Helium Diffusion Age of 6,000 Years Supports Accelerated Nuclear Decay

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Abstract

Experiments co-sponsored by the Creation Research Society show that helium leakage deflates radioisotopic ages. In 1982 Robert Gentry found amazingly high retentions of nuclear-decay-generated helium in microscopic zircons ($ZrSiO_4$ crystals) recovered from a borehole in hot Precambrian granitic rock at Fenton Hill, NM. We contracted with a high-precision laboratory to measure the rate of helium diffusion out of the zircons. The initial results were very encouraging. Here we report newer zircon diffusion data that extend to the lower temperatures (100° to 277° C) of Gentry's retention data. The measured rates resoundingly confirm a numerical prediction we made based on the reported retentions and a young age. Combining rates and retentions gives a helium diffusion age of 6,000 ± 2,000 years. This contradicts the uniformitarian age of 1.5 billion years based on nuclear decay products in the same zircons. These data strongly support our hypothesis of episodes of highly accelerated nuclear decay occurring within thousands of years ago. Such accelerations shrink the radioisotopic "billions of years" down to the 6,000-year timescale of the Bible. (§ is section of reference being cited.)

Introduction

Under the deep blue skies of northern New Mexico in the fall of 1974, drillmen labored to extract cores from a borehole called GT-2 (Figure 1) nearly three miles deep. The site was Fenton Hill, on the west flank of the Valles volcanic caldera in the pine-covered Jemez Mountains. Two dozen miles to the east, geoscientists at Los Alamos National Laboratory analyzed the drill cores, investigating whether the hot, dry rock would be suitable for providing geothermal energy.

The geoscientists identified the rock as biotite granodiorite, a granitic rock containing shiny flecks of a black mica called *biotite*. They crushed a core sample from a depth of 2,900 meters and extracted microscopic crystals of zirconium silicate ($ZrSiO_4$) embedded in the biotite. These crystals, called *zircons*, were radioactive, containing high amounts of uranium and thorium relative to the rest of the rock, as is usual for that mineral. Comparing two

Figure 1. Drilling rig for borehole GT-2 at Fenton Hill, NM. Photo: courtesy of Los Alamos National Laboratory.

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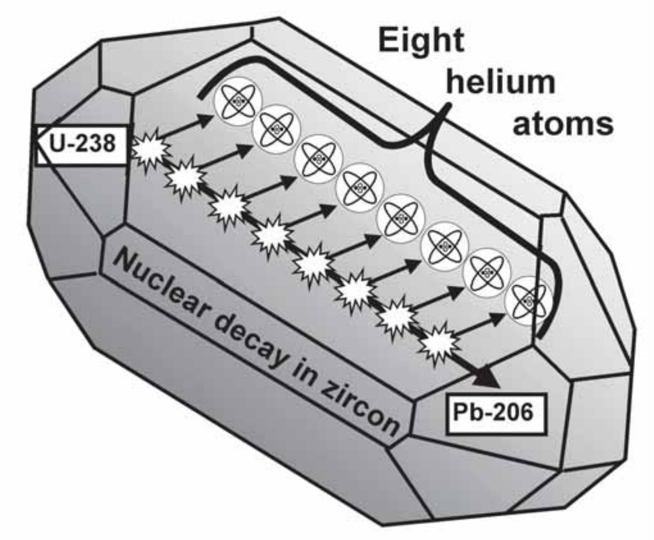

Figure 2. Uranium, lead, and helium in a schematic zircon. ^{238}U decaying to ^{206}Pb releases eight alpha particles (which become helium atoms) within the crystal.

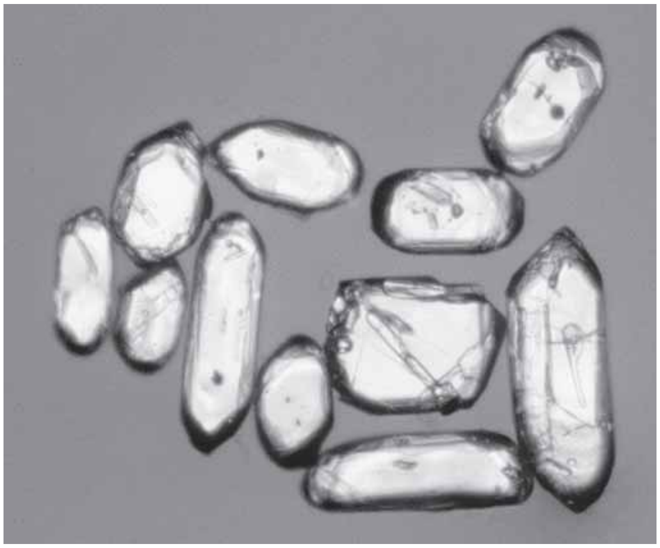

Figure 3. Zircons 50 to 75 μm long extracted by Gentry et al. **(1982a) from GT-2 core samples. Photo: courtesy of R. V. Gentry.**

isotopes of *radiogenic* (formed by nuclear decay) lead (^{206}Pb from ^{238}U and ^{207}Pb from ^{235}U), they determined that 1,500 ± 20 million years worth of nuclear decay had occurred in the zircons—assuming as usual that nuclear decay rates have always been constant (Zartman, 1979). The date is consistent with uniformitarian expectations for this Precambrian "basement" rock unit.

The zircons also would be expected to contain helium (^4He), which comes from the alpha particles (nuclei of helium atoms) emitted by many of the nuclear decays (Figure 2). This prompted Robert Gentry at Oak Ridge National Laboratory to ask the Los Alamos team to send him core samples from various depths in GT-2 (as well as samples from deeper boreholes nearby). Gentry and his team extracted zircons from the samples, hand-picked crystals between 50 and 75 microns long (Figure 3), and measured the total amounts of helium in them. From the amounts of radiogenic lead in the zircons, they estimated how much helium the nuclear decay should have deposited in the crystals. They found that "an almost phenomenal amount of He has been retained" in the zircons, despite them being small, hot, and allegedly old (Gentry et al., 1982a). Table I shows their results as samples 1 through 6.

Note carefully: Gentry's large retentions are *not* what uniformitarians mean by "excess helium" (Baxter, 2003), a common mental pigeonhole into which they shove helium anomalies. In the context of these zircons, "excess" helium would correspond to retentions *greater than 100%* of the amount nuclear decay could produce in 1.5 Gyr. We are not claiming such "excess" helium at all. As we explain in "Latest results arrive in mid-2003" on page 8 of this paper, the uniformitarian method of "helium dating," called *(U-Th)/He chronometry*, is entirely different from the helium *diffusion* dating we are employing here. Uniformitarian "helium dating" methods would not call attention to the large helium *retentions* we are concerned with, so it may be that sites like borehole GT-2 are common throughout the world.

RATE gets involved

When creationists became aware of Gentry's data, many of us thought that it would have been impossible for the zircons to have retained that much helium for even a million years, much less over a billion years. Helium is a lightweight, fast-moving atom that does not attach itself to other atoms, so it *diffuses* (spreads out) through the atomic lattices of most minerals relatively fast. However, we knew of only one published measurement of diffusion rates of helium through zircon (Magomedov, 1970). That report was sketchy and ambiguous, leaving room for quite different ways to interpret the numbers. There were no published measurements for helium diffusion through biotite, the mineral surrounding the zircons. Until we had reliable numbers for these diffusion rates, we could not say for certain that the large retentions require a young earth.

In 1998, the Radioisotopes and the Age of the Earth (RATE) steering committee began planning to do experiments to measure helium diffusion in the relevant miner-

Sample	Depth (m)	Temp. (˚C)	He (ncc/μg)	Fraction	Error
2002	750	96	~12.1	~0.80	——
1	960	105	8.6	0.58	±0.17
2003	1490	124	6.3	0.42	±0.13
2	2170	151	3.6	0.27	±0.08
3	2900	197	2.8	0.17	±0.05
4	3502	239	0.16	0.012	±0.004
5	3930	277	~0.02	~0.001	——
6	4310	313	~0.02	~0.001	——

Table I: Observed helium retentions for zircons recovered from various depths in borehole GT-2 and others nearby, Fenton Hill, NM. Samples 2002 and 2003 were recovered for the RATE project and named for the years we analyzed them. Samples 1– 6 are those reported by Gentry et al. **(1982a). Column 4 is the total helium yield (1 ncc = 10^{-9}cm^3 at standard temperature and pressure) per microgram of zircon. Column 5 is the corresponding fraction of the estimated amount of helium deposited in the zircons by nuclear decay. Column 6 is the estimated error in the fraction. All zircons were of length 50–75 μm, except for those from sample 2002, which were not sorted into size groups.**

als. RATE (Vardiman et al., 2003) is a research initiative started in 1997 by seven scientists from three major creationist organizations: the Institute for Creation Research (ICR), Answers in Genesis (AiG), and the Creation Research Society (CRS). In 1998 a personnel transfer reduced the sponsoring organizations from three to two, ICR and CRS. Three board members of CRS have been on the RATE steering committee from the outset: Donald DeYoung, Eugene Chaffin, and Russell Humphreys. CRS members have supported the project by designating donations to the RATE research fund administered by ICR.

The charter for RATE was to make a focused investigation of the problem posed by two large bodies of geoscience evidence for (A) large amounts of nuclear decay having occurred, and (B) a young world. From the start, several members of the steering committee were convinced that episodes of greatly accelerated nuclear decay rates had occurred within thousands of years ago. For the preservation of life, such episodes seem possible only under special circumstances: (1) before God created living things, (2) after the Fall but well beneath the biosphere, and (3) during the year of the Genesis Flood, when the occupants of Noah's ark would be safe from most radiation (Humphreys, 2000, pp. 340–341). Accordingly, the steering committee planned a research program to test the accelerated decay hypothesis, and they wrote a book (jointly published by ICR and CRS) outlining the various projects (Vardiman et al., 2000).

In developing our plans for the helium experiments, we calculated what diffusion rates would be necessary to produce Gentry's reported helium retentions if the zircons were (a) 6,000 years old, or (b) 1.5 billion years old. Figure 4 shows the graph we published (Humphreys, 2000, p. 348, Figure 7) of the resulting two models.

Figure 4, the prediction, is a typical *Arrhenius* plot. The vertical axis shows *diffusivities* (giving rates of diffusion) logarithmically. The horizontal axis shows inverse absolute temperature (1000 divided by degrees Kelvin) linearly. High temperatures are on the left and low temperatures are on the right. Diffusion data on such a plot usually fall into one or two straight lines (Humphreys et al., 2003a, § 3). Notice that the diffusion rates in the Uniformitarian model are over 100,000 times slower (to retain helium 1.5 billion years) than those in the Creation model (6,000 years). Such a large difference made it likely that experiments would be able to distinguish between the two models, so we began seeking ways to conduct or commission such measurements.

Experiments begin

In 2000, through an intermediary, we contracted with a well recognized expert (Humphreys et al., 2003a, § 5) to

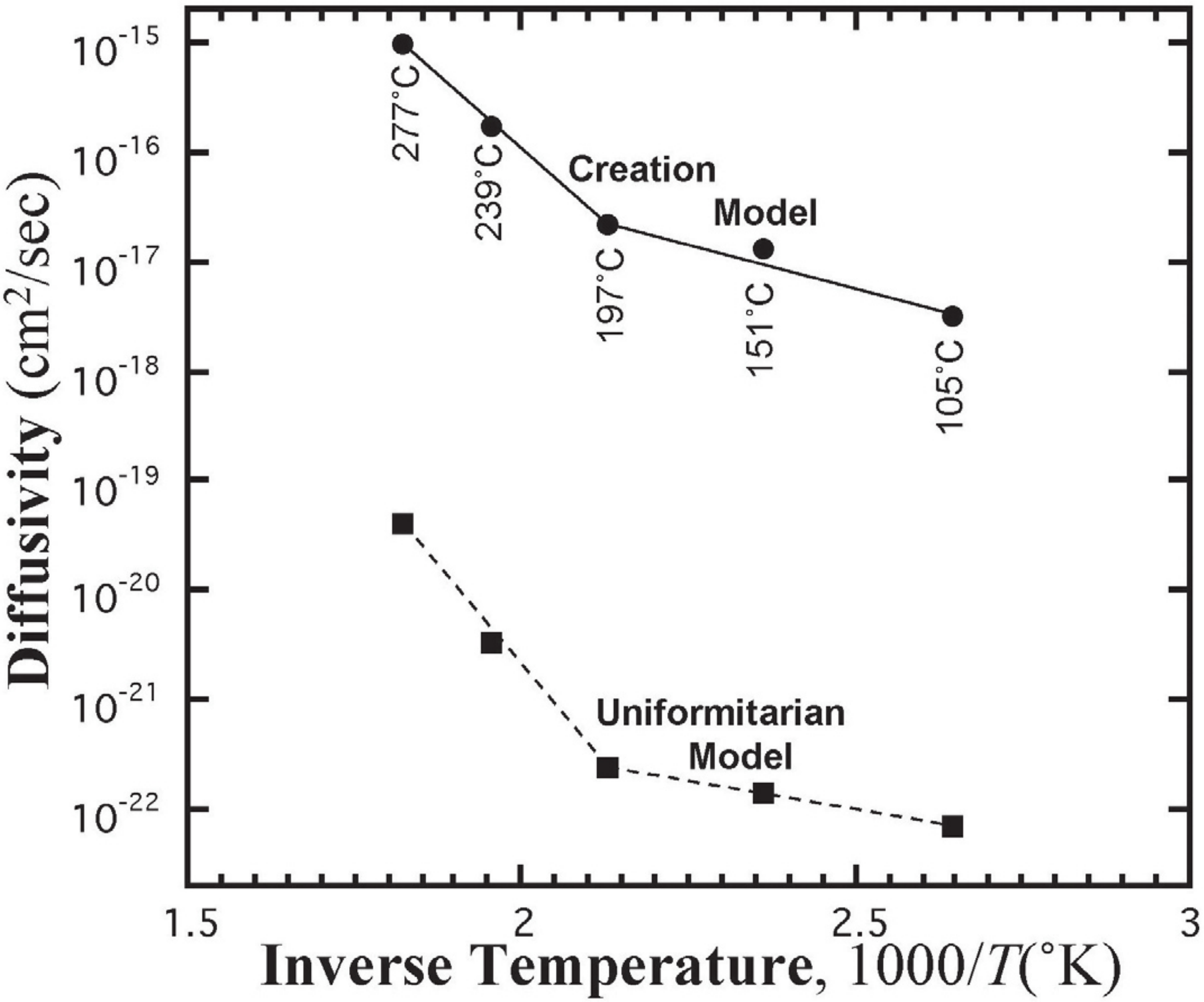

Figure 4. Predicted (Humphreys, 2000, p. 348, Figure 7) helium diffusion rates necessary to retain the observed amounts of helium (Table I) for (a) 6,000 years (Creation model), or (b) 1.5 billion years (Uniformitarian model).

measure helium diffusion rates in biotite, the mineral we thought was the main restriction to helium loss from the zircons. We sent the experimenter biotite we had on hand, from the Beartooth Gneiss in Wyoming near Yellowstone National Park. In early 2001 he sent us data, the first ever reported on biotite. Also in 2001, we received a preprint of a paper reporting helium diffusion rates measured in zircons from Nevada (Reiners et al., 2002).

These data quickly showed us that the zircon itself, not the surrounding biotite, was the main restriction to helium outflow. But we found that when we modified the theoretical model accordingly (see appendix of this paper, page 14, discussion on change 5), the diffusivities in our prediction changed by less than 0.5 percent (Humphreys et al., 2003a, § 6). So the numbers of the prediction in Figure 4 are valid, but they should be applied to zircon, not biotite.

The Nevada zircon data agreed encouragingly with our prediction, but did not extend to low enough temperatures to overlap it. Moreover, those zircons were not from the New Mexico site. Neither were the biotites. Because helium diffusion in minerals can vary significantly from site to site, we decided to get data on biotites and zircons from the same borehole, GT-2, from which Gentry's zircons came. Los Alamos National Laboratory kindly gave us several GT-2 core samples from a depth of 750 meters. That is somewhat shallower than Gentry's samples, but still in the same rock unit. We sent one of them to Activation Laboratories in Ontario, Canada, where Jakov Kapusta extracted the biotite and zircons. He did not separate the zircons into size groups. He measured the uranium and lead isotopes in three of the zircons, finding a ^{206}Pb/^{207}Pb age of 1439 ± 2 million years (Humphreys et al., 2003a, Appendix A). That is within a few percent of the published age for zircons deeper in the borehole (this paper, page 1).

Then we sent both the biotite and the zircons to our experimenter. He sieved the biotite sample to get flakes between 75 and 100 µm, but he used all the zircons Kapusta extracted, regardless of size. Size of crystals (*effective radius*) is important in converting the raw data into diffusivities. He sent us the raw data in 2002.

As before, in the temperature range of interest to us, the biotite diffusivities were about one order of magnitude greater than those of zircon. That confirmed that the zircon rates were more important. Those data lined up well with the prediction, but again did not extend to low enough temperatures to overlap it (Humphreys et al., 2003a, § 8, Figure 8).

After that, in the summer and fall of 2002, we tried several times to get lower-temperature data. However, we only discovered several wrong ways to make such measurements. First, we asked the experimenter to do new runs on the same batch of zircons, but at lower temperatures. The results were ambiguous, an effect we decided was due to exhaustion of helium from the smaller zircons in the batch (Fechtig and Kalbitzer, 1966, § 2.5, p. 72).

Second, we sent the experimenter a new set of zircons from the same depth in GT-2 and asked him to sieve out crystals in the 50–75 µm size range. Before sieving, he decided to leach the crystals in cold concentrated hydrofluoric acid (HF) to remove flecks of biotite clinging to them. Though the technique was new, it seemed reasonable. However, the values of D/a^2 he then obtained were over fifty times higher than all previous zircon data, both ours and published. Scanning electron microscope images done later (see next section) revealed severe pitting and cracking in the HF-treated zircons. That would allow helium to leave the zircons much faster than normally.

These were all the data we had by February 2003, the deadline for the final version of our conference paper (Humphreys et al., 2003a).

Latest results arrive in mid-2003

In the fall of 2002, we acquired new samples from borehole GT-2, this time from a depth of 1490 meters. That is between the depths of Gentry's samples 1 and 2 (see Table I). We sent them to Activation Laboratories, where Kapusta extracted both biotites and zircons. This time he sorted the zircons into several size groups, getting about 1200 crystals in the size range Gentry used, having lengths of 50–75 µm.

Figure 5 shows a scanning electron microscope (SEM) image of one such zircon. Mark Armitage obtained the image in his newly established microscopy laboratory at ICR, where he also obtained SEM images of the HF-treated zircons the previous section mentioned. In the spring of 2003, we sent our experimenter the 50–75 µm zircons, along with the biotites. This time we asked him (a) not to etch the crystals in HF (unnecessary because no sieving was needed) and (b) to get zircon diffusivities at lower temperatures. We also asked that he measure more precisely the total helium per unit mass in both the zircons and the biotites. In July 2003, one month before the conference, we received his results.

As usual, the experimenter measured the rate of helium release at various steps of temperature. Then he placed that data into standard formulas to calculate D/a^2, where D is the diffusivity and a is the effective radius of the crystals. Column 6 of Table II shows the resulting values of

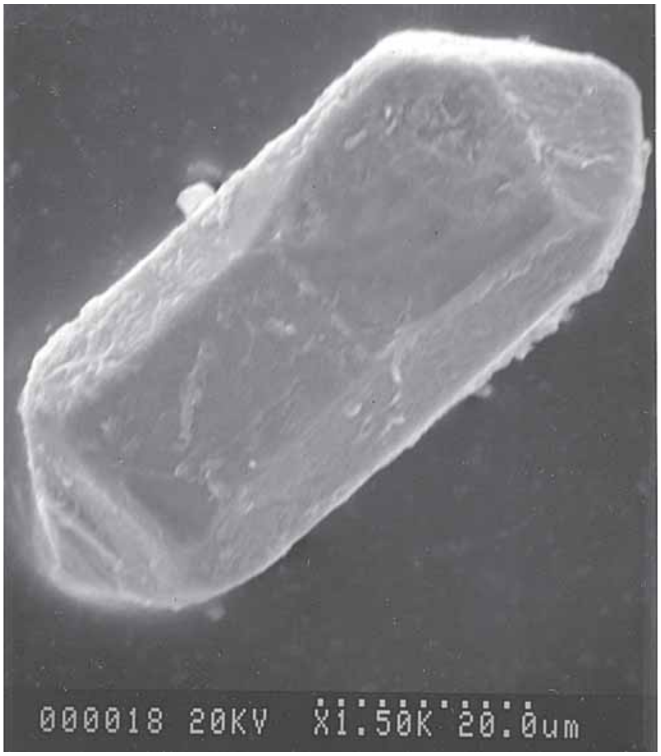

Figure 5. Scanning electron microscopy photograph by Mark Armitage of zircon from group selected by size by Jakov Kapusta.

Step	Temp. (˚C)	He (ncc)	Time (sec)	Cum. Fract.	D/a^2 (sec^{-1})	D (cm^2/sec)
1	50	1.91E-05	3660	1.41E-08	4.73E-21	
2	100	3.82E-03	3660	2.83E-06	1.91E-16	
4	200	3.17E-01	3600	0.000256	1.58E-12	
5	250	1.32E-01	3660	0.000354	1.41E-12	
6	300	3.43E-01	3660	0.000606	5.78E-12	
7	350	2.97E+00	3660	0.002798	1.78E-10	
8	400	9.86E+00	3600	0.010072	2.27E-09	
9	450	4.28E+01	3660	0.041626	3.89E-08	
10	500	1.48E+02	3600	0.150546	5.55E-07	4.99E-12
11	475	3.93E+01	3660	0.179567	2.63E-07	2.37E-12
12	425	4.90E+00	3600	0.183185	3.72E-08	3.35E-13
13	375	6.29E-01	3660	0.183649	4.75E-09	4.28E-14
14	325	7.77E-02	3600	0.183706	5.98E-10	5.38E-15
15	275	1.01E-02	3660	0.183714	7.64E-11	6.88E-16
16	225	3.56E-03	7260	0.183716	1.36E-11	1.22E-16
17	175	7.78E-04	7260	0.183717	2.97E-12	2.68E-17
18	205	2.03E-03	7200	0.183718	7.81E-12	7.03E-17
19	255	4.25E-03	3660	0.183722	3.22E-11	2.90E-16
20	305	3.03E-02	3600	0.183744	2.33E-10	2.10E-15
21	355	2.41E-01	3660	0.183922	1.83E-09	1.65E-14
22	405	1.94E+00	3600	0.185352	1.50E-08	1.35E-13
23	455	1.47E+01	3600	0.196188	1.18E-07	1.06E-12
24	505	8.09E+01	3660	0.255886	7.87E-07	7.09E-12
25	460	1.35E+01	3660	0.265832	1.57E-07	1.41E-12
26	410	1.86E+00	3660	0.267207	2.23E-08	2.00E-13
27	360	2.46E-01	3600	0.267389	3.00E-09	2.70E-14
28	310	3.18E-02	3660	0.267412	3.82E-10	3.43E-15
Total helium yield: 1356 ncc at STP (includes fusion step). Mass: 216 µg						

Table II: Latest (2003) helium diffusion data for 50–75 µm length zircons from borehole GT-2 at a depth of 1490 meters. Column 3 is the amount of helium released (ncc defined in Table I) at the given temperature step. Column 4 is the time at each step. Column 5 is the cumulative fraction of the total helium yield (at bottom of table). Column 6 is the value of D/a^2 estimated by the experimenter according to standard formulas, where D is the diffusivity and a is the average effective radius. Column 7 is the value of D assuming $a = 30$ µm, and omitting steps 1–9 according to advice from the experimenter (see below).

D/a^2 for the zircons. The standard formulas (Fechtig and Kalbitzer, 1966, p. 71) assume that the initial distribution of helium in the zircons is uniform. But in reality, the zircons would have a "rounded" helium-versus-radius profile due to the *in situ* helium loss into the biotite. That is, less helium would emerge during the initial heating steps, because the outer regions of the zircon would be helium-depleted. In that case, said the devisers of the standard formulas (Fechtig and Kalbitzer, 1966, p. 71), "The apparent diffusion constants will come out too low, and the activation energies too high." In his report on the 2002 zircon runs (Humphreys et al., 2003, Appendix C), our experimenter advised us that to account for this effect, we should ignore the first set of increasing-temperature steps in his runs. For the 2003 zircons, he reported that we should treat them just the same. Accordingly, we ignored

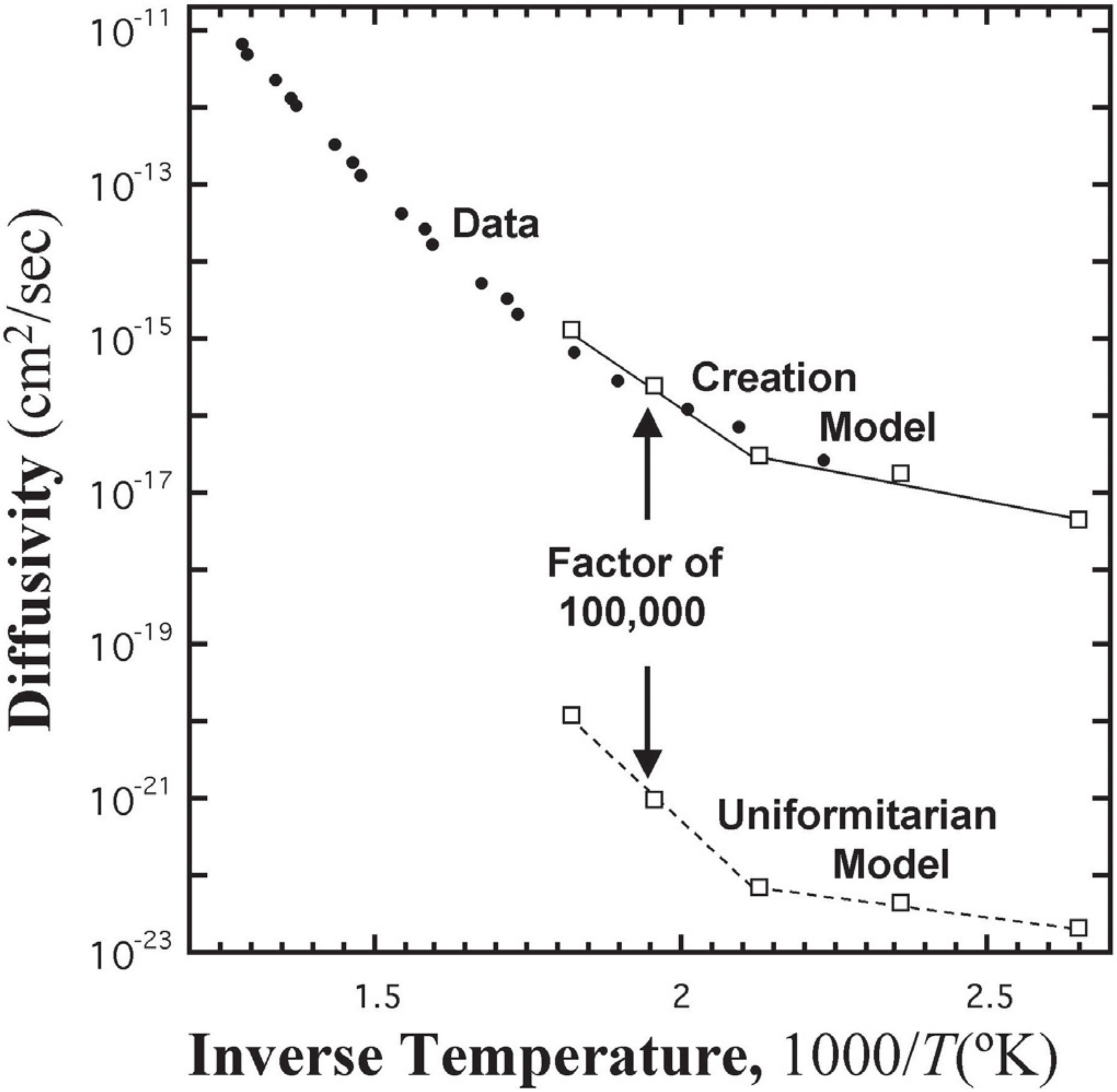

Figure 6. Recent (2003) zircon diffusion rate data compared with the Creation and Uniformitarian models shown in Figure 4.

steps 1–9 in calculating *D*. A more sophisticated analysis could probably extract accurate values of *D* from the raw helium-time data for those steps, but we leave that work for later research.

The effective radius *a* for zircons is about half their length (Humphreys et al., 2003a, § 6), which in this case gives us an average value for *a* of about 30 μm. Multiplying column 6 by the resulting value of a^2 gives us values of the diffusivity *D* for points 10-28, which we show in column 7 of Table II.

Figure 6 shows those values, plotted with the two models for comparison. The data agree with the predicted Creation model—as close as errors in the data and approximations in the model would lead us to expect. The data points extend past the "knee" of the model at 197° C, into the lower-temperature "defect" region determined by radiation damage in the crystals. This was quite important to examine, because the defect part of the curve can vary greatly from site to site (Humphreys et al., 2003a, §§ 3, 4). Even in the defect region, the data agree quite well with the model. It is not often in science that experimental data so clearly validate a pre-published numerical model!

The data also resoundingly reject the Uniformitarian model. The points of that model are the values of diffusivity required to retain the observed amounts of helium for 1.5 billion years at *today's* temperatures in the rock unit. However, uniformitarian thermal models of the rock unit require that the temperatures have been *higher* in the past (Kolstad and McGetchin, 1978; Harrison et al., 1986; Sakada, 1989). So the points of our Uniformitarian model are below the average temperatures during the alleged eons. A more accurate depiction would slide the Uniformitarian model points horizontally leftward to represent the allegedly higher average temperatures. That would make the vertical gap between that model and the data even larger (Humphreys et al., 2003b, see poster and extended temperature range Arrhenius plot at its bottom). Thus the Uniformitarian model in Figure 6 is very generous to uniformitarians, minimizing the gap. Even so, the data points are about 100,000 times higher than the model points. Uniformitarianism has totally failed this experimental test!

We can also compare the new diffusivities with the observed retentions to calculate the age of the zircons. Doing that point-by-point gives an average of 5,681 years with a sigma (square root of variance) of 1,999 years (Table III). We round that off to 6,000 ± 2,000 years.

Note carefully: Our diffusion dating method above differs entirely from the "helium dating" of (U-Th)/He chronometry (Reiners, 2002). Very crudely, the difference is this: (U-Th)/He chronometry divides the number of helium atoms *in* a crystal by *nuclear* decay rate. Diffusion dating divides the number of helium atoms *lost* from the crystal by the *diffusion* rate. It appears the practitioners of (U-Th)/He chronometry, in their unpublished comments upon this work, have not yet understood this basic distinction.

Recent data also close loopholes

After stepwise heating the 216 micrograms of zircons to get the diffusivity data, our experimenter raised the temperature to a high value, holding it there long enough to get the rest of the helium out of the crystals. The total yield of helium from the zircons was 1356 ncc (1 ncc = 10^{-9} cm^3 at standard temperature and pressure = 0.4462 x 10^{-4} nanomole), or 6.05 x 10^{-2} nmol. Dividing by the mass gives us 6.28 ncc/µg, or 303 nmol/g. Multiplying the latter value by the density of zircon,

Sample	Temp. (˚C)	Retention (%)	Diffusivity (cm²/s)	Age (years)
1	105	58	——	——
2	151	27	1.09×10^{-17}	7270
3	197	17	5.49×10^{-17}	2400
4	239	1.2	1.87×10^{-16}	5730
5	277	~0.1	7.97×10^{-16}	~7330
			Average:	5680
			Sigma:	1999

Table III: Estimates of age from Gentry's helium retentions (Table I) and our measurements of helium diffusivity in the same zircons (Table II). Diffusivities here come from best exponential fits to nearby measured points from Table II, column 7. Because our lowest measured value for *D* is at 175° C, we extrapolated 24° C down to the temperature of sample 2 but not 45° C further down to that of sample 1. Then we calculated ages as we did in our *ICC03* paper (Humphreys et al., 2003a, §§ 6 and 8), putting the *x*-values of *ICC03* Table 2 and the values of *D* above into *ICC03* equation 17 to get the values for the age *t* we show above. See our comments on page 9 (related to Figure 8) about sample 3, which above has the greatest deviation from the average age. We report the average and sigma above as 6,000 ± 2,000 years.

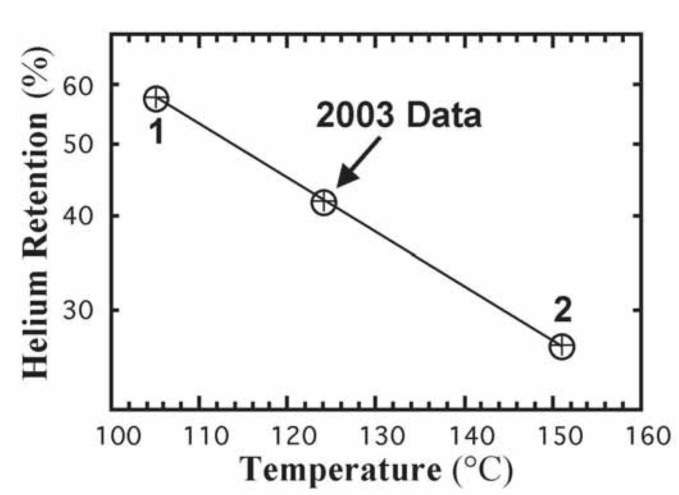

Figure 7. Recent (2003) measurement of percentage helium retained for zircons from 1490 meters depth and 124° C, compared with percent retentions (points 1 and 2) measured by Gentry et al. **(1982a) shown in Table I.**

4.7 g/cm^3, gives us the helium concentration in the zircon: 1320 nmol/cm^3.

For the 5.562 milligrams of biotite, the total yield of helium was 257 ncc, giving 2.06 nmol/g. Multiplying by the density of biotite, 3.2 grams/cm^3, gives us the helium concentration in the biotite: 6.57 nmol/cm^3.

These data are quite useful in closing possible loopholes in our case. First, the 6.3 ncc/μg yield of these zircons is quite consistent with Gentry's retention data. Using Gentry's estimate of radiogenic helium deposited in the zircons, about 15 ncc/μg (which is consistent with our data on radiogenic lead in the zircons), gives us a retention fraction of 0.42 (42%). These zircons came from a depth of 1490 meters, nearly midway between Gentry's samples 1 and 2 in Table I. The interpolated temperature at that depth would be 124° C. Figure 7 shows that our new retention point fits quite well between Gentry's retentions for samples 1 and 2. This confirms the validity of Gentry's retention measurements.

Second, the concentration of helium in the zircon, 1320 nmol/cm^3, is about 200 times greater than the concentration in the surrounding biotite, 6.6 nmol/cm^3. Because the laws of diffusion require flow from greater to lesser concentrations, these data mean that helium is moving *from* the zircons *into* the biotite, not the other way around.

Third, because the average volume of the biotite flakes is hundreds of times greater than that of the zircons (Humphreys et al., 2003a, § 6), the amount of helium in the biotites is on the same order of magnitude as the amount of helium lost by the zircons. That rebuts a specious uniformitarian conjecture (Ross, 2003) that there could have been vast amounts (100,000 times greater than the already-large observed amounts) of non-radiogenic primordial helium in the zircons 1.5 billion years ago.

Our conference paper answers other commonly raised objections, including (1) a nearly automatic response among uniformitarians involving the geoscience concept of "closure temperature" (Humphreys et al., 2003a, § 10), and (2) the possibility that the rock unit was much cooler for most of its history. Two short answers are that (1) the closure temperature for these zircons happens to be relatively low, 128° C, permitting the zircons above that temperature to leak helium freely, and (2) the zircons would have to have been *refrigerated* cooler than *minus* 100° C in order to retain the helium for the alleged eons (Humphreys et al., 2003b, see poster and extended temperature range Arrhenius plot at its bottom). Our conference paper clarifies these points and adds other answers (Humphreys et al., 2003a, §§ 9, 10).

Our new helium retention fraction (0.42 at 124° C) can be treated the same way as we treated Gentry's retention data to make a prediction of diffusion rates. That is, we can use our retention figure to calculate what value of *D* at 124° C would be required if the zircons were 6,000 years old. Figure 8 shows how this "retrodiction" point fits very well with the diffusion rate data and the Creation model prediction.

In Figure 8, we have redrawn the lines in accord with the new data. The largest outlier from the lines is the model point at 197° C. The difference suggests the true retention fraction for that sample might have been about half the fraction Gentry et al. reported (Table 1, sample 3). Whatever the cause, a two-fold discrepancy for one point pales into insignificance in light of the whopping 100,000-fold discrepancy between the observed diffusivities and all points of the Uniformitarian model!

Lead diffusion supports our case

Lead also diffuses out of zircon, although much more slowly than helium does. In addition to studying helium, Gentry and his team (1982b) at Oak Ridge also studied lead retention in 50–75 μm zircons from the same rock unit. The deepest sample was from a depth of 4310 meters and a temperature of 313° C. The paper reports, "there was little or no differential Pb loss which can be attributed to the higher temperatures at greater depths." Judging from their experimental error, their results mean that more than 90% of the lead generated by "1.5 billion years" worth of nuclear decay has remained in even the deepest, hottest zircons.

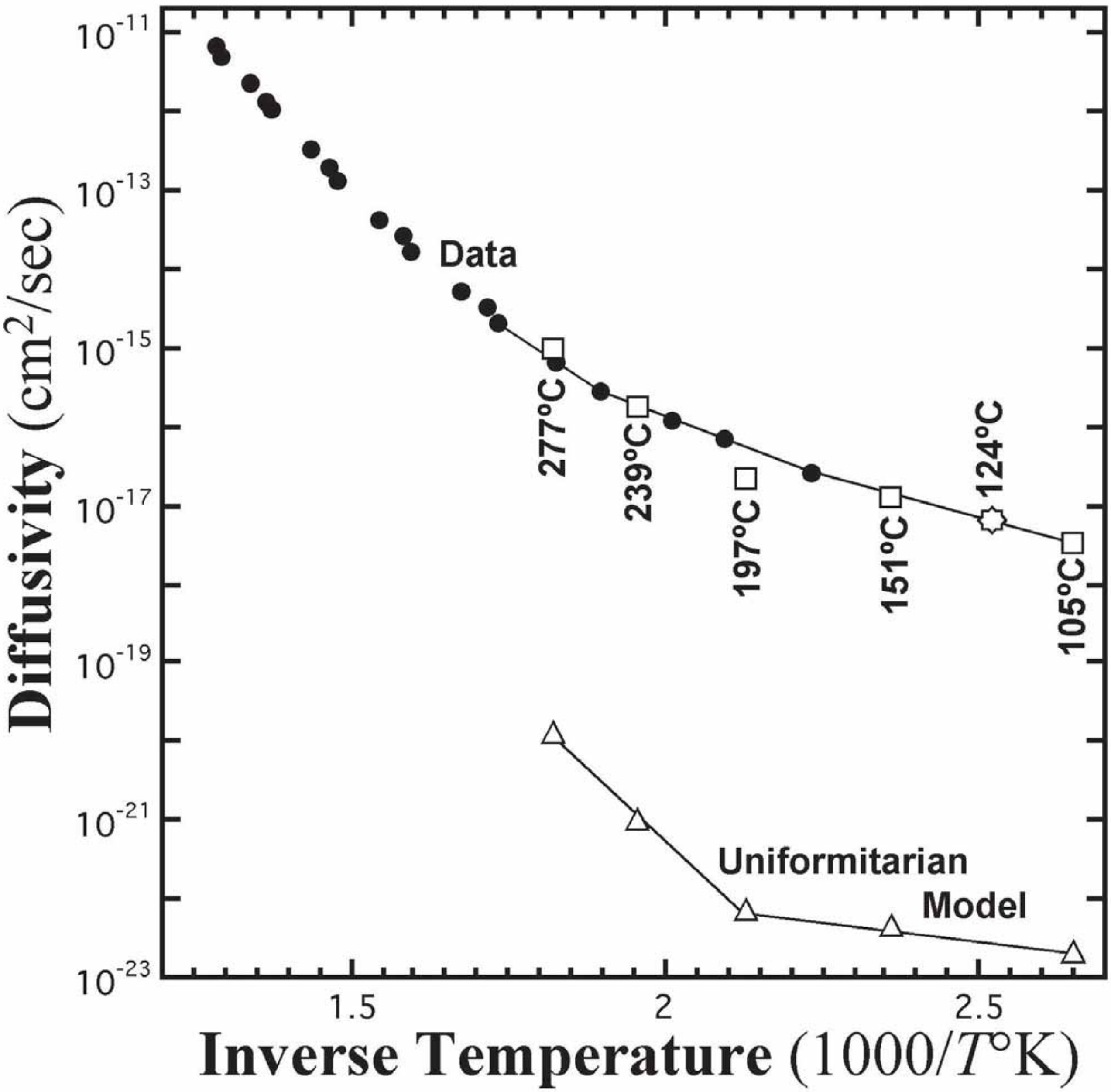

Figure 8. Figure 6 with lines redrawn in accordance with the new (2003) data. Squares with temperatures below them are the diffusivities we predicted in 2000 on the basis of Gentry's reported retentions (Table I, samples 1-5) and an age of 6,000 years. The star with "124° C" above it is the diffusivity required by our new retention datum (Table I, sample 2003, and page 9) and a 6,000-year age.

The diffusion rates for lead in zircon are known, and the article reports that at 200° C, it would take 50 billion years for 1% of the lead to diffuse out of a 50-μm zircon. However, the article does not report such times for higher temperatures. Using the same equation and data (Gentry et al., 1982b, note 16; Magomedov, 1970), we calculate that at the highest borehole temperature, 313° C, a zircon 60 μm long (a = 30 μm) would lose about 50% of its lead in 1.5 billion years. Because the observations show that those zircons did not lose anywhere near that much lead, these data imply an age much less than 1.5 billion years. Thus the lead diffusion data support the young helium diffusion age of the zircons.

Conclusion: a tale of two hourglasses

Experiments have strongly vindicated what creationists felt when Gentry reported the high helium retentions over twenty years ago. The helium indeed could not have remained in the zircons for even a million years, much less the alleged 1.5 billion years. Even more exciting, the most recent experiments give a helium diffusion age of 6,000 ± 2,000 years, which resonates strongly with the date of creation we get from a straightforward Biblical chronology.

Figure 9 illustrates the contrast between this helium age and the radioisotopic age. It shows two different "hourglasses," representing helium diffusion and uranium-to-lead nuclear decay. These hourglasses give drastically different dates.

We have much data to show that we have read the helium hourglass correctly: (1) Uranium-lead data give us the initial amount of sand (helium) in the top half (the zircon). (2) Gentry's measurements, confirmed by ours, give us the present amount of sand in the top half. (3) Our diffusion rate experiments show how fast sand is presently trickling (diffusing) out of the top half into the bottom half (the biotite). (4) Our measurements show roughly the right amount of sand in the bottom half (helium in the biotite). (5) A third hourglass representing lead diffusion, though crude, agrees with the helium result.

For the nuclear decay hourglass, we also know similar things: present amounts in the top half (uranium in the zircon), the present trickling rate (nuclear decay rates), and the amounts in the bottom half (lead isotopes in the zircon). The large amount of helium, the actual alpha particles from the decays, confirms that a large amount of nuclear decay has taken place.

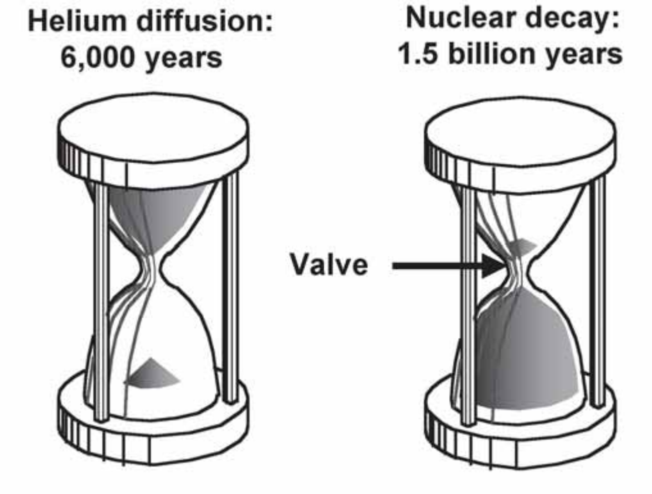

Figure 9. Two hourglasses representing dating by (a) helium diffusion and (b) uranium-lead decay. "Valve" represents possible nuclear decay acceleration.

One way to reconcile these two hourglass readings is to suggest that one of them has a "valve" at its bottleneck controlling the trickling rate, a valve that was adjusted drastically in the past, possibly by direct intervention from God.

Long-agers might want to imagine that the valve is on the helium hourglass, and that for billions of years, diffusion rates were over 100,000 times slower until a few thousand years ago. But diffusion rates are tied straightforwardly to the laws of atomic physics, which are in turn intimately connected to the biochemical processes that sustain life. It is difficult to imagine any such drastic difference in atomic physics that would have allowed life on earth to exist.

On the other hand, it is much simpler to imagine that the valve is on the nuclear decay hourglass. Nuclear forces affect only a tiny region at the center of the atom. They have very little effect on the electronic structure of the atom or its chemical interactions. Moreover, a relatively small change in nuclear forces can cause a billion-fold acceleration of nuclear decay rates (Chaffin, 2003; Chaffin, 2000; Humphreys, 2000). Finally, the preponderance of Biblical and geoscience evidence for a young world (Humphreys, 2000, pp. 337–339) points to a change that would only affect dating methods which depend on slowly-decaying nuclei.

Thus our new diffusion data support the main hypothesis of the RATE research initiative: that God drastically accelerated the decay rates of long half-life nuclei during the earth's recent past. For a feasibility study of this hypothesis—including God's possible purposes for such acceleration, Biblical passages hinting at it, disposal of excess heat, preserving life on earth, and effects on stars—see Humphreys (2000, pp. 333–379). The last three problems are not yet fully solved, but we expect to see progress on them in future papers.

Other RATE projects (Vardiman et al., 2003) are providing other very exciting evidence supporting the hypothesis, such as precision radioisotope measurements (Snelling, et al., 2003a), carbon 14 in fossils and diamonds (Baumgardner et al., 2003a, 2003b), nuclear theory (Chaffin, 2003), and new radiohalo evidence (Snelling and Armitage, 2003; Snelling et al., 2003b). After submitting this paper for publication, we presented these new results in a poster and an abstract at the American Geophysical Union Annual Fall Meeting in San Francisco in December 2003 (Humphreys et al., 2003b). We also presented results from two other RATE projects there (Baumgardner et al., 2003b; Snelling et al. 2003b). RATE hopes to release both a final technical report and a layman's book (authored by Don DeYoung) in the fall of 2005. These are exhilarating days for creation science!

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Appendix: Responding to a Critic

Recently a critic sought very hard to find loopholes in our arguments. While none of his points had any significant impact on our conclusions, it is worthwhile to review the specifics of his critique and answer them here. The critic felt a crucial issue was the possibility that the interface between zircon and biotite might slow or stop helium diffusion because of helium having different *chemical potentials* or *solubilities* in those two minerals, or because of *interface resistance* between them due to other causes. In the next three sections we will explain those terms and quantify their effects.

Differences in chemical potential

A diffusion theorist (Manning, 1968, § 5-3, p. 180, equation 5-36) expresses the *chemical potential*, μ, for helium atoms constituting a fraction N of all the atoms in a crystal at temperature T as the sum of two parts:

$$\mu = kT \ln N + \mu' \tag{A1}$$

where k is the Boltzmann constant. The first term on the right, the "entropy of mixing," contains no information related to the forces between atoms. The second term, μ', is of interest. It is the contribution from all other factors, particularly the interaction energy between helium and the other atoms of the crystal.

The same theorist (Manning, 1968, § 5-3, p. 180, equations 5-37 and 5-39) then expresses the flux J of helium atoms in the x-direction through a region with diffusivity D as:

$$J = -D\frac{\partial C}{\partial x} - \frac{DC}{kT}\frac{\partial \mu'}{\partial x} \tag{A2}$$

The first term on the right represents ordinary diffusion. It is the second term that represents an additional flux due to a driving force, the gradient of μ'. This force originates in whatever chemical attraction the helium atom might have for the atoms of the crystal in which it resides. Inside the crystal, these forces average to zero, but at the interface with another crystal, there may be a jump in μ'. If a helium atom were to have greater chemical attraction for the atoms of zircon than for the atoms of biotite, that would result in a force at the interface hindering its outward motion into the biotite. The question we need to address is, "Just how great is the effect?"

Because helium is one of the noble gases, we might suspect that it would have very little chemical attraction for any other atoms. In fact, helium is the least chemically active of all the noble gases (Holloway, 1968, p. 45, Table 2.1). Nevertheless it does exhibit a faint attraction for other atoms. Theory and experiments (Wilson et al., p. 936, Table XI) show that helium atoms adhere very slightly to the surfaces of alkali halide crystals, with interaction potentials on the order of a few hundred calories per mole of helium. The largest estimated potential is 293.4 cal/mol, at the "saddle point" between the sodium and fluorine ions at the surface of NaF. The smallest potential listed is beside a chlorine ion at the surface of NaCl, 111.7 cal/mol.

The difference of those potentials provides an estimate of the difference of μ' at an interface between NaF and NaCl: 181.7 cal/mol, or 0.00788 eV per helium atom. Because noble gases have a greater chemical affinity for halides (Holloway, 1968, p. 89) than for most other ions, the above number is almost certainly greater than the corresponding number for the silicate minerals we are considering. So at the interface between zircon and biotite, we can take the following value as a generous upper bound on the magnitude (absolute value) of the difference in μ':

$$|\Delta\mu'| < 0.0079 \text{ eV/atom} \tag{A3}$$

Now we need to quantify the effect of that difference on the flux of helium atoms in equation A2. As we did in our ICC paper (Humphreys et al., 2003a, § 6, after equation 9), we assume for simplicity that the diffusivity D is the same for biotite as for zircon, and therefore constant across the interface. Because the observed value of C in the biotite is hundreds of times smaller than in the zircon (this paper, § 5), the magnitude of the change in concentration, ΔC, across the interface is nearly equal to the concentration C in the zircon:

$$|\Delta C| \approx C \tag{A4}$$

Assuming that the changes ΔC and $\Delta\mu'$ both occur within roughly the same small distance δx, the width of the interface, the helium flux J in equation (A2) becomes:

$$J \approx -D\frac{\Delta C}{\delta x} - D\frac{\Delta C}{\delta x}\frac{\Delta \mu'}{kT} \tag{A5}$$

To make this mechanism a viable possibility for rescuing the uniformitarian scenario, the second term on the right-hand side must be: (1) of opposite sign to the first

($\Delta\mu'$ must be negative, meaning helium is more attracted to zircon than biotite), and (2) large enough to reduce J to a level about 100,000 times lower than what the first term alone would give. That could reduce the helium flow enough to let the zircon retain the helium for 1.5 billion years. In the coolest sample we analyzed, at 100° C, the average thermal energy kT of the atoms was 0.0321 eV. Then $|\Delta\mu'/kT|$ would be less than 0.246 for that sample, and even smaller for the hotter samples. That makes the magnitude of the second term less than 25% of that of the first term for all the samples we analyzed. However, our upper bound on the value of $\Delta\mu'$ based on the chemical affinity of helium with alkali halides is likely at least an order of magnitude larger than the actual value. So a magnitude of the second term less than 2.5% of the first term is probably more realistic. The second term obviously does not provide the large reduction of helium flow the uniformitarian scenario requires.

However, an even more basic consideration shows our measurement procedures have already accounted for such differences. We note that the magnitude of $\Delta\mu'$ is several times greater for a zircon-vacuum interface than for a zircon-biotite interface. That is, the attraction of a helium atom for the biotite it is entering partly cancels its attraction for the zircon it is leaving. But our experimenter measured the diffusivities of zircons in a *vacuum*. So the zircon diffusivities we report in Table II already include the effect of a *stronger* interface reflection than would exist for the zircons in their natural biotite setting. So however strong or weak the "chemical potential" interface effect may be, our measured diffusivities already account for it in a way that is generous to the uniformitarian model.

Solubility

Solubility in this context corresponds to the maximum number of helium atoms one gram of crystal can absorb per bar of pressure (Weast, 1986, p. 101). The critic used the term as a measure of the difficulty with which a helium atom could enter biotite. As a hypothetical example, if all the spaces between atoms in biotite were much smaller than the diameter of a helium atom, then helium could never enter the crystal, so helium would be completely insoluble in biotite. If an alpha-decaying nucleus inside the biotite were to generate a helium atom therein, then the atom could distort the lattice and push its way out. The crystal would have a small but non-zero helium diffusivity and zero solubility.

However, real minerals have non-zero solubilities. The solubilities of helium in obsidian and basaltic glass between 200 and 300° C, for example, are on the order of 50 nmol/g per bar (Jambon and Shelby, 1980, Fig 2c) and on the same order in other minerals (Broadhurst et al., 1992). The solubility of helium in biotite has not been measured (we were the first to measure even diffusivity for that pair of substances), so we must find a way to estimate its effect in this case.

One way is to consider the interaction potential part μ' of the chemical potential we mentioned in the previous section. For a helium atom near the surface of a crystal, the gradient of the potential is negative, making the force attractive. But the force can become repulsive for a helium atom entering a tightly packed crystal. For example, imagine that a helium atom has to come very close to an oxygen atom. If their nuclei are closer together than 2.94 Å (1 Å = 1 angstrom = 1×10^{-8} cm ≈ diameter of a neutral hydrogen atom), the force between the two atoms is repulsive (Kar and Chakravarty, 2001, Table I σ_{os} column and gradient of their equation 2).

However the space between silicate sheets in biotite is much larger than that (Deer et al., 1962, Vol. 3, pp. 1–3, 55; Dahl, 1996, Figure 1 and Table 4). The large spacing is the reason the diffusivity of helium in biotite (Humphreys et al., 2003a, Figure 6b) is about ten times higher than in zircon, which has tighter spacing (Deer et al., 1962, Vol. I, pp. 59–68). The relative spacings and diffusivities imply the solubility of helium in biotite is greater than in zircon, so the force related to solubility, included in the gradient of μ', would tend to push helium atoms out of zircon and into biotite. Hence their respective solubilities would not hinder helium outflow from the zircon but rather enhance it.

Interface resistance

Our critic also postulated some type of *interface resistance* arising from special distortion of the crystalline lattices at the interface between zircon and biotite. We can model such hypothetical interface resistance (Crank, 1975, p. 40, § 3.4.1) as a very thin layer of very low diffusivity between the zircon and biotite. The concentration of helium would drop rapidly across the layer, approximating a discontinuous change of concentration between zircon and biotite. Such a layer might consist of physically or chemically altered zircon or biotite, and it would be at most a few dozen angstroms thick.

Let us estimate how low the diffusivity D of the interface would have to be in order to retain the helium in the zircon for 1.5 billion years. Since D is supposed to be much lower than the diffusivities of both zircon and biotite, we can approximate the situation as a hollow sphere with a wall of diffusivity D having an inner radius a and outer radius b. A source (representing nuclear decay) inside the sphere generates helium at a steady rate q_0, and the helium diffuses through the wall out into a vacuum outside the sphere. Textbooks show (Carslaw and Jaeger, 1959, §9.2, p. 231, example IV, $Q_0 \to q_0$, $K \to D$, $v_1 \to C$, $v_2 \to 0$) that the steady-state helium outflow q_0 is

$$q_0 = 4\pi D C \frac{a b}{b-a} \tag{A6}$$

where C is the steady-state concentration of helium inside the sphere. Taking the wall thickness δ ($b = a + \delta$) to be small compared to a ($\delta << a$), integrating q_0 for time t, and C over the sphere volume (Humphreys et al., 2003a, § 7, equation 16) gives us the ratio of helium retained, Q, to total helium generated, Q_0:

$$\frac{Q}{Q_0} \approx \frac{a\,\delta}{3\,D\,t} \tag{A7}$$

Turning this around gives us the interface diffusivity D required to retain a fraction Q/Q_0 of helium for time t in a zircon of effective radius a surrounded by an interface of thickness δ:

$$D \approx \frac{a\,\delta}{3\left(Q/Q_0\right)t} \tag{A8}$$

For example, with an interface thickness of 30 angstroms, $a = 30$ μm, and a time of 1.5 billion years, the 17% retention of sample 3 requires an interface diffusivity of

$$D \approx 3.8 \times 10^{-26} \text{ cm}^2/\text{second} \tag{A9}$$

This is over *ten billion times lower* than the diffusivities we measured in biotite (Humphreys et al., 2003, Figure 6b) and zircon (this paper, Figure 8) at the same temperature, 197° C. To see whether this is an achievable value or not, let us examine an example the critic gave for physical alteration of the minerals at the zircon-biotite interface.

The critic suggested that when biotite crystallizes around a zircon, it possibly forms with its silicate sheets (along which are the cleavage planes) everywhere parallel to the surface of the zircon, so that the biotite wraps up the zircon like layers of cellophane. But in the thousands of zircon-containing biotite flakes that we ourselves have observed under the microscope (Snelling and Armitage, 2003; Snelling et al., 2003b), the silicate sheets remain parallel all the way to the edge of the zircon crystal and do not wrap around the included zircons. A Los Alamos report has a photo of a radiohalo in biotite from borehole GT-2 showing the biotite cleavage staying parallel to itself, running right up against the zircon, and not becoming parallel to the zircon surface (Laughlin and Eddy, 1977, Figure 6, p. 18). There is simply no observational support for the critic's hypothesis that layers of biotite envelop an included zircon.

However, for the sake of having a specific illustration of interface resistance, let us indulge the critic and imagine that a few of the biotite layers closest to the zircon wrap around it. We will even imagine that there are no openings in the biotite wrapping at the edges and corners of the zircon faces. In that case, diffusion in the interface would have to take place in the harder direction, perpendicular to the silicate sheets rather than parallel to them.

Let us estimate the diffusivity in that harder direction. Measurements show that in biotite, "Ar diffusion is ~500 times faster parallel to the silicate sheets than perpendicular to the silicate sheets" (Onstott et al., 1991, § 7, p. 166). Because a helium atom has a smaller diameter, 2.28 Å, than an argon atom, 3.35 Å (Kar and Chakravarty, 2001, Table I σ_{ss} column), then for helium there should not be as great a difference between "parallel" diffusivity $D_{||}$ and "perpendicular" diffusivity $D_{\perp}$. So for helium in biotite, the ratio $D_{||} / D_{\perp}$ should be less than 500. Our measurements for helium in biotite (Humphreys et al, 2003, Figure 6b) gave, for example, $D_{||} = 8.6 \times 10^{-15}$ cm²/s at 200° C. Dividing that diffusivity by 500 gives us a lower bound on the diffusivity in the difficult direction:

$$D_{\perp} > 1.7 \times 10^{-17} \text{ cm}^2/\text{second} \tag{A10}$$

That is over *400 million times greater* than the maximum diffusivity, equation (A9), that a 30 Å interface could have to retain the helium for 1.5 Gyr. Hence such a hypothetical mechanism fails to account for the high helium retention we document. Moreover, as we have already indicated, there is no observational support for the sort of interface crystallographic structure our critic speculates might exist.

Effects of model assumptions

The critic also explored the effects of several changes in the assumptions of our models: (1) inserting a large interface resistance, (2) greatly increasing the creation model D for biotite, (3) decreasing the uniformitarian model D for biotite from infinity to that of zircon, (4) accounting for anisotropy of biotite and zircon, and (5) changing the effective radius a from our early value of 22 μm (Humphreys, 2000, p. 347) to our more recent and more appropriate value of 30 μm (Humphreys et al., 2003a, § 6, after equation 9).

We have discussed change (1) in the preceding section, showing that it is unrealistic. Change (2) increases the helium loss rate from the zircons by a factor of six, making it less realistic than our assumption, which had a worst-case effect of 30% (Humphreys et al., 2003a, § 6, after equation 9). Change (3) decreases the loss rate from zircons by a factor of six, but we think it is unrealistic for uniformitarians to demand an extremely low value of D for the biotite as well as the zircon.

Regarding mineral anisotropies (4), we point out two things: (a) switching from sphere to cylinder geometry (roughly approximating anisotropy effects) for the most important mineral (zircon) would alter the results by less than a factor of two, and (b) even a factor-of-ten reduction in the modeled diffusivity of the surrounding mineral (biotite) would change our results by less than 30% (Humphreys et al., 2003a, § 6 after equation 9). Thus, accounting for biotite anisotropy would affect our results by much less than 30%. As for zircon, anisotropy in it is probably just as negligible as it is in many other similarly shaped crystals, such as quartz. Both our experimenter and other diffusion experts have not assigned a high priority to investigating that possibility.

Change (5), the increase in effective radius a required by our better knowledge of zircons, by itself would have increased the model-required D's by a factor of about two. But our better knowledge also required another model change, from a "bubble" in biotite to a solid in biotite. This second change reduced the D's by about a factor of two. Because the two effects nearly cancelled each other out, the *net* change in predicted D was less than 0.5%. We explained these things in our ICC paper (Humphreys et al., 2003a, § 6), but perhaps not clearly enough.

The critic acknowledged that changes (2-5) would not come anywhere close to eliminating the 100,000-fold discrepancy between our data and any reasonable uniformitarian scenario. But he asserted that the several-fold sensitivity to changes in assumptions means that the close agreement between the creation model and the data was merely accidental. That may be a possibility, but it may also mean we exercised good theoretical judgment in choosing the simplifying assumptions for our prediction.

Closing criticisms

Finally, the critic proposed we postpone publication until (a) further theoretical and experimental investigations would close all alleged loopholes, and (b) until we have much more data supporting our case from boreholes all over the world. We disagree with him. On point (a), detractors can allege loopholes eternally, and we think we have addressed all the so-far-alleged loopholes well enough to place the burden of proof on the detractors.

On point (b), the critic was laboring under a misunderstanding. He reasoned that the possible scarcity of sites with what uniformitarians call "excess helium" meant that sites with high retentions and short helium *diffusion ages* are rare. As we explained at the ends of the introduction and the "Latest results arrive in mid-2003"section in this paper, those two concepts (excess helium and diffusion age) are fundamentally different. But even if he were correct, we feel that the data in this paper are so well established that immediate publication is warranted.

We thank Roger Lenard, a physicist at Sandia National Laboratories in Albuquerque, NM, for his expert advice on chemical thermodynamics, which helped us to prepare the "Solubility" and "Interface resistance" sections above.

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CRSQ: Creation Research Society Quarterly

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