Specifically, the zircons obtained by Heimlich (1976) had a mean length of about 100 microns and, considering two standard deviations, the lengths could have reached nearly 180 microns. Using a mean length/mean width ratio of 2.4 for the zircons from a depth of 2902 meters described in Heimlich (1976) (Table B5), the 50-75 micron zircons used by Gentry *et al.* (1982a) and Dr. Humphreys should have had widths of about 20-30 microns.

Table B5: Mean length and width of zircons from the Fenton Hill cores (Heimlich 1976, p. 7).

Depth (meters) from Heimlich (1976)	**Relevant Gentry *et al.* (1982a) or Humphreys *et al.* (2004) Sample**	**Mean Length (2 std. dev.), microns**	**Mean Width (2 std. dev.), microns**	**Mean Length/ Mean Width Ratio**
960	1	96.9 (57.4)	43.3 (24.2)	2.2
960 (2nd sample)	1	70.7 (41.0)	38.3 (18.8)	1.8
1492	~2003	91.1 (60.2)	40.2 (23.6)	2.3
2165	~2	92.1 (64.0)	47.4 (28.8)	1.9
2902	~3	101.7 (76.0)	43.2 (26.4)	2.4

Estimating the widths for samples 5 and 6 are even more uncertain. Sample 5 (like 3) is a biotite granodiorite (Laughlin *et al.*, 1983, p. 26). ***I will assume that the mean length to mean width ratio for sample 5 is similar to sample 3 (another biotite granodiorite) or about 2.4.*** Sample 6 is a gneiss that has been intruded by a fine-grained granodiorite (Laney *et al.*, 1981, p. 4). The mean length to mean width ratios are probably in the range of 1.8 to 2.4. To obtain a maximum range of possible F_T values for sample 6, a ratio of 1.8 will be used for any 75 micron long zircons and a value of 2.4 would be used with the 50 micron long zircons.

Using the F_T values in Table B6, the alpha ejection values for the zircons from Zartman (1979) are roughly estimated at 60-80%. As shown in Table B7, the alpha ejection values are used to obtain a range of Q_0 values for the zircons from Zartman (1979). Uranium and thorium results for the Fenton Hill zircons in Gentry *et al.* (1982b) suggest that the helium concentrations (Q values) should greatly vary from zircon to zircon. To obtain highly accurate Q/Q_0 values for every zircon, the helium concentration (Q) of each individual zircon must be known. Unfortunately, this information is not available.

Table B6. Calculated fraction of retained alphas in zircons using the equations from Tagami *et al.* (2003).

Depth (meters) from Heimlich (1976)	**Relevant Gentry *et al.* (1982a) or Humphreys *et al.* (2004) Sample**	**Length, microns**	**Estimated Width, microns (one significant digit)**	**F_T, fraction alphas retained by zircon (one significant digit)**
960	1	75	30	0.4
		50	20	0.2
960 (#2)	1	75	40	0.5
		50	30	0.4
2902	~3	75	30	0.4
		50	20	0.2
3930	5	75	30	0.4
		50	20	0.2
4310	6	75	40	0.5
		50	20	0.2

Table B7. Q/Q_0 values for the Zartman (1979) zircons. Q is from Humphreys *et al.* (2004). The calculated Q/Q_0 value from Loechelt (2008c) for nearby sample 3 of Gentry *et al.* (1982a) is listed as a comparison.

He ncc STP/µg	**Alpha Ejection Fraction**	**Q_0 He ncc STP/µg**	**Q He ncc STP/µg**	**Q/Q_0**	**Q/Q_0 Loechelt (2008c)**
77.2	0.6	31	2.8	0.09	0.038
	0.8	15	2.8	0.18	

Gentry *et al.* (1982b) obtained uranium and thorium data on seven zircons from samples 1, 5, and 6 (three zircons from sample 1 and two each from 5 and 6). The data are in atomic parts per million rather than mass parts per million. To obtain a maximum possible range of helium Q_0 values for each of the seven zircons, the calculations were paired up the highest uranium concentration for each zircon with its highest concentration of thorium and the lowest uranium concentration with the lowest thorium value. As an example, Table B8 shows the calculations for zircon 1A from sample 1.

First, the atomic parts per million uranium and thorium values were converted into mole fractions by multiplying by one million (Table B8). Following Gentry *et al.* (1982b), the uranium and thorium were assumed to occur in otherwise pure zircon. The mole fractions were converted into moles element/gram zircon by dividing them by the molecular mass of pure zircon (183.3071 g/mole based on atomic weights from Faure, 1998). Zartman (1979) obtained a radiometric date of 1.500 billion years for the sample 3 granodiorite. Pb-Pb dates in Appendix A of Humphreys *et al.* (2003a) indicate that the zircons from sample 2002 are up to about 1.44 billion years old. To calculate the amount of radiogenic lead and helium, an age of 1.44 billion years was assumed for sample 1 and samples 5 and 6 were dated at 1.50 billion years. The remaining calculations are the same as those used in Tables B2-B7. The resulting range of Q/Q_0 values for samples 1, ~3 (Zartman, 1979 data), 5, and 6 are listed in Table 2.

Table B8: Calculation of Q/Q_0 values from uranium and thorium data of zircon 1A from Gentry *et al.* (1982b). Zircon 1A is from sample 1 of Gentry *et al.* (1982a).

Sample	**Element (minimum and maximum)**	**Element, atomic ppm**	**Element, mole fraction**	**Element, mole/g**
1A	U min	240	0.00024	1.31E-06
	Th min	800	0.0008	4.36E-06
	U max	5300	0.0053	2.89E-05
	Th max	2000	0.002	1.09E-05

Table B8: Continued.

Sample	Element (min and max)	^{283}U, mole/g	^{235}U, mole/g	^{232}Th, mole/g
1A	U min	1.30E-06	9.43E-09	
	Th min			4.36E-06
	U max	2.87E-05	2.08E-07	
	Th max			1.09E-05

Sample	Element (min and max)	^{238}U atoms/g	^{235}U atoms/g	^{232}Th atoms/g
1A	U min	7.83E+17	5.68E+15	
	Th min			2.63E+18
	U max	1.73E+19	1.25E+17	
	Th max			6.57E+18

Sample	Element (min and max)	^{206}Pb, atoms/g	^{207}Pb, atoms/g	^{208}Pb, atoms/g
1A	U min	1.96E+17	1.78E+16	
	Th min			1.94E+17
	U max	4.33E+18	3.92E+17	
	Th max			4.85E+17

Table B8: Continued.

Sample	Element (min and max)	Total He, atoms/g	Total He, ncc STP/µg	Zircon Length, microns	Estimated Zircon Width, microns	F_T
1A	U & Th min	2.86E+18	106	75	40	0.5
				50	20	0.2
	U & Th max	4.03E+19	1498	75	40	0.5
				50	20	0.2

Sample	Element (min and max)	Alpha Ejection Fraction	Q_0, He ncc STP/µg	Q, He ncc STP/µg	Q/Q_0
1A	U & Th min	0.5	56.6	8.6	0.15
		0.8	26.5	8.6	0.33
	U & Th max	0.5	798	8.6	0.011
		0.8	373	8.6	0.023

As shown in Tables 2, B1 and B8, the uranium, thorium, Q_0 and Q/Q_0 values of the individual zircons are highly variable and the range of values are too inadequately known for accurate modeling. Even if the dating equations in Humphreys *et al.* (2003a) were reliable (see above and Loechelt, 2008c), the inaccurate and poorly defined Q/Q_0 values in Gentry *et al.* (1982a), Humphreys *et al.* (2003a) and Humphreys *et al.* (2004) clearly preclude any attempts to date the zircons with the Humphreys *et al.* method.

APPENDIX C: CRUCIAL QUESTIONS THAT DR. HUMPHREYS CAN'T OR WON'T ANSWER

Introduction

In Humphreys (2005a) and Humphreys (2006), which are replies to my original and November, 2005 Talkorigins essays, respectively, Dr. Humphreys repeatedly failed to properly address the frequent problems in his work. Humphreys (2008a), Humphreys (2008b) and Humphreys (2010) also lacks suitable responses to a number of his critics. It's obvious from his superficial statements and numerous misconceptions that Dr. Humphreys has never attempted to properly review and understand the criticisms of his work from others and me. To encourage Dr. Humphreys to finally address these issues, I have summarized some of his problems as a series of questions in this appendix. I'm hoping that Dr. Humphreys will take several months and properly perform the necessary experiments to really deal with these issues rather than just making additional rash, superficial and insulting replies. Furthermore, Dr. Humphreys' other critics (such as Drs. Loechelt, Isaac and Whitefield) raise many other questions and criticisms that Dr. Humphreys needs to answer to salvage his research. Dr. Humphreys must carefully and rationally deal with these questions before scientists will ever take his work seriously.

Missing *a* Values and Anisotropic Diffusion

1. As admitted in Humphreys (2005a) and Humphreys *et al.* (2004, p. 5), why did Dr. Humphreys never bother to have experimenter sort the zircons in the 750-meter sample by size and have the *a* values of the zircons measured when accurate values of *a* are critical in calculating the "dates" with equations 13-14 and 16 in Humphreys *et al.* (2003a)? How is Dr. Humphreys practicing good science by taking shortcuts and not carefully measuring **all** of his parameters?

2. Equations are available that deal with the anisotropic diffusion of noble gases in solids (e.g., McDougall and Harrison, 1999, p. 141). Why did Dr. Humphreys not use these more accurate equations with his zircons rather than improperly assuming that they and his biotites were isotropic? Humphreys *et al.* (2004, p. 15) claims that assuming anisotropy for his zircons would only alter his results "by less than a factor of two." What evidence or calculations does Dr. Humphreys have to support this claim?

Missing *b* values

3. The variable b must be precisely known to obtain "helium diffusion dates" from equations 12-14 and 17 in Humphreys *et al.* (2003a). Humphreys *et al.* (2003a, p. 8) also used *b* as part of their efforts to justify removing sample #6 from their "creation model." Dr. Humphreys' documents only contain one approximation for *b*, which is an average of ~1000 microns for an unknown number of biotites from the 750-meter sample (Humphreys *et al.*, 2003a, p. 8). Because the zircons and

biotites of the Fenton Hill cores come from gneisses and a variety of igneous rocks, what justification does Dr. Humphreys have for applying only one poorly defined *b* value and one Q_0 value to all of his and R. V. Gentry's samples from the Fenton Hill cores? Why would Dr. Humphreys expect a *b* value of a metamorphic biotite to be the same as a *b* for an igneous biotite?

4. How is it good science for Dr. Humphreys not to carefully measure *b*, *a*, or any of his other parameters and not provide suitable standard deviations?

5. Humphreys (2005a) replies to my criticisms of his single *b* value with the following nonsensical statement:

> "However, Henke has the raw data we published, so he can compute the standard deviations for himself."

Where are these raw data, Dr. Humphreys? Because his papers only contain one average *b* value (p. 8, Humphreys *et al.*, 2003a), how can anyone obtain a suitable standard deviation from only one number? Using the proper unbiased equation (see Davis, 1986, p. 33) for calculating standard deviations would lead to division by zero. Also, why is Dr. Humphreys not concerned about carefully determining his standard deviations?

Impure and Improper Biotite Separations

6. What calculations does Dr. Humphreys have to claim that assuming isotropy for his zircons and biotites would only lengthen the helium diffusion time by no more than 30% (Humphreys *et al.*, 2003a, p. 9)? How would any loss of helium from grinding the biotites affect his calculations (see question #8 below)? Considering the pronounced cleavage planes in biotite, why would Figure 7 in Humphreys *et al.* (2003a) even be a reasonable approximation?

7. Humphreys (2005a) calls on me to do a better job in separating the biotites from his samples, but why should I do his work for him? Why shouldn't he strive to do his own work properly?

8. Why did Dr. Humphreys' workers grind instead of cut his biotite specimens when Trull and Kurz (1993, p. 1314) and Mussett (1969, p. 298) warn that silicate minerals can lose much of their noble gases through grinding? Why should we accept the helium measurements on the Fenton Hill biotites (Appendix B of Humphreys *et al.*, 2003a) when they have been ground?

9. The experimenter in Appendix B of Humphreys *et al.* (2003a) indicates that the biotite samples from the Fenton Hill core are impure, which would affect the diffusion results of the biotites. Even if ICR laboratory personnel managed to successfully separate some biotites from the Fenton Hill samples, why did Dr. Humphreys trust them when they so messed up the mineral separations from Dr. Austin's Mt. St. Helens' sample? (See: "Young-Earth Creationist 'Dating' of a Mt.

St. Helens Dacite: The Failure of Austin and Swenson to Recognize Obviously Ancient Minerals" for specific examples of the faulty mineral separations in Dr. Austin's work.) What scientist would trust the haphazard and unreliable efforts of such a laboratory?

10. How can Humphreys *et al.* (2003a) justify the use of data from these ground biotites to remove sample #6 from their "creation model"?

Unexplained "Typos" in Gentry *et al.* (1982a)

11. How and when were the "typos" related to the helium measurements (Q values) in Gentry *et al.* (1982a) discovered? Were the original laboratory notes consulted to correct the typographical errors? If not, how were they reliably corrected? Were the values corrected independently of any of Dr. Humphreys' results or were the values "corrected" to comply with Dr. Humphreys' results? **(R. V. Gentry never replied to my emails on this issue.)**

12. If the 30-40% alpha ejection values in Gentry *et al.* (1982a) are too small and "misstated" as Humphreys (2005a) claims and the Q values in Gentry *et al.* (1982a) have typos, why should Dr. Humphreys accept the Q/Q_0 values or any other data in Gentry *et al.* (1982a) especially when the chemical data in Gentry *et al.* (1982b) indicate that the Q/Q_0 values are often inflated? (See my calculations in Appendix B.) How does Dr. Humphreys know that the 30-40% alpha ejection values in Gentry *et al.* (1982a) are too small? Why is Dr. Humphreys still willing to trust the Q/Q_0 values in Gentry *et al.* (1982a) after he's admitted that almost every other datum in this paper is a "typographic error" or "misstated" number?

Inaccurate Q_0 values and Inflated Q/Q_0 values

13. Because Humphreys (2005a) had no problem immediately correcting his unit of measure error in Appendix C of Humphreys *et al.* (2003a), why is Dr. Humphreys taking so many years to fulfill his commitment in Humphreys (2005a) to provide the calculations on how he and R. V. Gentry obtained a Q_0 value of only 15 ncc STP/μg?

14. What justification does Dr. Humphreys have for applying only one Q_0 value to all of the zircons from the diverse metamorphic and igneous rocks of the Fenton Hill cores?

15. How can Humphreys (2005a) maintain that his and R. V. Gentry's Q_0 value of 15 ncc STP/μg is approximately accurate when it's inconsistent with the chemical data in Gentry *et al.* (1982b)? (See the calculations in my Appendix B, which Dr. Humphreys repeatedly ignores.) The chemical data in Gentry *et al.* (1982b) indicate that the Q_0 values for the different Fenton Hill zircons are highly variable and may be as high as 800 ncc STP/μg. Even the three zircons in Appendix A of Humphreys *et al.* (2003a) have significantly variable uranium concentrations that

range from 218 to 612 parts per million, which would yield very different Q_0 values.

16. Humphreys (2005a) claims:

> "But after discussing the matter with him [R. V. Gentry], I'm inclined to think that even if he had an error in Q_0, the error canceled out when he calculated the ratio Q/Q_0, which is the crucial quantity in this analysis."

What detailed calculations does Dr. Humphreys have to support this claim? Why should any scientist trust Q/Q_0 values that rely on serious errors in Q and Q_0 to just fortuitously cancel out? How can erroneous Q and Q_0 values in Gentry *et al.* (1982a) yield accurate Q/Q_0 values? Since when do two wrongs make a right?

17. Why should anyone continue to assume that the Q/Q_0 value of sample #1 is 0.58, when chemical analyses on sample #1 zircons in Gentry *et al.* (1982b) indicate that the value may be as low as 0.011 (see my Appendix B)?

18. Because the chemical data in Gentry *et al.* (1982b) indicate that the Q/Q_0 values in Dr. Humphreys' documents are often an order of magnitude too high (see my Appendix B), how can Humphreys *et al.* (2003a, Table 1 on p. 3, etc.) and Gentry *et al.* (1982a) claim that their values are accurate within ± 30%?

19. Considering that the actual uranium and thorium analyses in Table 1 of Gentry *et al.* (1982b) and the calculations in my Appendix B indicate that Dr. Humphreys' Q/Q_0 values are often inflated by an order of magnitude, how can Humphreys (2005a) claim that the data for his zircons are "perfectly consistent" with the chemical data in Gentry *et al.* (1982b)?

20. Why do Humphreys (2005a), Humphreys (2006) and subsequent articles ignore the important Q/Q_0 calculations in my Appendix B and how they affect his "creation date" of 6,000 years?

21. Because equations in Humphreys *et al.* (2003a) are based on known false assumptions (such as isotropic diffusion) and because their a, b, and Q/Q_0 values are either missing, poorly measured or inadequately explained, how would I have any difficulty meeting the "burden of disproof" to refute Dr. Humphreys' "creation model" as stated in Humphreys (2005a)? Why are the numerous arguments in this essay and other documents by Dr. Humphreys' critics not enough to at least raise questions about the claims in Dr. Humphreys' work?

22. Considering how Dr. Humphreys has manipulated and misused his a, b, and Q/Q_0 values (see the other questions in this appendix and the text of my Talkorigins essay for details), why couldn't the alignment between the "creation model" and the defect curve in Figure 2 of Humphreys (2005a) be nothing more than a fluke or an inappropriate manipulation as indicated by Loechelt (2008c; 2009a)?

Important Thorium Data are Missing

23. Why didn't Humphreys *et al.* (2003a) measure the thorium in their zircons when chemical data in Gentry *et al.* (1982b) indicate that thorium concentrations in the Fenton Hill zircons are highly variable and could be significant sources of radiogenic helium?

The Wet Past of the Fenton Hill Rocks

24. How can Dr. Humphreys in Humphreys *et al.* (2003a) and Humphreys (2005a) dismiss the importance of fluids in altering the rocks of the Fenton Hill cores in the past and possibly affecting their helium concentrations when Laney *et al.* (1981), Laughlin and Eddy (1977, p. 28), Sasada (1989), and other references indicate that the rocks were more permeable and contained fluids in the recent past? How did the extensive hydrothermal (i.e., "hot water") alterations and hydrothermal minerals identified by Laney *et al.* (1981) and Laughlin and Eddy (1977, p. 28) form in these rocks if Dr. Humphreys thinks that they were dry?

25. How does the presence of abundant fluid-altered minerals and grains in the Fenton Hill cores support the undocumented proclamation in Humphreys (2005a) that fluids could not have traveled very far in the Fenton Hill Precambrian rocks because "the interface widths between minerals would be microscopic, perhaps only an Ångström (the diameter of a hydrogen atom) or so"?

26. Where is the evidence of the no more than one Ångström wide interface widths? Even if they existed, why couldn't the fluids pass through any persistent fractures in the minerals rather than only in the interface spaces between minerals?

27. Uranium deposited by past fluids has been detected in fractures in the Fenton Hill rocks (West and Laughlin, 1976, p. 618). Because uranium produces extraneous helium, why won't Dr. Humphreys look for extraneous helium in his samples? (Also see next section.)

Possibility of Extraneous Helium

28. In my section "An Extraneous Helium Hypothesis and How to Test It", I propose an hypothesis on how extraneous helium could have contaminated the Fenton Hill rocks in the past and how the extraneous helium could still be in the relatively impermeable zircons, but not in the surrounding permeable biotites. Rather than trying to understand this hypothesis, Humphreys (2005a) just repeats his same old Lyell uniformitarian mantra, which states that because his **ground** biotites from **one** section of the Fenton Hill cores **currently** have very little helium, they could **never** have had any more helium in them thousands of years ago:

> "First, if the helium in the zircons were 'excess' and came from outside them, it would have had to come through the biotite. As I pointed out on p. 9 of *CRSQ* 2004, the helium concentration in the biotite is two hundred

> times lower than the concentration in the zircon. That means, according to the laws of diffusion, that the helium is presently leaking *out* of the zircons into the biotite, not the other way around. Also, as I pointed out, the total amount of helium in the biotite is roughly the same as the helium lost from the zircon."

Dr. Humphreys, do you now understand how you're making invalid Lyell uniformitarian assumptions about the **past** helium concentrations in the Fenton Hill biotites?

29. Why does Humphreys (2005a) want me to perform a series of superfluous field studies to look for extraneous helium at Fenton Hill when the possibility of extraneous helium could be easily tested if he would simply look for ^{3}He in his zircons and ^{4}He in surrounding low-uranium/thorium quartz grains? If I did any field work, why wouldn't Dr. Humphreys simply invoke more miracles or other excuses to dismiss any of my field results that he doesn't like just as he did with the U/Pb dates of his own zircons in Appendix A of Humphreys *et al.* (2003a)?

30. Why does Humphreys (2005a) believe that helium contamination of the Fenton Hill rocks would require "magmatic fluids" and in particular "basaltic magmatic fluids" when uranium deposits have already been identified in the Fenton Hill cores (West and Laughlin, 1976, p. 618), which could locally produce extensive extraneous helium? Furthermore, fractures produced from orogenies can allow extraneous helium to enter the Fenton Hill rocks not only from deep degassing magmas, but also from massive and solid portions of the mantle (Goff and Gardner, 1994, p. 1816).

31. Extraneous helium currently exists in the Valles Caldera only 8 or so kilometers from Fenton Hill (Smith and Kennedy, 1985; Truesdell and Janik, 1986). Since helium has already traveled from deep in the Earth to the Valles Caldera, why couldn't the helium travel a few extra kilometers to contaminate the Fenton Hill samples? The helium could have easily traveled with the other fluids that contaminated the Fenton Hill cores as described in Sasada (1989).

32. If extraneous helium is present in Dr. Humphreys' zircons, why can't his "uniformitarian model" have an age anywhere between thousands to 1.5 billion years?

33. Because Gentry *et al.* (1982a) admits that the helium in their #5 and #6 samples may not be radiogenic but "derived from some other sources", why shouldn't Dr. Humphreys look for extraneous helium in his zircons?

34. Why does Humphreys (2005a) consider the possibility that his zircons were contaminated with extraneous helium to be a "pure conjecture", "improbable coincidences" and "short of credibility" while he considers his flawed Figure 2 and groundless claims of miraculous accelerated radioactive decay rates to be

"scientific evidence"? Unlike his miraculous fantasies, is not my extraneous helium hypothesis testable as described in question #29?

Temperature Problems

35. How can Dr. Humphreys assume that temperatures have been constant over time in the Fenton Hill cores when that assumption is refuted by Harrison *et al.* (1986), Sasada (1989) and the detailed discussions in Loechelt (2008c)? Why should any scientist accept Dr. Humphreys' known false and unjustified assumptions of constant temperatures as an "act of generosity" to the "uniformitarians"? Why doesn't Dr. Humphreys realize that scientists are interested in accuracy and not any unrealistic "acts of generosity" from him?

36. How could temperatures in the Fenton Hill cores remain constant from all of the heat that would have been released by any of these "periods of accelerated radioactive decay"? Where is there **any** evidence that the Earth's mantle and crust have suffered from massive heating events in the past few thousand years? (Also see Whitelaw, 2008, Morton and Murphy, 2004; Pitts, 2009).

Possible Pressure Problems

37. Why is Dr. Humphreys convinced that his defect curve (see my Figure B), which was produced from bare zircons in a laboratory vacuum (probably less than 5×10^{-6} bar), would accurately represent the diffusion of helium at 200 to 1,200 bars in the subsurface of Fenton Hill? This represents a pressure difference of *at least 8 orders of magnitude*. How are Carroll (1991) and other references cited by Humphreys (2006), which involve high-temperature INTRINSIC curves of glasses and minerals, relevant to the DEFECT curve of Dr. Humphreys' zircons?

38. What scientific evidence does Dr. Humphreys have that allows him to confidently proclaim that fractures and other defects in the Fenton Hill zircons would not significantly seal under subsurface pressures and at least slow down helium diffusion along his defect curve? When Dr. Humphreys proclaims that zircons are "too hard" to be affected by high pressures, why does he repeatedly ignore the conclusions in Dunai and Roselieb (1996) that at high pressures of 250 bars and at temperatures up to 700^{o}C, helium would take *TENS* to *HUNDREDS OF MILLIONS OF YEARS TO JUST PARTIALLY DIFFUSE* out of garnets, which are "hard" silicates like zircons? Why doesn't Dr. Humphreys perform the experiments to determine whether or not helium also diffuses slowly from zircons under 200-1,200 bars pressure?

39. In Humphreys *et al.* (2003b) and his other documents, Dr. Humphreys invokes an invalid cryogenic fallacy to attack the validity of his strawperson "uniformitarian model." He believes that slowing down helium diffusion in the zircons to support his "uniformitarian model" would require a cryogenic temperature of -140°C. More realistic models in Loechelt (2008c) refute Dr. Humphreys' cryogenic argument. This absurd temperature is based on an extrapolation of the defect line

in Dr. Humphreys' data (Humphreys *et al.*, 2003b) rather than using the intrinsic curve, the latter of which is more likely to represent diffusion under subsurface conditions (my Figure B; also see discussions on the possible origin of Dr. Humphreys' defect line in Whitefield, 2008). While everyone can agree that cryogenic temperatures are absent on and within the Earth, how are laboratory vacuums any more realistic in terrestrial environments? Why doesn't Dr. Humphreys test his vacuum-based defect curve and its creation model under more realistic high pressure conditions?

Dr. Humphreys' Inconsistent Treatment of Samples 5 and 6

40. How can Humphreys *et al.* (2003a, p. 8) say: "...the disk-like (not spherical) volume of biotite the helium enters is more than 1000 (~32 **squared**) times the **volume** of the zircon, [my emphasis]" when volumes have three dimensions and not two? Because of the consequences of their geometry error (see my text and the following questions for details), how can Humphreys *et al.* (2003a) justify removing sample #6 from their "creation model" and keeping sample #5?

41. How can Humphreys *et al.* (2003a, p. 8) claim that their "hypothesis" of "helium equilibrium" between the zircons and biotites of sample #6 was "supported" when the $V_{biotite}/V_{zircon}$ only equals 0.0095 and not 1000 (~32 squared) as they believe? (See "Dr. Humphreys Confuses Area and Volume" in my main text for further information.)

42. How can Humphreys *et al.* (2003a) argue that the helium concentrations of the zircons and biotites in sample #6 are essentially the same on the basis of comparing the amount of helium in the sample #6 zircons (4310 meters depth) with the helium concentration of an impure and ground biotite sample from a different rock type at only 750-meters depth?

43. Because the calculation of $V_{biotite}/V_{zircon}$ in Humphreys *et al.* (2003a, p. 8) is wrong and they cannot justify comparing the helium concentration of biotite from their 750-meter sample with the helium from zircons from sample #6, how can Humphreys *et al.* (2003a) justify removing sample #6 from their "creation model" while keeping sample #5?

44. Humphreys (2005a) states:

> "However, we could dispense with both samples [*i.e.*, samples #5 and #6] entirely with no damage to our case at all. This is just another quibble about an inconsequential issue."

How can Dr. Humphreys make this claim, when removing sample #5 from his dataset only leaves three samples (#2, #3 and #4) in Table III of Humphreys *et al.* (2004, p. 8) and these three samples provide an outlandish average "date" of 5,100 ± 5,000 years (2-sigma using the unbiased equation, Davis, 1986, p. 33; Keppel,

1991, p. 43-44, 58)? Because the 2-sigma standard deviation is almost as large as the average, how does Dr. Humphreys have a robust data set?

Using Improper Equations to Calculate Standard Deviations

45. Why does Dr. Humphreys use the biased equation in his documents to calculate standard deviations instead of the usual unbiased equation?

46. Applying the proper unbiased equation and two standard deviations to the results in Table III of Humphreys *et al.* (2004, p. 8) yields a ridiculous "creation date" of 6,000 ± 4,600 years. Does Dr. Humphreys realize that his results indicate that "creation" may have occurred as recently as 600 AD?

47. Why does Dr. Humphreys use one standard deviation on some of his calculations and two on others? As examples, Figure 13 in Humphreys (2005b, p. 55) uses two standard deviations, which helps to overlap the diffusion data with the "creation model." In contrast, the errors on his high Q/Q_0 values are only given in one standard deviation (Humphreys, 2005b, p. 30), which deemphasizes the errors associated with these values, probably because the Q/Q_0 values are crucial components of his "creation model."

48. Considering that his "creation model" actually yields a "date" of no better than 90,000 ± 500,000 years (2 unbiased standard deviations) and is based on flawed data, inaccurate equations, and vacuum measurements that might not represent subsurface conditions at the Fenton Hill site, what justification do YECs have for criticizing errors on radiometric dates of only ±1 to 7%? Why don't Dr. Humphreys and his allies scrutinize his work and the Bible to the extent that they do with radiometric dating?

Inaccurate Claims about Lead Diffusion in Zircons

49. Why does Dr. Humphreys use the lead activation energy and diffusion coefficients from Magomedov (1970) to argue in Humphreys *et al.* (2004, p. 10) that the Fenton Hill zircons must be much younger than 1.5 billion years old when lead activation energies and diffusion coefficients on less metamict samples in more recent references (Lee *et al.* (1997, p. 160, 161 and Cherniak and Watson, 2000) are consistent with the zircons being 1.4-1.5 billion years old?

The Graph in Magomedov (1970)

50. Magomedov (1970) clearly states that the activation energy of helium in his zircons was 15 kcal/mole:

> "Estimates of activation energy of bulk diffusion are 58 kcal/mole for Pb in zircon, and only 15 kcal/mole for He."

Yet, when Dr. Humphreys changed the diffusion coefficients on Magomedov's graph from $\ln_e$ to $\log_{10}$ (Figure 5 of Humphreys *et al*, 2003a, p. 6) to comply with his results and the results in Reiners *et al.* (2002), the activation energy off the intrinsic curve was no longer equal to the results in the abstract of Magomedov (1970), but increased to about 40 kcal/mole (see my Figure 2). How is changing the units of measure on the Magomedov graph justified when Magomedov's value of 15 kcal/mole indicates that the diffusion coefficients on his graph are indeed $\ln_e$?

51. Why does Dr. Humphreys believe that ln values in Magomedov (1970) were actually log base 10, when the lead and other data in the tables of Magomedov (1970) clearly refute that interpretation?

52. Since when do scientists manipulate data on graphs so that they line up with "everybody else's zircon data" as advocated by Humphreys (2005a)?

53. Because Humphreys *et al.* (2003a, p. 6) says: "Measurements of noble gas diffusion in a given type of naturally occurring mineral often show significant differences from site to site, caused by variations in composition", why should Humphreys (2005a) expect Magomedov's data to line up with his and Reiners *et al.*'s (2002) results? Why should we expect helium diffusion in these highly metamict Soviet zircons to comply with "everybody else's" results as Humphreys (2005a) claims?

54. Because Humphreys *et al.* (2003a, p. 6) and Humphreys (2005a) admit that the Magomedov data are "ambiguous", why didn't Dr. Humphreys simply discard them instead of manipulating them in Humphreys *et al.* (2003a)?

55. Humphreys (2005a) accuses me of being "inconvenienced" by the Magomedov (1970) data. But, how can I be "inconvenienced" when the unmanipulated Magomedov natural log data actually support my argument that Dr. Humphreys' helium diffusion dating method is seriously flawed? As Humphreys (2000, p. 347) admitted, when he applied the natural log helium diffusion values from Magomedov (1970) to the other parameters of the Fenton Hill sample #1, he got a ridiculous "creation date" of 23 years. It was Dr. Humphreys that had to fudge the Magomedov data so that he would not be inconvenienced with results that do not comply with the diffusion data in his documents and Reiners *et al.* (2002).

Misrepresenting the Arrhenius Plot in Lippolt and Weigel (1988, p. 1454)

56. Why did Humphreys *et al.* (2003a, his Figure 6b on p. 7) selectively connect certain data points on a graph from Lippolt and Weigel (1988, p. 1454), which creates the false impression that a "knee" and "defect line" are present, when Lippolt and Weigel (1988, p. 1454) never recognized the existence of these features in their graph? (See my Figure 4.)

57. Because Dr. Humphreys in Humphreys *et al.* (2003a) manipulated graphs from Lippolt and Weigel (1988) and Magomedov (1970) to support his agenda, why should we not scrutinize his "corrections" of the "typos" in Gentry *et al.* (1982a)? Also, why shouldn't the actual comments of the critic mentioned in Humphreys *et al.* (2004) be published so that we don't have to just accept Dr. Humphreys' summaries of the critic's claims? If the critic's review is considered confidential by the CRS, why did Humphreys *et al.* (2004) refer to its contents?

The "Jemez Granodiorite" Myth: Dr. Humphreys Sampled Gneisses

58. Laughlin (1981), detailed analytical data in Laughlin *et al.* (1983) and even YEC R. V. Gentry in Gentry *et al.* (1982a) recognize that gneisses and a variety of other metamorphic and igneous rocks occur in the Fenton Hill cores. To be exact, most of the cores are gneisses (Laughlin, 1981, p. 308; Laney *et al.*, 1981, p. 2; and my Figure 1). Detailed information in Laughlin *et al.* (1983) and other references also clearly indicate that gneisses and not granodiorites or granites are present in the sections of the cores that were sampled by Drs. Humphreys, Baumgardner and their colleagues (see my Figure 1). What X-ray diffraction, petrographic, or other detailed chemical and mineralogical evidence does Dr. Humphreys have to contradict these references and support his claims that they actually sampled a granodiorite from the Fenton Hill cores?

59. In his emails to me, Dr. Baumgardner was only able to provide naked eye descriptions of the Fenton Hill samples. Since when should Dr. Humphreys or anyone else accept naked-eye observations of small core samples as conclusively distinguishing a granodiorite from a granite or even a weakly banded gneiss? Did not Dr. Baumgardner eventually correct Dr. Humphreys at a conference by stating that their samples were actually gneisses? See: April, 2007 report by Dr. Todd Feeley of a RATE presentation.

60. When Dr. Humphreys and his friends "named" the "Jemez Granodiorite", why did they ignore the fact that most of this "granodiorite" actually consists of gneisses (metamorphic rocks) and not granodiorites (intrusive igneous rocks)? (See Laughlin *et al.*, 1983; Laney *et al.*, 1981; Sasada, 1989, Figure 2, p. 258; Burruss and Hollister, 1979 and my Figure 1.)

61. The USGS database of accepted US rock names has no record of the "Jemez Granodiorite" existing (accessed June 7, 2010). When Dr. Humphreys and his friends "named" the "Jemez Granodiorite", why didn't they follow the required rules for naming a rock unit in the North American Stratigraphic Code? Does Dr. Humphreys realize that by "inventing" invalid rock names and not following the rules of the North American Stratigraphic Code that he and his colleagues are participating in spreading clutter and confusion in the literature?

62. Because Dr. Humphreys once falsely believed that all of his and R. V. Gentry's Fenton Hill zircons came from a single rock unit (the "Jemez Granodiorite"), he made the following statement in Humphreys *et al.* (2003a, p. 6):

> "Measurements of noble gas diffusion in a given type of naturally occurring mineral often show significant differences from site to site, caused by variations in composition. For that reason it is **important** to get helium diffusion data on zircon and biotite from the **same** rock unit (the Jemez Granodiorite [*sic*]) which was the source of Gentry's samples." [my emphasis]

How can Humphreys (2005a) now claim that his inability to distinguish a gneiss from a granodiorite in the Fenton Hill cores is a "distinction without a difference" when he once openly admitted that any mixing of experimental results from different rock types would be inappropriate for his modeling efforts?

63. Humphreys (2005a) makes the following claim:

> "The important point is that, regardless of the name we put on the rock unit [*sic*, units], the zircons throughout it have been measured to contain essentially the same **amounts and ratios** of lead isotopes (Gentry *et al.*, 1982b), and therefore have undergone the same amount of nuclear decay." [my emphasis]

Although the rocks in the Fenton Hill cores have similar radiogenic Pb/Pb ratios and radioactive dates of 1.4-1.5 billion years (Zartman, 1979; Appendix A in Humphreys *et al.* 2003a), contrary to the claims in the above quotation from Humphreys (2005a), the uranium and thorium data in Table 1 of the very article that Humphreys (2005a) cites (that is, Gentry *et al.*, 1982b) indicate that the **amounts** of radiogenic lead would often greatly vary in the Fenton Hill zircons, even within different regions of the same zircon (*e.g.*, Sample #1 in Gentry *et al.*, 1982b; my Appendix B). That is, two zircons can have the **same radiogenic lead ratios** (that is, have the same Pb/Pb dates or have undergone the "same amount of nuclear decay" as Humphreys, 2005a says), but still have radically different sizes (*a* values) and highly variable uranium, thorium and resulting radiogenic lead concentrations (as shown in Gentry *et al.*, 1982b and my Appendix B). If the *a* values and uranium, lead and thorium concentrations are radically different in two zircons of the same age, they will probably have very different helium concentrations. So, how would Dr. Humphreys obtain a similar date for these two zircons with the equations in Humphreys *et al.* (2003a)? (See Table 4 in my essay for numerous examples of the inability of the equations in Humphreys *et al.*, 2003, to provide consistent dates on zircons.)

64. Dr. Humphreys discusses some results on biotites from the "Beartooth gneiss." In Humphreys (2005b), the rock is reclassified as an amphibolite, an entirely different metamorphic rock. Before we can believe his claims about these biotites, we need to have adequately detailed information on the "Beartooth gneiss/amphibolite." Unfortunately, like the "Jemez Granodiorite", the USGS database has no record of this gneiss or amphibolite existing and there is no record of their existence in the peer-reviewed journals listed in the Georef and Web of Science literature databases (accessed June 7, 2010). So, what are the

origins of the names of the "Beartooth gneiss/amphibolite"? What criteria were used to originally identify the rock as a gneiss and then later reclassify it as an amphibolite? Because of the gross misidentification of the "Jemez Granodiorite," how do we even know that Dr. Humphreys' sources have finally properly identified the "Beartooth" as an amphibolite?

Formation of Gneisses

65. Laboratory and field studies indicate that gneissic banding requires metamorphic conditions of about 600-750°C and minimal pressures of 4-6 kilobars to form (see further discussions in my text). How did the gneisses in the Fenton Hill cores and their zircons form in only a few thousand years, especially when Dr. Humphreys claims that these rocks were dry? Even if Dr. Humphreys finally admits that the Fenton Hill rocks had a wet past, how could any amount of water promote extensive metamorphic reactions in only a few thousand years? Dr. Humphreys needs to look at the voluminous references on the geology of the Fenton Hill cores and then try to squeeze the chemistry of these igneous and metamorphic rocks and their complex structures into his YEC "model." In the process, Dr. Humphreys must remember that geology and all other sciences allow no cheating (invoking of miracles) to dismiss inconvenient problems and anti-YEC results. I think that Dr. Humphreys will quickly discover that he has the impossible task of explaining why these numerous metamorphic and igneous Fenton Hill rocks (see my Figure 1) have obviously complex structures and textures that indicate a long history (Laney *et al.*, 1981, Laughlin and Eddy, 1977, Laughlin *et al.*, 1983, Sasada, 1989, Loechelt, 2008c and their references) that refutes young-Earth creationism. Like an old scratched phonograph record or a dented old car, the properties of a metamorphosed rock indicate an extensive and complex history. For example, the development of paragneisses in outcrops and rock cores involves erosion of precursor igneous, sedimentary and/or metamorphic rocks; sediment deposition; deep burial of sediments; at least one metamorphic heating and cooling event; various complex metamorphic reactions; possible faulting and finally uplifting to where geologists can sample and investigate them.

Closure Temperatures for the Zircons

66. A closure temperature of 128^{o}C was listed for the 2002 zircons in Humphreys *et al.* (2003a, Appendix C). However, why was a closure temperature not reported for the 2003 zircons in Humphreys (2005b)? Did the experimenter calculate a closure temperature from the 2003 helium diffusion data listed in the table of Humphreys (2005b, p. 45)? If so, what was it and was it measured from the intrinsic line or the defect line of the data?

Dr. Humphreys Violates the Scientific Method

67. Humphreys (2005a) refers to my objections of his invoking of "God did it!" (*i.e.*, the supposed "accelerated" radioactive decay event[s]) as a matter of "taste." In reality, my objections are based on using the scientific method and the Method of

the Multiple Working Hypotheses. Since when are the rules of the scientific method and the Method of the Multiple Working Hypotheses based on matters of taste? Since when do the rules of the scientific method allow Dr. Humphreys to invoke miracles (i.e., accelerated radioactive decay) to eliminate scientific data (*e.g.*, U/Pb dates) and questions that he doesn't like? Why does Dr. Humphreys repeatedly ignore the rules of the scientific method and the North American Stratigraphic Code in his work? (See question #61.) Professions have rules for good reasons, but Dr. Humphreys feels that he has a privilege to ignore them. Aren't individuals supposed to be ethical and play by the rules established by members of their professions?

68. How is "accelerated radioactive decay" not just another example of the Omphalos and "god-of-the-gaps" fallacies?

69. How is Dr. Humphreys justified in generating "dates" from equations based on false assumptions (constant temperatures over time, isotropic diffusion in biotites and zircons, etc.) and flawed and incomplete data, and then using his Bible and "god of the gaps" to support the invalid results?

70. Since when has invoking "God did it!" provided a satisfactory explanation for the origin of lightning, hail storms, volcanic eruptions, earthquakes, or any other meteorological or geological events? Since when has the invoking of miracles ever been tolerated in a court room, medical school, research laboratory, or anywhere else outside of a religious forum? If psychologists don't blame demons for causing manic depression, car mechanics don't blame gremlins for engine problems, and forensics scientists don't invoke witchcraft to solve unwitnessed crimes, what makes Dr. Humphreys believe that geologists should use the supernatural to explain the origin of a rock?

71. How does Dr. Humphreys distinguish between a miracle and a natural event?

72. What evidence would geologists have to present to Dr. Humphreys before he would be willing to admit that the Earth is ancient and that his biblical interpretations are just plain wrong? If no evidence will do, is not Dr. Humphreys dogmatic? How can anyone that is dogmatic successfully perform the scientific method? In contrast, YECs only need to produce the remains of an *in-situ* village in dinosaur-bearing Cretaceous rocks or in-place mammal remains (bears, squirrels, whales, or bats, etc.) in Cambrian rocks to falsify biological evolution.

73. Because miracles by definition don't obey the laws of chemistry and physics, what keeps individuals from invoking miracles with their subjective imaginations to explain away any natural phenomenon that conflicts with their religious or political agenda? How can miracles be falsified since additional miracles can always be invoked by "psychics" or YECs to explain away failures? How is Dr. Humphreys' invoking of accelerated radioactive decay falsifiable? In contrast, my proposed experiments (such as looking for ^{3}He in zircons) are testable.

74. How are YECs giving up on scientific investigations and invoking miracles via god-of-the-gaps morally equivalent to scientists admitting that they don't understand a lot about the origin of the Big Bang and the origin of life, but that they are too early in their research to give up on searching for answers that comply with the laws of chemistry and physics? Why should YECs invoke god-of-the-gaps when the research possibilities using natural explanations are not even close to being exhausted? Since when has just saying "God did it!" ever provided a satisfying explanation for anything in nature?

75. Why does Dr. Humphreys scoff at my hypotheses that his "dating" results could be undermined by extraneous helium, high uranium and inflated Q/Q_0 values, and pressure effects on his defect curve, when unlike his magical "accelerated radioactive decay event(s)", my hypotheses are testable and falsifiable with the scientific method?

76. Since when is it acceptable for any scientist to allow the *Bible*, *Koran*, *Humanist Manifesto*, or any other religious or political document to dictate their scientific results?

77. Why did the RATE committee hire a Hebrew language scholar to make sure that their results "stay on course" (Morris, 2000, p. viii)? Since when do real research centers and committees allow their results to be screened by a religious or political commissar?

78. In Humphreys (2005a), Dr. Humphreys tries to play down his religious agenda by claiming:

> "The main subject of my articles is the experimental data, and I offered only a few paragraphs about our hypothesis simply to explain what we think really happened."

If this is true, why hasn't Dr. Humphreys ever published a **full article** of his work in an authentic secular peer-reviewed science journal under the scrutiny of world experts on gas diffusion in solids rather than YEC magazines and pamphlets that are willing to accept just about any groundless fantasies and speculations as long they seem to support their biblical agenda? Since when is an author's true motives simply determined by counting paragraphs? Why are Dr. Humphreys' allies only interested in his 6,000 "year" old "date" and these "few paragraphs" on "accelerated" radioactive decay, and generally ignore or unquestionably accept his calculus and faulty and incomplete data? If the emphasis of Dr. Humphreys' work is science, why is it that his work is only cited by YEC fundamentalists and evangelists and not positively by scientists?

Dr. Humphreys' Peer-Review Hypocrisy

79. How can Humphreys *et al.* (2004) count as a "peer-reviewed" article when the *CRSQ* refused to publish statements from Dr. Humphreys' critic (referred to in the

article)? What scientific journal would allow its authors to invoke miracles to get rid of radiometric dates just because they offend the religious agenda of some fundamentalist Christians? What authentic science journal rejects criticisms of its articles as Loechelt (2008c, p. 35) claims? What authentic science journal allows its physics editor (Dr. Eugene F. Chaffin) to edit and control the "peer review" on Humphreys *et al.* (2004) when he had a conflict of interest by serving with Dr. Humphreys on the RATE committee? What scientific organization requires its members to sign loyalty oaths to a religious or political dogma? Why should anyone consider the *CRSQ* to be a respectable science journal when even YECs (*e.g.,* Whitmore *et al.,* 2007) admit that it is not?

80. What moral authority does Dr. Humphreys have to call on me to publish my criticisms as an article in a mainstream scientific journal, when he has never done it? How can a brief abstract in *EOS* (i.e., Humphreys *et al.*, 2003b) and articles in YEC publications count as authentic peer review?

81. Why should I publish my criticisms in a journal, when Talkorigins has no page limits, peer reviews its submissions and is probably more widely read than *CRSQ* and most scientific journals?

Dr. Humphreys needs to rigorously deal with these and other questions. Until he stops his sophomoric insults and flippant statements, he will never achieve any respect among physicists, chemists, engineers, and geologists. If other individuals have appropriate questions for Dr. Humphreys, they can be easily added to this list. Just email me at **krhenk2@pop.uky.edu** . Nevertheless, after seeing how Dr. Humphreys persistently throws out shallow and irrelevant responses to any serious challenges and questions (*e.g.*, Humphreys, 2005a; 2006; 2008a; 2008b; 2010), we shouldn't be surprised if he continues to avoid the real issues at the center of these questions and flippantly respond with more denials and unsubstantiated nonsense.

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