

Answer to a persistent critic of RATE helium research

D. Russell Humphreys

Editor's note: Recently Steve McRae, the owner of a website¹ called "The Great Debate Community", asked Dr Russell Humphreys to respond to recent critiques (of Humphreys' research) by Dr Kevin Henke and Dr Gary Loechel² on McRae's site. Most of the people posting on the website are not Christians and are strongly opposed to young-earth creationism. A few old-earth Christians, such as Loechel and Dr Hugh Ross, occasionally post on the website also, also criticizing the idea of a young world. Here is Humphreys' response, which McRae posted on 7 July 2018.

Critics of my part of the Radioisotopes and the Age of the Earth (RATE) research initiative³ have been numerous ever since the project published a technical book⁴ in 2005 containing the results of our field and laboratory research. The critics don't like the results of my project. I found evidence that tiny radioactive zircon crystals (figure 1) in granitic rock several miles deep in a borehole in New Mexico are only about 6,000 ($\pm 2,000$) years old, in contrast to the 1.5 Ga worth (assuming today's rate) of uranium-to-lead decay the zircons have undergone. The evidence is the remarkably high percentage of helium from uranium decay that the crystals have retained, combined with measurements by an expert showing that helium leaks out of these crystals rather

rapidly. Exhaustive details are in my chapter of the RATE results book.⁵

Figure 2 shows the evidence. The vertical axis, 'Diffusivity', gives the rate of leakage of helium out of the crystals. Notice that the vertical scale is very compressed ('logarithmic'), covering a trillion-fold range of numbers. The horizontal axis is the temperature of the crystals in the borehole and then in the lab. To help non-experts, I've shown the temperature increasing from left to right, rather than the other way around (as in figures 5 and 6 below). The dots show the helium leak rates that Ken Farley measured from these zircons. Dr Farley, at Cal Tech, is a world-class expert on helium diffusion (spreading in, leakage from)

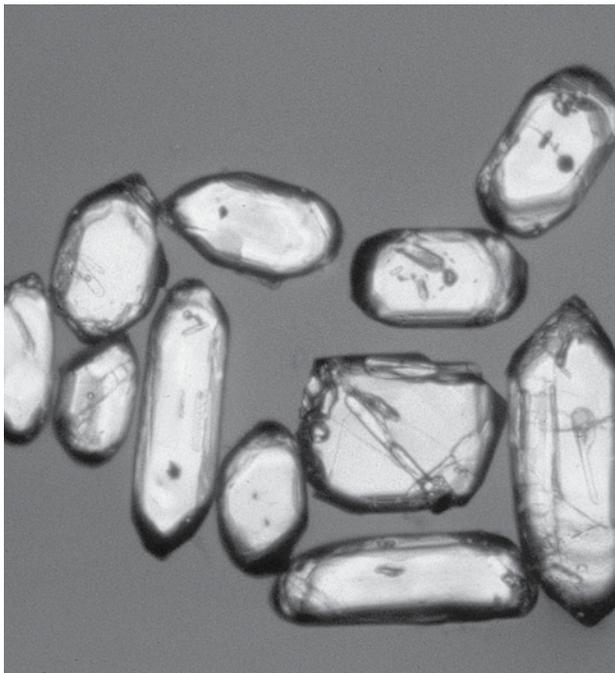


Photo by R.V. Gentry

Figure 1. Microscopic zircons used in this research

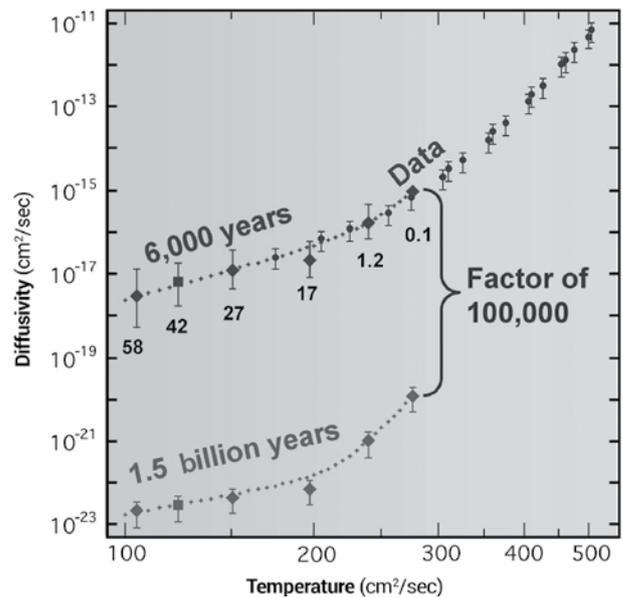


Figure 2. Predicted (diamonds) and measured (dots) helium leak rates ('diffusivity') of zircons. The data fit the 6,000-year prediction very well. Note that temperature increases from left to right, in contrast to figures 5 and 6 below.

in geologic minerals. The black numbers, ‘58’, ‘42’, etc., are the percentages of helium now retained in the zircons as compared to the amount of helium that 1.5 Ga worth of uranium-to-lead decay deposited in them initially. The diamonds labelled ‘6,000 years’ are the diffusivities I predicted⁶ the zircons would be shown to have if they were indeed six millennia old. The data (the dots), taken three years later, confirm the prediction remarkably well. On the other hand, the data are between 100,000 times to a million times higher than the low diffusivities labelled ‘1.5 Billion years’ (diamonds) that would be required for the zircons to retain the measured percentages after that much time.

In other words, a straightforward understanding of the experimental data says the zircons are far too leaky to retain their helium longer than thousands of years, certainly not for millions of years or more.

A short non-technical internet article of mine discusses the above things in more detail and then discusses all the critics of the helium project up to November 2008.⁷ Those include Dr Kevin Henke and Dr Gary Loechelt, who recently made PowerPoint presentations on this site. Upon my request (so I could decide if I wanted to spend time listening to them), both of them sent me (via Steve McRae) some brief outlines of what they said. Kevin Henke had nothing new that I could see, and he himself advised me to concentrate on what Gary Loechelt had to say. So that is what I’ll do here. If you don’t have the time for a detailed discussion, please at least see the epilogue at the end. It explains why this issue is important.

Loechelt’s case in a nutshell

In my opinion, the most significant point in Loechelt’s presentation is encapsulated in table 1 (my caption) from one of his slides:

Table 1. Parameters (E_a and D_0) of straight-line fits to parts of helium diffusion data, and diffusivities (D) at 180°C and 87°C implied by the parameters (from Loechelt, ref. 2; boxes added by Humphreys)

Comparison of published helium diffusion activation energies (E_a) and pre-factors (D_0) to the results from the RATE experiment.				
E_a (kCal/mol)	D_0 (cm ² /s)	D (T = 180°C) (cm ² /s)	D (T = 87°C) (cm ² /s)	Reference
13.9	1.7E-10	3.1E-17	5.7E-19	Humphreys 2004 (low temperature data)
38.1	3.1E-01	1.3E-19	2.3E-24	Humphreys 2004 (high temperature data)
40.4	4.6E-01	1.5E-20	1.4E-25	Reiners 2004
34.9	2.3E-03	3.4E-20	1.5E-24	Cherniak 2009 (perpendicular direction)
35.4	1.7E-01	1.5E-18	5.8E-23	Cherniak 2009 (parallel direction)
38.2	3.0E-02	1.1E-20	1.9E-25	Wolfe 2010
33.0	2.7E-02	3.2E-18	2.5E-22	Guenther 2013 (Mud Tank, parallel)
39.7	2.3E-02	1.6E-21	1.8E-26	Guenther 2013 (RB140, parallel)
38.9	2.7E-02	4.5E-21	6.4E-26	Guenther 2013 (M127, parallel)

You might think that the numbers in the big box represent actual diffusion data measured by the experimenters at the temperatures indicated at the top of the column, 180°C and

87°C. But no, these are *synthetic* data points extrapolated down from high temperatures, about 300°C and higher. Except for the RATE experiments, none of the experimenters made any actual measurements at low temperatures, say from 200°C down to 100°C. The small box shows numbers made from a line drawn through Farley’s measurements for low temperatures, and the numbers represent his results fairly accurately. The big point that Loechelt is trying to make is that the small-box numbers disagree with the big-box numbers so, he assumes, the small-box numbers must be wrong.

Why does he make that assumption? Why does he prefer synthetic numbers over the real ones? The big reason is that he believes the earth is old, so he wants to greatly reduce the low-temperature diffusivities in order that the zircons might retain their helium for 1.5 Ga. The solid line and stars in figure 3 show how he wants to revise the RATE data downward. Notice, however, that the solid line is still significantly higher than the ‘1.5 Ga’ model ... by a factor of 10 on the left up to a factor of nearly 100,000 on the right.

Figure 4 shows Loechelt’s rationale, which was part of the same slide that had his table. Picking up the word ‘anomalies’ from the last line of figure 4, Loechelt is apparently applying it to Farley’s low-temperature data, implying that they are somehow not legitimate. He thinks Fechtig and Kalbitzer⁸ (hereafter abbreviated F&K) recommend that he ignore such measurements and that he should instead extrapolate down from the high-temperature measurements. Loechelt supplied me with five quotes from F&K which he thinks support what he says. But all of them appear to apply only to a special variety of ‘non-volumic’ diffusion, a variety which only

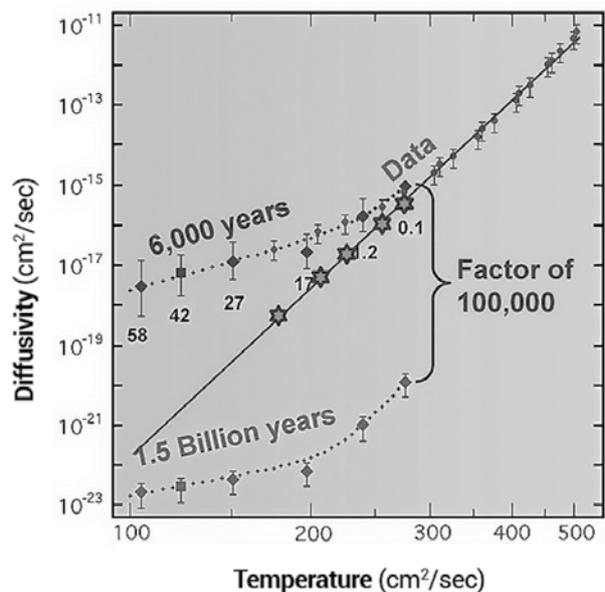


Figure 3. Extrapolated solid line and stars show how Loechelt wants to reduce the diffusivities measured by Farley.

Low Temperature Data

Fechtig, H. and Kalbitzer, S., The diffusion of argon in potassium-bearing solids; in: *Potassium Argon Dating*, Schaeffer, O.A., Zähringer, J. (Ed.), Springer-Verlag, New York, pp. 68-107, 1966.

Fechtig and Kalbitzer

These pioneer developers of the step-wise heating diffusivity measurement technique advocated extrapolating from the high temperature data when anomalies appear at low temperature.

Figure 4. Loechelt's rationale for replacing data with an extrapolation (from Loechelt, ref. 2; underlining by Humphreys)

affects a very small percentage of the total helium in the crystal. I will discuss that case in the next section. Loechelt did not send me the F&K quote below; it says exactly the opposite of what he wants (highlighting mine):

These results on this "simple" system show that the diffusion of argon at low temperatures should not be **calculated** from high-temperature measurements, but that **measurements** have to be performed in the low-temperature interval.

—Fechtig and Kalbitzer (1966), p. 84.

This appears to be a general principle which would reasonably apply to any situation in which significant amounts of a noble gas diffuse out of a crystal differently at low temperatures than at high temperatures. I.e. the diffusion experts prefer measurements over extrapolations. The next section shows why Loechelt thought otherwise.

Confusion over 'non-volumic' diffusion

Non-volumic diffusion includes a special case I here call *surface* diffusion, meaning diffusion of *small* quantities of helium from *at or near the surface* of a crystal, including the surface of cracks in the crystal. This contrasts with volumic diffusion: large quantities of helium coming from within the entire body of the crystal. Noble gas at or near the surface is often "weakly bound" or "loosely bound," and so "will diffuse out easily," say F&K (p. 74). "This *exhaustible* small amount" (F&K, p. 82) of noble gas will normally be released in the earliest part of an experiment, when the experimenter increases the temperature in steps from a low temperature

(say 100°C) up to a high temperature (say 500°C). When the experimenter then decreases the temperature in steps, the subsequent diffusion data will not follow the data from the first upward steps because the 'loose' gas has already left the crystal. Subsequent cycles up or down usually follow the data from the first downward steps. That means one can eliminate the surface diffusion data simply by ignoring the first set of upward steps. For a variety of reasons, Farley recommended that we ignore the data from those initial steps, and that is what we did. So the data we show do not include any surface diffusion. I'll give a specific example a few paragraphs below.

Unfortunately, F&K use a broader definition of non-volumic diffusion. It includes both surface diffusion and

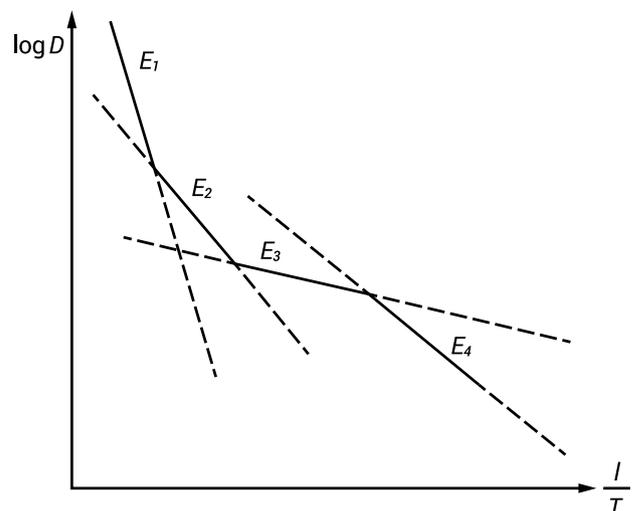


Figure 5. Logarithm of diffusivity D versus inverse (absolute) temperature T (from Fechtig and Kalbitzer, figure 13, p. 85). Math details in the next section. Steepest-slope component is labelled E_1 , while E_2 , E_3 , and E_4 label components with lower slopes. Here, temperature increases from right to left. Figure 6, below, explains this type of graph more fully.

any other diffusion mechanism that gives a shallower slope in figure 5 than the slope of the main high-temperature component, the one with the label E_1 . Most of the other shallow-slope components, besides surface diffusion (which also has a shallow slope), are from sources that are usually distributed uniformly throughout the whole volume of the crystal, such as lattice imperfections (point defects, grain boundaries, dislocations, etc.) and impurities, all of which are very common in naturally occurring minerals.^{8,9} These mechanisms draw upon *all* the helium in the volume, merely offering different pathways of diffusion. We might call this variety of non-volumic diffusion *inexhaustible* diffusion, 'inexhaustible' by comparison with the much more exhaustible surface diffusion.

This is the crucial problem I see with Loechelt’s reasoning: he seems to think that *all* diffusion with low slopes at low temperatures is exhaustible, as exhaustible as surface diffusion. He ignores the most likely mechanisms for low-temperature diffusion, which are as inexhaustible as volumic diffusion. So he thinks we should simply ignore Farley’s low-temperature data, because he thinks it represents only a tiny fraction of the helium in the zircon.

But Loechelt is ignoring important evidence against his assumption. The first nine steps of Farley’s experiment,¹⁰ whose diffusivities he told us to ignore, cooked out 4% of the total helium in the crystal. The very next step, number 10, 1 hour at 500°C, cooked out an additional 11%. I would think that any exhaustible diffusion mechanisms would no longer be represented after that point, and that the remaining 85% of the helium, whether measured at low temperatures or high ones, would be from sources as inexhaustible as volumic diffusion. So there is no reason to ignore Farley’s low-temperature data, as Loechelt wants to do.

Summary of Loechelt’s main strategy:

1. Assume Farley’s low-temperature data are from an *exhaustible* diffusion mechanism yielding less than a few percent of the total helium in the zircon.
2. Ignore the low-temperature data altogether. In effect, that is assuming that the zircons are near-perfect crystals, having almost no defects or impurities.
3. Extrapolate the high-temperature data down to low temperatures.

A math digression

If you have a severe Algebra Allergy, you might want to skip this section. Or you could do like most people do (including physical scientists in a hurry), and simply read the text, look at figure 6 and its caption, and skip the equations themselves. But I must bring this up simply because Loechelt brought this up. He wants to know how I got an equation I used in my general introduction¹¹ to diffusion:

$$D = D_0 \exp\left(-\frac{E_0}{RT}\right) + D_1 \exp\left(-\frac{E_1}{RT}\right) \tag{A}$$

(The symbol ‘exp’ is the exponential function). The answer is that it is simply an *approximate* equation fitting the typical shape of the diffusion curves seen in experimental data. Figure 6 shows the curve I showed in that discussion. Its caption explains what the different symbols mean. We can derive the exact equation for the curve from the general Arrhenius equation (F&K, p. 73):

$$D = D_\infty \exp\left(-\frac{Q}{RT}\right) \tag{B}$$

where D_∞ is a constant giving the overall diffusivity at infinite temperature and Q is the overall activation energy, which can have several components (F&K, p. 74):

$$Q = E_0 + E_1 \tag{C}$$

Now define D_∞ as the product of two constants related to each component of diffusion:

$$D_\infty = D_0 D_1 \tag{D}$$

Now put equations (C) and (D) into eq. (B) and take the natural logarithm of both sides of the result:

$$\ln D = \underbrace{\ln D_0 - \frac{E_0}{RT}}_{\text{High-temp line}} + \underbrace{\ln D_1 - \frac{E_1}{RT}}_{\text{Low-temp line}} \tag{E}$$

This describes the curve of figure 6 more exactly, since the figure’s vertical scale is logarithmic. But in regions where one term is much larger than the other, then equations (A) and (E) give nearly the same diffusivity. This should satisfy Loechelt’s questions about the validity of eq. (A). It doesn’t

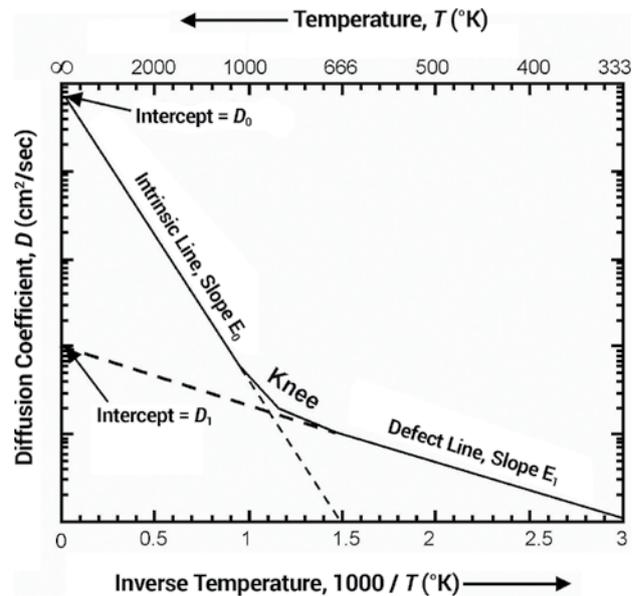


Figure 6. Typical two-slope curve of diffusivity D versus inverse of the absolute temperature T . E_0 and E_1 are the ‘activation energies’ of each diffusion mechanism; D_0 and D_1 are constants given by the intercepts on the left side of the curves above; and R is the gas constant.

matter anyway, because I didn't use eq. (A) in the other sections of the paper.

Did Farley interpret his data correctly?

Loechelt claims that the simple equations (by which Farley converted his measurements to diffusivities) do not apply in the case of the low-temperature data. The equations are given by F&K, pages 70–72. However the equations appear to apply quite generally, and at the bottom of page 71, F&K even discuss using the equations in the low-temperature range ... and they make no caveats about that. Their only warning is to not use results from the initial steps. Those steps are needed only to make the helium or argon have a uniform distribution within the crystal, without a rounded fall-off at its edge. I would add that discarding the diffusivities from those steps also gets rid of the effects of 'loose' helium at or near the surface of the crystal.

Most important, Farley is much more of an expert than Loechelt on helium diffusion in geologic minerals, and Farley had no hesitation about using the F&K equations.

Were the zircons once much colder?

In two letters^{12,13} to *Journal of Creation*, Loechelt argued that temperatures (deep in the granitic formation where the zircons were) were much lower than they are now for a long time, only having risen to today's levels fairly recently, within thousands of years. He doesn't appear to have taken my replies^{14,15} to heart. But now that I understand his thinking better, I can shape my answers more clearly for him. I hope I can clarify the complexities for the readers.

Figure 7 shows why Loechelt wants lower temperatures in the formation. The six black squares connected by lines labelled 'Now' show the diffusivities the zircons would have

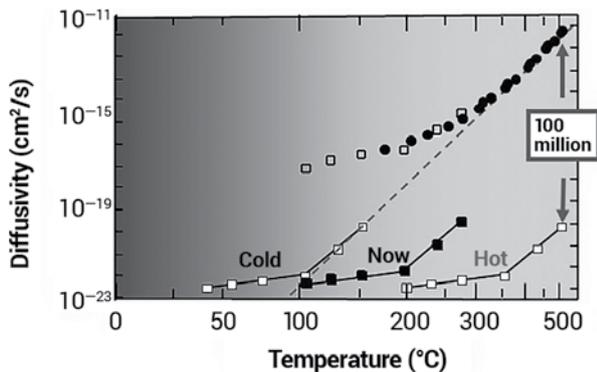


Figure 7. Squares with solid lines show diffusivities necessary with various scenarios for the temperatures in our borehole. Dashed line gives the diffusivities Loechelt wants for the zircons, in contrast to the black circles showing the diffusivities Farley measured.

to have to retain the observed amounts of helium for 1.5 Ga, if the formation had always been at today's temperatures. Those are all below the dashed red line showing the diffusivities Loechelt wants the zircons to have. But if we slide the 'Now' points about 100°C to the left (cooler) to where the white squares are, the warmest (right-most) three white squares will line up fairly well with the dashed red line. These 'Cold' points show the (low) temperatures the formation would have to have to retain the observed amount of helium for 1.5 Ga. The coldest (left-most) three white squares do not coincide with the dashed red line, so Loechelt would have to invoke some additional mechanism to explain the flattening there.

If the formation was at the low temperatures for all but the last few thousand years, the 'Cold' set of white squares would be a reasonable approximation of the diffusivities Loechelt wants.

But what were the actual temperatures the formation experienced in the past? Loechelt cites several papers. One published in 1978 is by two Los Alamos scientists, C.D. Kolstad and T.R. McGetchin. It shows computer simulations of the heating from a nearby volcanic eruption during the Ice Age (Pleistocene). They assumed, as does Loechelt, that the radioisotope dating of the eruption, 1.04 Ma ago, is correct. (Creationists think the eruption was only thousands of years ago and would have an entirely different reconstruction of the temperature history.)

Figure 8 shows their results at different depths in the location of our borehole (called GT-2; figure 9), for two scenarios on the size of the underground magma chamber (pluton) that fed the eruption. The dashed line is at a depth

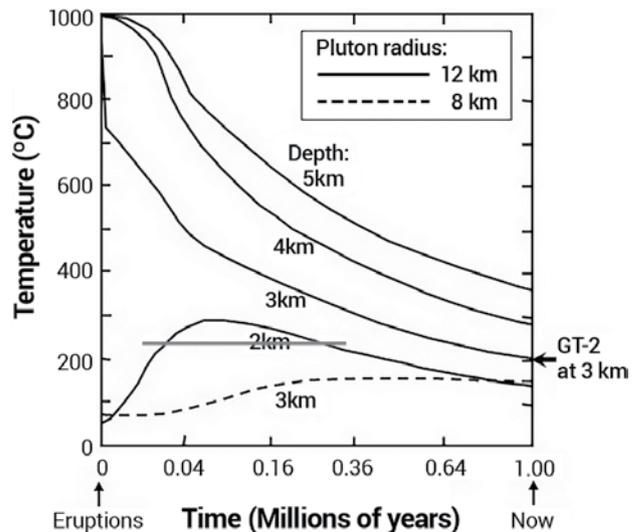


Figure 8. Heat models from Kolstad and McGetchin (1977), showing decreasing temperatures in our borehole (GT-2) at various depths. Timescale gives alleged millions of years after eruption of the nearby volcano. Horizontal line shows a range of possible times for the (time undetermined) peak temperature, 230°C, which Sasada (1989) measured for a depth of 2.6 km.

of 3 km for a pluton radius of 8 km. The solid lines show the temperatures versus time for a 12 km pluton radius at various depths, from 2 km down to 5 km. The arrow on the right shows the measured temperature of GT-2 today at a 3 km depth, about 200°C. The dashed line (8 km pluton radius, depth of 3 km) falls about 50°C short of the measured temperature at that depth. Notice that at about 0.2 Ma after the eruption, the dashed line is almost level. That is, that scenario would have the temperatures be roughly *constant* at today’s observed temperatures for about 800,000 years.

The solid lines (12 km pluton radius) have *much higher* simulated temperatures in the past than now. The line for 3 km depth lines up on the right with today’s measured temperature (arrow labelled GT-2).

Loechelt also cites a *measured* temperature by M. Sasada (1989) for the same site.¹⁶ Looking at fluid inclusions in the formation, Sasada deduced that at a depth of 2.6 km, the temperature rose to a peak of about 230°C at some time, and then subsided. His technique did not assign a time for the peak. In figure 8, I have drawn a horizontal line at 230°C showing a range of possible times for the peak temperature at 2.6 km depth. The measured temperature today at that depth is about 178°C. So Sasada’s measurement says that the temperature at 2.6 km depth *decreased* about 50°C sometime during the past history of the formation. He does say that other inclusions record a minimum temperature of 152°C at the same depth, but there is no way of knowing whether that occurred before or after the peak temperature. Sasada suggests it happened after the peak, but the minimum temperature could just as well have occurred right before the volcano erupted.

If Kolstad and McGetchin were to do another simulation with somewhat different parameters, say a pluton radius of 10 km and a somewhat higher initial temperature for the formation, they could probably generate a set of curves that agree with both the present borehole temperatures and also with Sasada’s results. In that case the simulated temperatures would probably *decrease* by scores of degrees for the past 500,000 years or so.

Loechelt wouldn’t like my using the Kolstad and McGetchin results to interpret Sasada’s maximum

temperature as occurring after his minimum temperature. That’s because it would mean the formation was hotter than today for hundreds of thousands of years, gradually cooling down to today’s temperatures. That’s not what Loechelt wants. Even if the formation temperatures had only been roughly constant at today’s values for a half-million years, there would be much less helium left in the zircons ... even using Loechelt’s preferred low diffusivities.

In table 2 are the results of a simple calculation¹⁷ of the retentions Loechelt’s diffusivities would imply. The Loechelt retentions are calculated for the temperature having been constant at today’s levels during the past half-million years. If the temperatures had been higher, as Sasada’s measurement and most of the simulations imply, Loechelt’s retentions would be even lower than observed.

A dissenting temperature history

Loechelt prefers a 1986 paper by T.M. Harrison, P. Morgan, and D.D. Blackwell. It looks at argon from potassium decay in feldspar in a nearby borehole (EE-2, not GT-2). They concluded (from the amount of argon lost) that there had been only thousands of years’ worth of diffusion in the feldspar. Since they believe that the feldspar has existed for over a billion years, they were compelled to assume that the diffusivities were very low up to a few thousand years ago. That is, they assumed that the temperature of the formation increased considerably and rapidly up to today’s values within only thousands of years ago. There are two problems with this scenario:

1. Kolstad and McGetchin’s simulations show that temperatures can’t change all that fast (over a few thousand years) in this particular formation. That’s because the rock is dry, so heat can move only by conduction, which is quite slow in rock.
2. It assumes what Loechelt wants to prove, namely that the earth is old.

On the second point, I have published a technical paper¹⁸ and a popular article¹⁹ that works out the alternative possibility: that the feldspar has not existed for a long time. The Harrison *et al.* data then imply that the feldspar is only about 5,000 years old, in agreement with our helium-in-zircon data.

In summary, Loechelt wants to ignore the likely possibility (in his own long-age worldview) that the nearby volcano would have heated the formation significantly.

Answering the *ad hominem* attacks

Unfortunately, Loechelt makes a number of attacks on my character. He resembles a prosecuting attorney trying to undermine the credibility of a witness for the defence. That suggests to me that Loechelt feels the facts themselves do not support his case as strongly as he would like. Here are the two most serious of his character allegations:

Table 2. Observed and theorized helium retentions in the zircons

Sample	Temperature (°C)	Observed Retentions (%)	Loechelt Retentions (%)
1	105	58	4.5
2	124	42	0.6
3	151	27	0.1
4	197	17	0.002
5	239	1.2	0.00003
6	277	0.1	0.000003

Humphreys is dishonest—Loechelt thinks that (1) we invented Zodiac Mining and Minerals Inc. solely to be our intermediary with our experimenter Farley, and that (2) we instructed Zodiac to lie about our funding the experiments. On the first accusation, Zodiac was a legitimate business that existed before we asked them to approach Farley. On the second accusation, Zodiac didn't lie to Farley. We merely asked Zodiac to not *volunteer* the fact that they were working for creationists. If Farley had asked Zodiac, 'Are creationists paying for this?' we would have acknowledged that. Zodiac made a simple commercial contract with Farley; they gave him money, and he analyzed the helium in the crystals. Though I was under no ethical obligation to do so, I told Farley in 2003 that we were creationists, in an email before we published our first paper with the results.²⁰ Farley wasn't happy about that, but he told me he still stands by his data.

Humphreys is withholding information—Loechelt thinks that Farley made a formal report on the all-important 2003 zircon data, and he thinks that I withheld the alleged report. But, in contrast to his earlier procedure (see his reports in Appendices B and C in the RATE technical book),²¹ Farley didn't make a formal report on the 2003 data. He merely sent us an Excel file with the data, accompanied by a brief email. The unexpurgated 2003 zircon data²² (the 2003 biotite data were not much different than his earlier biotite runs, so we left them out) are in the RATE technical book, and here is his cover email in full (highlighting is mine):

From: Ken Farley [email address]
Sent: Monday, June 30, 2003 11:57 PM
To: Majdah Al-Quhtani [Zodiac employee]
Subject: Re: Sample Instructions

Majdah:

Here are the two data sets in excel format, on zircon and biotite. They look more or less the same as we have already seen. The basic outline of what I did is the same as in the past with the exception [*sic*] that I weighed the samples to get total He concentration, **so I did not bother with another summary report**. Let me know if you need help understanding the data files or interpreting the data.

Ken

Ken Farley
Division of Geological and Planetary Sciences
MS 170-25
California Institute of Technology
Pasadena, CA 91125
[Plus phone and fax numbers,
email address and
list of two attachments, the Excel data files on the
biotite and zircon.]

Loechelt made other attacks on my character, and also on my competence as a scientist. But I don't find them to be particularly relevant to the scientific issues, so I'll let them slide. However, I would like Dr Loechelt to know that I'm not holding any grudges against him for his *ad hominem* tactics.

What the prediction means

In the late 1990s I estimated what diffusivities would give the observed retentions if the earth were 6,000 years old. Although the math is a bit complicated, essentially all it amounts to is dividing the amount of helium lost from the zircons by 6,000 years. That gives a rate of loss, which gives the diffusivity. These are the '6,000 year' diamonds in figure 2. I knew that temperatures in the formation could not naturally change much in only thousands of years, so I connected each diffusivity value with the temperature of that sample in the borehole today.

I had no idea whether the yet-to-be measured diffusivities would agree with the prediction, except that scaling from other measurements seemed to give results within an order of magnitude or so. We published this prediction in 2000 in a hardcover book (reference 6). If the data turned out to be different than the prediction, I had no way to change the prediction. I was out on a limb.

Three years later, Farley did his experiments on our zircons. He didn't know what results we wanted, and we didn't tell him. I remember well when I first plotted out Farley's data (the dots in figure 2) on a graph which already had the prediction on it. I was astounded at how well the data lined up with the prediction, because real-world diffusivities can differ from each other by factors of thousands to billions. A lineup would be exceedingly unlikely if there was something seriously wrong with either the prediction or the experiment.

The fulfilled prediction shows that the most straightforward understanding of the diffusion processes in the formation is the right one. If something weird was going on, as Loechelt alleges, it is very unlikely that the agreement we got would have occurred. This should build confidence, even in a non-expert, that our results are valid.

Summary

In my opinion, most of what Loechelt said in his two-hour presentation was neither new nor particularly cogent. I think we can boil his criticisms down to two essentials:

1. He thinks he can ignore Farley's low-temperature data, and
2. He thinks he can ignore simulated and measured past temperatures in the formation.

If either (1) or (2) are incorrect, then his case collapses. I have given evidence above that both assumptions are incorrect, so his case would then be doubly dead. On the other hand, the most straightforward interpretation (that is, without imagining strange and exotic processes) of the data is that the zircons are roughly 6,000 years old.

Epilogue: why this is important?

This helium argument is a sample of over 100 pieces of evidence^{23,24} that the world is young, not billions of years old as most of us (including me) have assumed. That's important, because the pervasive opinion that the world is old is one of the most serious impediments to taking the Bible at face value, as historical and scientific truth. That was the case for me many years ago as a young, atheistic, and rather arrogant, grad student in physics. As I began to read the Bible, I began to learn some bad news and some good news. Part of the bad news was that the human race is suffering from a fatal disease of the soul, which the Bible calls sin.²⁵

Most people, including me, do not naturally think of this disease as being something very serious. We tend to minimize the symptoms of our condition with thoughts like:

"I only lie occasionally, and for a good purpose ... the company condones a little pilfering of low-priced things ... sometimes I have lustful thoughts about my neighbour's wife, but that's natural ... and yes, I hate some people, but they really deserve it."

Most of us, including me, think we're fairly nice people. If there really is a heaven, God wouldn't keep us out of it, especially not send us to that other place, would He?

The really bad news, I found, is that God hates all sin intensely and must judge it.²⁶ If we don't accept His remedy²⁷ for our disease, and instead cling to our sin, there is no place in His new cosmos²⁸ for us but isolation in what Christ called 'eternal fire'.²⁹

The really good news is that God has provided a remedy for our disease. He loved you and me so much that He sent his only-begotten Son into the world to die on the cross, taking upon Himself as our substitute the penalty, death, which we deserved for our sins ... so that whoever believes on (trusts in, commits to, relies upon) Jesus Christ might not perish, but have everlasting life. Most of that comes from John 3:16, that verse which most people know but few ever think about. The substituting death of the Messiah is in Isaiah 53:6,³⁰ written seven centuries before the time of Christ. I hope you will find, as I have, the abundant life that Christ promises.³¹

The helium results are also important to the minority of you here who are genuine Christians, but who believe the world is billions of years old. I think Dr Loechelt and Dr Ross are in that class. I understand your point of view

from personal experience. After I had accepted Christ as my Saviour, I was an evolutionist for about one year. I was doing what comes quite naturally, trying to fit what I had been taught all my life (billions of years of physical and biological evolution) into the Bible that I was beginning to learn. I thought God caused the evolution, but behind the scenes.

However, the fit wasn't too good, and I wasn't happy with it. After about a year, two things happened. The first was that someone suggested to me that the Flood in Genesis, chapters 6–9 was a worldwide event, and that it would have buried all the fossil-bearing strata during one year. The second was reading a book by Henry Morris, *Biblical Cosmology and Modern Science*. It listed some of the evidence for a young earth that I mentioned in the first sentence of this epilogue. That evidence was what made me a young-earth creationist.

My dear Christian brothers and sisters who believe the earth is old, I get the impression that you *want* to resist the same evidence that convinced me. Is that because you think that the consensus of scientists can't be wrong? When I worked at Sandia National Laboratories, I discovered something surprising about that consensus: it's not based on a study of the evidence. I found that that *most scientists believe the world is old simply because they believe that most other scientists believe the world is old!* The idea was just 'in the air' ever since their youth, as it was for me, and most of them believed it because it had never occurred to them to question it.



Figure 9. Rig which drilled borehole GT-2, at Fenton Hill on the western rim of the Valles volcanic caldera, about 30 km west of Los Alamos, New Mexico, US

Or do you believe that non-Christian scientists are too objective to be influenced by their deep desire to imagine an old world without a Creator? Surely you have found out that all of us, scientists or non-scientists, are powerfully moved by our biases, especially when the religious stakes are high. Last, perhaps some of you are moved by peer pressure; it's just easier to go with the majority and conform to its ideas. I appeal to you, my brothers and sisters, to stop being conformed to this age and instead be transformed by the renewing of your minds.³² Then you will find that all the scientific evidence for a young world^{22,23} is actually quite good news for you.

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 - “All have sinned, and fall short of the glory of God” (Romans 3:23; all quotes here from NAS Bible).
 - “The wages of sin is death ...” (Romans 6:23).
 - “... but the gift of God is eternal life in Christ Jesus” (Romans 6:23).
 - “Then I saw a new heaven and a new earth ...” (Revelation 21:1).
 - “Then He will say to those on His left, ‘Depart from Me, accursed ones, into the eternal fire which has been prepared for the devil and his angels’” (Matthew 25:41).
 - “All of us like sheep have gone astray, each of us has turned to his own way; but the Lord has caused the iniquity of us all to fall on Him” (Isaiah 53:6).
 - “... I have come that they may have life, and have it more abundantly” (John 10:10).
 - “And do not be conformed to this world, but be transformed by the renewing of your mind, so that you may prove what the will of God is, that which is good and acceptable and perfect” (Romans 12:2).

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