Helium Evidence for A Young World Remains Crystal-Clear

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Recently an anti-creationist geochemist, a part-time instructor at the University of Kentucky named Kevin Henke,[1] posted on the Internet a 25,000-word rejection[2] of scientific evidence that the world is only about 6,000 years old, the helium-leak age of zircons (radioactive crystals) from deep underground. In politics, his procedure would be called "mud-slinging," which in this case tries to bury truth under a mountain of minutiae. I normally don't reply to Internet posts from skeptics because I want them to try to publish their criticisms in peer-reviewed scientific journals, the proper place to carry out scientific debates.

However, in this case I want to take the opportunity to share updated information about our research which will appear later this year in the RATE[3] "results" book[4] and in the accompanying book for laymen.[5] I also plan to submit technical details of this reply to a peer-reviewed scientific journal, the *Creation Research Society Quarterly* (*CRSQ*). If Henke chooses to sling yet more mud, let him try to do so in a scientific journal. The RATE helium research has been peer-reviewed and published in several different scientific venues. Critics like Henke must gird up their loins and undergo the same kind of scientific discipline—if they want people to take them seriously. If they refuse to do that, I plan not to reply to them further.

First I'll point out what it is that the skeptics are trying to obscure. Then I will go through Henke's summary of his criticisms point-by-point. Amazingly, in his entire fifty pages he specifies only two real errors of mine: (a) I misspelled a name in one of my references, and (b) I was not precise enough in my geological description of a rock formation. The only other possibly significant items are (1) a quibble about how much helium should have been deposited in the zircons, and (2) a minor mistake I made (which Henke failed to discover) in summarizing our results. Last I'll analyze Henke's tactics and try to plumb his motives.

The evidence Henke wants to hide

I'll try to keep this simple, so for the scientific details, please consult two most relevant publications, which are also archived on the Internet. I'll call them *ICC* 2003 [6] and *CRSQ* 2004.[7] Decades ago, Robert Gentry analyzed tiny zircon (zirconium silicate) crystals recovered from hot Precambrian (over 545 million years old according to the geologic timescale) "basement" rock in New Mexico.[8] Figure 1 shows some of the zircons he analyzed, between 50 and 75 microns (millionths of a meter) long.

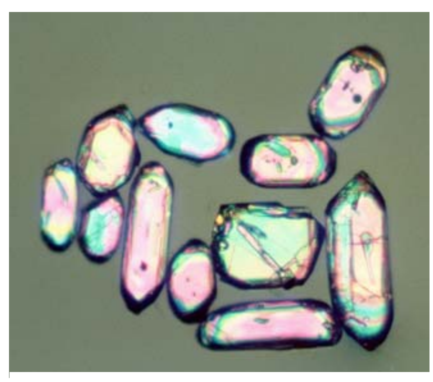

Figure 1. Microscopic zircons. Photo by R. V. Gentry.

Enough of the uranium in the zircons had decayed to lead to give them a radioisotope (radioactive element) age of "1.5 billion" years. But Gentry found that up to 58% of the helium that the nuclear decay would deposit in the zircons was still in them. This was surprising, because helium diffuses (leaks) rapidly out of most minerals.

Not knowing how fast helium leaks from zircon, I estimated what the leak rates would be when we measured them. In essence (of course the math is more complicated), all I did to get the estimates was to divide the amount of helium lost from the crystal by the time (assumed by each model) during which it had been lost. That gives us the leak rates for each of the two models. The "1.5 billion year" model has rates over 100,000 times slower than the "6,000 year" model, because the former has to retain the helium for a much longer time. Then in the year 2000, the RATE group published the estimates as numerical predictions for those two models.[9]

Figure 2 shows the predictions as red and magenta diamond symbols. The bottom axis shows the temperature (in °C) of each sample *in situ*, that is, while it was in the granitic rock unit. (I have reversed the direction of temperature from what is traditional in such "Arrhenius" plots.) The vertical axis shows "diffusivity", which is a measure of how fast helium leaks from a material. The vertical axis is tremendously compressed, representing a factor of one trillion increase in leakage rates from bottom to top. The black numbers under the diamonds are the percentages of helium retained in each sample.

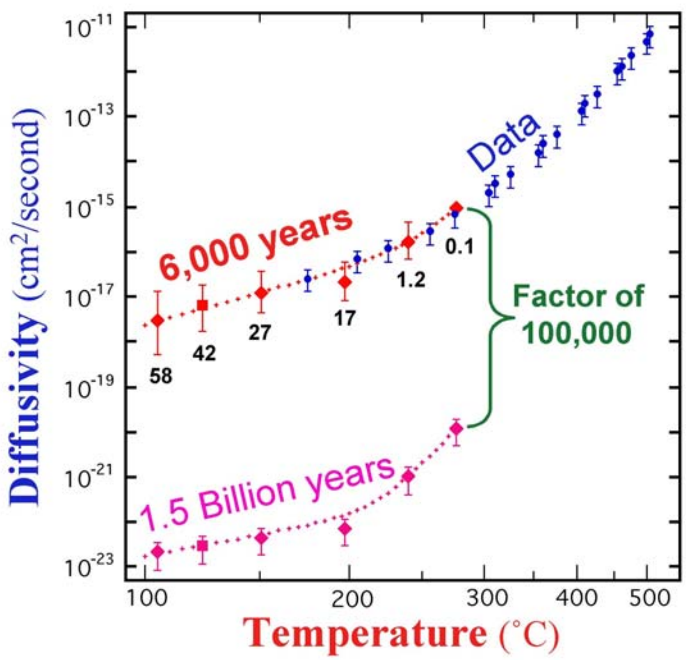

Figure 2. Model-predicted (red and magenta diamonds) and measured (blue dots) helium leak rates of zircons. The data fit the 6,000-year prediction very well.

The red and magenta vertical lines through the diamonds are the "two-sigma error bars". These statistical error bounds were implicit in our reports, but we had not shown them explicitly in our graphs before now. The bars essentially show the 95% confidence limits I estimate for the accuracy of the predictions. The forthcoming RATE "results" book gives details on how I estimated the error bounds.

In 2001 we commissioned one of the world's most respected experimenters in this field to measure the diffusivity of helium in the same-size zircons from the same borehole in the same rock formation. We used an existing mining company as an intermediary, and we asked it to not tell the experimenter about us or our goals. The experimenter, being a uniformitarian (believer in long ages) and not having read our prediction, had no idea what results we were hoping for. It was a truly "blind" experiment, and we (the RATE team) were eagerly awaiting the data.

Figure 2 shows the experimental results as blue dots with blue "2-sigma error bars" going vertically through them. If we repeated the experiments hundreds of times, we estimate the data points would remain within the caps on the error bars over 95% of the time. Again, the RATE "results" book (which has now passed through extensive peer review and is being proofread) will have the details on the error estimates.

To our great delight, the data fell right on the "6,000 year" prediction! This alignment validates the young-age model even for readers who are not experts in this field, because the probability of such a lineup by accident is small. The data resoundingly reject the "1.5 billion year" model. The experimenter, whose name is in one of our articles, stands by his data, even though as a uniformitarian he does not like our interpretation of them. (Even after several years, he has not offered an alternative interpretation.)

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.

Rebutting Henke's Charges

In his abstract, Henke summarized his fifteen principal charges. I'll number them and quote them, indented and in orange font. I'll answer each charge with no more detail than necessary to dispose of it.

1. invoking groundless miracles to explain away U/Pb dates on zircons,

This means he does not find RATE's "accelerated nuclear decay" hypothesis to his taste. But, as the ancient Romans said, "There's no disputing about taste." In other words, Henke's personal preference in theories means exactly nothing to the rest of us. Moreover, it is beside the point. 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 Henke doesn't like our explanation, let him offer his own. I'd be very interested to hear (preferably in a peer-reviewed scientific journal) how he thinks the zircons suffered 1.5 billion years worth of nuclear decay but only 6,000 years worth of helium losses!

2. misidentifying samples as originating from the Jemez Granodiorite,

Henke means that I didn't specify that the top 1000 meters or so of the Precambrian granitic rock unit in question might contain gneiss or schist instead of granodiorite. What he doesn't realize is that "Jemez Granodiorite" is a name I invented (since the literature had not previously named it) to apply to the whole unit from about 700 meters depth down to below 4,310 meters. Our co-author John Baumgardner, a geophysicist, saw large portions of the GT-2 core at Los Alamos and picked our samples from it. He says:

> Yes, there are occasional veins of material other than the coarse-grained granodiorite that forms the vast majority 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.

The important point is that, regardless of the name we put on the rock unit, the zircons throughout it have been measured to contain essentially the same amounts and ratios of lead isotopes,[10] 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.

3. performing helium analyses on impure biotite separations,

That, of course, is a gratuitous slap at the quality of the ICR geological lab, which did that particular separation. In the lab's defense, I would point out that their separation of biotite from another rock unit, the Beartooth Gneiss, was excellent. I'm judging that by the helium data from that unit in Appendix B of *ICC* 2003, which our experimenter called "remarkably linear". Henke's allegation is also unproven. Different localities, having different minerals, offer different degrees of difficulty with separation. The only way to gauge quality in this case would be to have another lab work on the same rocks and try to get a yet higher purity. I challenge Henke to procure his own samples of the same core from Los Alamos and to try to do a better separation himself!

However, haggling about the exact diffusivity of biotite is irrelevant, because as we pointed out in numerous parts of our articles, it is clear that that zircon has a diffusivity an order of magnitude lower than that of biotite in the low-temperature range of interest to us. That makes the diffusivity of zircon much more important to know accurately. Henke's attack here is a good example of what I meant by "mud-slinging"—nasty, irrelevant, and intended to distract the readers from the important issues.

4. dubiously revising helium measurements from Gentry *et al.* (1982a),

On p. 16 of *CRSQ* 2004, in my notes in the reference "Gentry *et al*. 1982a", I spelled out exactly why and how I, in consultation with Gentry, made two corrections in his tables (the main one being in the units he specified for his absolute amounts of helium). There is nothing dubious about it. Moreover, as I implied in that note, the corrections would not affect the main result of the paper, which depends on the *percentage* of helium retained, not the absolute amounts. Finally, as I pointed out on p. 9 of the same article, "the 6.3 ncc/μg yield of these zircons [our sample 2003] is quite consistent with Gentry's data [as revised]". Figure 7 on the same page shows how well the resulting 42% retention point interpolates between Gentry's points 1 and 2. Without the revision, no interpolation at all would have been possible. That is very strong evidence that the correction was justified.

5. relying on questionable Q/Q_0 (helium retention) values from Gentry *et al.* (1982a),

We checked Gentry's values for retention with our own data on the zircons, as I wrote in *CRSQ* 2004. 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. Henke's problem is with the value of Q_0, as I will explain below.

6. failing to recognize that the Q_0 values (maximum possible amount of radiogenic helium in a mineral) for their samples were probably much larger than 15 ncc STP/µg,

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. From our own measurements of lead in zircons and my own very rough estimate of alpha particle losses, I got a Q_0 considerably less than 25 ncc/µg. Gentry's original calculations are no longer available. But after discussing the matter with him, 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. In support of that is the remarkable alignment of the diffusion measurements with the predictions in Figure 2. The paper I plan to submit to *CRSQ* will discuss this issue more fully.

However, even if Henke's number were correct, it would reduce the percentage retentions by only a factor of two or so. That is not anywhere near the factor of about *100,000* reduction that Henke needs. Put another way, Henke's values for retentions would not move the predictions outside the error bars Figure 2 shows. This is a molehill, not a mountain.

7. inconsistently interpreting already questionable helium concentrations from samples 5 and 6 to make them comply with the demands of their "models",

I have already discussed this matter fully in sections 2 and 6 of *ICC* 2003. Sample 5 is the right-hand diamond of the predictions in Figure 2, the one at nearly 300°C with 0.1% retention. The fact that it fits the data so closely (one data point fell almost right on it) supports our interpretation. The total amount of helium in sample 6 supports our interpretation of that sample also. However, we could dispense with both samples entirely with no damage to our case at all. This is just another quibble about an inconsequential issue.

8. seriously underestimating the helium concentrations in the zircons from 750 meters depth and not realizing that their Q/Q_0 value for this sample (using Q_0 = 15 ncc STP/µg) would be greater than one and therefore spurious,

This is an interesting issue, if you like to delve into details. It turns out that the problem is not with the data itself, but rather with my summary of it, and the fact that Henke believed my summary uncritically! This all has to do with Appendix C in *ICC* 2003, where our experimenter reported that, "This sample has a very high helium yield, 540 nmol/gram", and where he reported the amounts of helium liberated per step in the "Helium 4" column of Table C1. He did not report the units for that column, so I assumed they were also "nmol/g" and added those units to the label of the column. I also assumed that the numbers in that column added up to 540, so at the end of section 9 of *ICC* 2003, I reported that the experimenter was reporting "a partial (not exhaustive) yield of 540 nanomoles of helium per gram of zircon."

However, it turns out that the units of the helium column should be "ncc". When we divide the sum of the numbers in that column (1794 ncc) by the mass of the sample (350 micrograms), we get 5.126 ncc/µg. Multiply that by a conversion factor (0.4462×10^{-4} nmol/ncc) and convert micrograms to grams to get 228.7 nmol/g. Dividing that by 540 nmol/g gives us a ratio of 0.4235, which agrees exactly with the bottom entry of the "Cumulative fraction" column. This means that 540 nmol/g is the *total* yield after melting the crystals, not a partial yield.

Converting 540 nmol/g to 12.1 ncc/µg and dividing by Q_0 = 15.0 ncc/µg gives us a retention for the 750 meter sample of 80.7 %. I reported that as "~80" in Table I of *CRSQ* 2004. (I used the "~" sign because as I reported in *CRSQ* 2004, p. 5, the average size of the zircons in the 750 meter sample is unknown, making detailed comparisons with the other samples inappropriate.)[11] By that time our own sample 2003 (the one with 42% retention) had made me conclude that the 540 nmol/g in sample 2002 was a total yield, but I did not think of going back to Table C1 in *ICC* 2003 to check on things there.

The bottom line is that the retention fraction for the 750 meter sample is less than one, not "greater than one", as Henke thought. I don't blame him for being misled by my mistake, but perhaps he will want to blame himself. The critic wasn't critical enough!

9. not properly considering the possible presence of extraneous ("excess") ^{3}He and ^{4}He in their zircons,

Henke's reason for raising this issue was his reasoning about the previous item. Because he thought that the retention fraction in sample 2002 was greater than 100%, he figured there had to be "excess" helium coming into the zircon from outside it. As the above item shows, his premise was wrong.

But let's look at his scenario more closely. 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.

In Henke's vague scenario, the source of the helium is "recent" (100,000 to 1.45 million years ago) volcanic magmas several kilometers away from our borehole. He is apparently assuming that conduits of such magma came relatively close to borehole GT-2. The conduits could not have broken through to the surface, because then they would have immediately vented their helium into the atmosphere. Henke wants "fluids" from the magma to carry helium through the mineral interfaces in the granodiorite, through the biotite, and into the zircons.

It is doubtful that such fluids could travel very far. First, the granodiorite is presently dry and well-consolidated, even at the surface. Second, the overlying rock puts the Jemez Granodiorite under *in situ* pressures hundreds to thousands of times greater than atmospheric pressure. Those factors would mean that the interface widths between minerals would be microscopic, perhaps only an Angstrom (the diameter of a hydrogen atom) or so. Henke needs to show—preferably with experimental data in a peer-reviewed scientific journal—just how far the helium could travel in this rock unit during the time he thinks is available. That would determine how close his conduits of magma would have to be. Then he would have to show geological evidence that conduits of basalt (solidified volcanic magma) presently exist within that distance of the borehole.

Next, Henke would have to show that the concentration (atoms or nanomoles per cc) of helium in the magmatic fluids could have been high enough to do the job. Our 15 ncc/μg value for Q_0 in the zircons means there was at least 3140 nanomoles of helium per cubic centimeter in the zircons originally. (Henke's value of "41" ncc/μg in item 6 above would require even more helium, 8590 nmol/cc.) The concentration in the assumed fluids would have to exceed that value in order to transfer helium from the fluid into the zircons. Yet the concentration of helium produced by uranium decay in typical basalt[12] (and hence in basaltic magmatic fluids) would be less than 80 nmol/cc, more than forty times too small. No transfer would take place. So Henke's scenario requires extraordinary amounts of helium in his magmatic fluids.

But let's assume for the sake of argument that the helium somehow gets into the zircons. Now it has to stay there. The magmatic fluids would raise the temperature of the zircons considerably higher than their present temperature, and temperatures would remain high for dozens of millennia. As I showed in *ICC* 2003, section 7, the zircons would then lose essentially all their helium—contrary to what we observe. Moreover, most of the helium outside the zircons has to disappear somehow, so that the biotite concentration would drop to its present low level, hundreds of times less than the concentrations in the zircons.

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.

All the data point to a much more straightforward scenario: the source of the helium is the *observed* nuclear decay in the zircon, the helium is diffusing as *observed* out of the zircon into the biotite, and according to the *observed* total quantities not much of it has gone beyond the biotite into the surrounding minerals.

10. listing the average date and standard deviation of their 2004 results as 6,000 ±2000 years, when [citing a two-] standard deviation (two-sigma) [error] of ±4000 years [would be] more appropriate.

(Brackets and black font show my clarification of Henke's confused grammar.) This is entirely a matter of personal preference. I made clear that my date was plus or minus one standard deviation (one-sigma), so it is easy enough for people like Henke to multiply that number by two to get a two standard deviation (two-sigma) error more to their liking. However, this is again just a ridiculous quibble. One or two standard deviations pale into insignificance compared to the difference between the helium leak age and his preferred age of 1.5 billion years—a whopping *750,000* standard deviations!

11. "fudging" old Soviet data that should have been ignored,

So Henke believes inconvenient data should be "ignored", does he? That offers insight into his attitude toward truth. Only people who blindly follow consensus thinking and modish fashions in science would dismiss data simply because it is "old". The same kind of people try to find excuses to ignore data that go against the consensus opinion. That is exactly what Henke is trying to do with the helium data.

Henke's word "fudging" is a lie about what we did, as anyone who wants to read section 5 of *ICC* 2003 can find out. As Figures 5 and 6(a) of that paper show, interpreting the ambiguous label of the Soviet graph in a reasonable way makes its high-temperature zircon data line up with everybody else's zircon data.

But again, this is just a ridiculous quibble, because our conclusions depend in no way on the Soviet data. The purpose of section 5 was simply to explain why I didn't understand those data until after we had made our own measurements.

12. deriving "models" that are based on several invalid assumptions (including constant temperature conditions over time, Q_0 of 15 ncc STP/µg, and isotropic diffusion in biotite),

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." That is because their thermal history models require a recent (by their timescale) pulse of high temperature which would wipe out all the helium in the zircons. I further pointed out that the zircons would have to be *colder than dry ice* [*CRSQ* 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. As I said in item 6, Henke's hoped-for value of Q_0 would make no practical difference in our results. And I discussed the assumption of isotropic diffusion in biotite, showing that a more precise assumption would make no practical difference in our results. Biotite has hardly any effect on the outflow of helium from zircon, as we demonstrated. Again, this is a molehill, not a mountain. 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?

13. failing to provide standard deviations for biotite measurements (*b* values) and then misapplying the values to samples from different lithologies,

Again majoring on minors. As we pointed out in the papers, the diffusion rates for biotite and other micas were so much higher than the rates for zircon that it was clear the biotite affects our results to only a small degree. However, Henke has the raw data we published, so he can compute the standard deviations for himself.

14. inserting imaginary defect lines into Arrhenius plots,

The curve fits, which have no imagination, show a numerical change of slope in the zircon data between 200 and 300°C. It doesn't take much imagination to see such a bend in Figure 2. The change of slope implies a change in the dominant physical mechanism of diffusion at that temperature. However, 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. Yet again, this is a ridiculous quibble.

15. deriving and using equations that yield inconsistent "dates."

Equations are only as good as the numbers one plugs into them. Henke plugs garbage into the equations and gets garbage out. Figure 2 shows obvious-to-the-eye evidence for the dates I got. Notice how well the data fit the "6,000 year" prediction. Notice how far away the data are from the "1.5 billion year" prediction. All of Henke's slung mud cannot obscure the obvious conclusion: the helium leak age is very much closer to 6,000 years than it is to 1.5 billion years.

That is the last of Henke's summary. He makes other allegations throughout the paper, but evidently he did not think them good enough to put into his summary, so I'll similarly disdain them.

Henke's Tactics and Motives

The first thing to notice about Henke's issues is how *few* of them there really are. For example, of the fifteen items above, six of them (4, 5, 6, 8, 9, 12) boil down to only one issue, how much helium was deposited in the zircons. Several other items repeated themselves similarly.

The second thing to notice is how *peripheral* they are. Not one of them has any chance of solving Henke's real problem: how to keep helium in leaky minerals for over a billion years.

Third, notice how *petty* most of them are. One of my challenges in answering those charges was to find different words describing their basic character: "molehill, not a mountain ... distinction without a difference ... haggling ... ridiculous quibble ... inconsequential ... majoring on minors ... irrelevant". Eight of the items (1, 2, 3, 6, 7, 10, 11, 12) fall into that class.

But despite his scarcity of significant issues, Henke chose to puff them up to enormous proportions with a torrent of hot air—fifty single-spaced pages using up my printer supplies. Why? Well, of course he is trying to bluff his readers. Unless the reader is technically well-informed in this specialty and wants to take the time to examine Henke's monograph carefully, he is apt to think that where there is so much verbal smoke there must be some factual fire.

However, I suggest there is a more basic reason for the inflation: Henke may be trying to reassure himself that he was correct in rejecting the Bible many years ago. This brings us into the area of motives, which require a lot of guesswork. But it is worthwhile to do so because people like Henke seem to be the worst enemies of creationism, and creationists need to understand that. In an Internet review[13] of a book Henke contributed to, he asserts that he was once a sincere convert to Christianity but then "deconverted" himself:

> I committed my life to Christ and I encouraged others to do so. However, ***after I read the Bible***, and especially the false prophecies in Revelation and the countless contradictions in the Gospels, I realized that the claims of Christianity were false.

(Emphasis mine). The order of events here is interesting. First Henke commits his life to something or someone he considers Christ. *Then* he reads the Bible. That order is contrary to the order in 1 Peter 1:23, where the word of God causes the new birth:

> For you have been born again, not of seed which is perishable but imperishable, through the living and abiding word of God.

It is possible that Henke had some exposure to the word of God at the outset, enough that, like the rocky soil in the parable of the Sower, he and others like him "believe for a while and in time of temptation fall away" (Luke 8:12). The previous verse (Luke 8:11) connects believing with being saved. If eternal life, after it begins with salvation, is truly eternal (some Christians might disagree with that), then someday Henke might be extremely shocked to find himself in heaven, though without rewards.

However, his hostility to Scripture when he encountered it is uncharacteristic of someone who has genuinely experienced the new birth. For example, after I was saved through reading the gospel of Mark and then accepting Christ as my Savior, my subsequent reaction to the rest of Scripture was the same as that of the prophet Jeremiah (15:16):

> Thy words were found, and I did eat them; and thy word was unto me the joy and rejoicing of mine heart: for I am called by thy name, O LORD God of hosts.

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.

Whether he was genuinely born again or not, his present symptoms might look the same to outside observers (and even to himself)—a severe allergic reaction to the Bible and to anyone saying it is straightforward and accurate.

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.

Henke also doesn't seem to see that the passage is remarkably accurate about the biggest intellectual blunder (uniformitarianism) of our age, a mistake characteristic of only the last two centuries since the time of Christ. That accuracy alone (which he inadvertently supports by his vehemence) would support its validity. Last, Henke would not like to hear that I have based a theory on the creation of planetary magnetic fields[14] on part of the passage (2 Peter 3:5) he disparages, and that NASA spacecraft have confirmed the scientific predictions of that theory.[15]

Because of his flight from Scripture, Henke has to keep reassuring himself that it can't possibly be true. That is why he has so much spleen to vent when he encounters someone saying, "Here's scientific evidence that the Biblical 6,000-year timescale is correct!" Henke cannot abide it; he must expunge it from his mind. His battle is not so much with creationists as with Christ himself. I'm glad that the Spirit of God may be using some of this crystal-clear zircon evidence to convict one who has fallen away from the truth.

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[1] Kevin Henke, Part-Time Instructor in geological sciences, http://www.uky.edu/ArtsSciences/Geology/faculty/henke.html

[2] Henke, K. R., Young-earth creationist helium diffusion "dates", posted March 17, 2005 at http://www.talkorigins.org/faqs/helium/zircons.html See March 17, 2005 copy archived here.

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[3] An acronym for "Radioisotopes and the Age of the Earth", an eight-year research initiative sponsored by several creationist organizations. See http://www.icr.org/newsletters/research/researchoct01.html

[4] Vardiman, L., A. A. Snelling, and E. F. Chaffin, editors., *Radioisotopes and the Age of the Earth: Results of a Young-Earth Creationist Initiative*, Institute for Creation Research, El Cajon, California, and the Creation Research Society, St. Joseph, Missouri, expected publication date, on or before November 2005.

[5] DeYoung, Don, *Thousands not Billions*, Master Books, Green Forest, Arkansas, expected publication date, on or before November 2005.

[6] (*ICC* 2003) Humphreys, D. R., S. A. Austin, J. R. Baumgardner, and A. A. Snelling, Helium diffusion rates support accelerated nuclear decay, 2003a, in *Proceedings of the Fifth International Conference on Creationism*, edited by R. L. Ivey, Jr., Creation Science Fellowship, Pittsburgh, Pennsylvania, pp. 175-195, 2003. See http://www.icr.org/research/icc03/pdf/Helium_ICC_7-22-03.pdf .

[7] (*CRSQ* 2004) Humphreys, D. R., S. A. Austin, J. R. Baumgardner, and A. A. Snelling, Helium diffusion age of 6,000 years supports accelerated nuclear decay, *Creation Research Society Quarterly*, *41*(1), 1-16, 2004. See http://www.creationresearch.org/crsq/articles/41/41_1/Helium_lo_res.pdf .

[8] Gentry, R. V., Glish, G. J., and McBay, E. H., Differential helium retention in zircons: implications for nuclear waste management, *Geophysical Research Letters*, *9*(10), 1129-1130, 1982a.

[9] Humphreys, D.R., Accelerated nuclear decay: a viable hypothesis?, in *Radioisotopes and the Age of the Earth: A Young-Earth Creationist Research Initiative*, edited by L. Vardiman, A. A. Snelling, and E. F. Chaffin, Chapter 7, pp. 333-379, Institute for Creation Research and the Creation Research Society, San Diego, CA, 2000.

[10] Gentry, R. V., T. J. Sworski, H. S. McKown, D. H. Smith, R. E. Eby, and W. H. Christie, Differential lead retention in zircons: implications for nuclear waste containment, *Science*, *216*, 296-298, 1982.

[11] For example, if the average length of zircons in that sample (number 2002) were larger than the average length in the other samples (about 60 microns), then the percentage of alpha particles retained would be higher. That would make Q_0 higher than the value of 15 ncc/µg we used for the other samples, thus dropping the retention from 80.7 % to a smaller value. This affects Henke's reasoning in item 8.

[12] Stacey, F. D., *Physics of the Earth*, John Wiley and Sons, New York, p. 245, Table 9.3, 1969. The table says the average amount of uranium in basaltic crust is 0.8 ppm by weight. Assuming that at most an equal amount of uranium has already decayed to lead (the thorium, having a much greater half-life, would not have decayed nearly as much), and that all the helium produced thereby has remained in the basaltic magma, gives an average helium concentration of less than 80 nmol/g in such magmas.

[13] Henke, K., Testimony to the failure of fundamentalism, posted December 31, 2001, http://www.amazon.com/gp/cdp/member-reviews/AKAJJROZZM9M4/103-0783137-0663064?_encoding=UTF8 .

[14] Humphreys, D. R., The creation of planetary magnetic fields, *Creation Research Society Quarterly* *21*(3):140-149, December 1984. Archived at http://www.creationresearch.org/crsq/articles/21/21_3/21_3.html .

[15] Humphreys, D. R., Beyond Neptune: Voyager II supports creation, *ICR Impact*, No. 203, May 1990, archived at http://www.creationresearch.org/crsq/articles/21/21_3/21_3.html .