# Critics of helium evidence for a young world now seem silent?

Recently, Russell Humphreys, a leading scientist of the RATE research project, published a perspectives article in this journal summarizing criticism of his model of helium diffusion in zircon crystals[1], which was used in the RATE project to support its accelerated nuclear decay hypothesis.[2–4] In his conclusions, Humphreys erroneously claimed that he had silenced his critics, including me. In doing so, Humphreys failed to acknowledge my response[5,6] to his original criticism[7] of my work.

As I pointed out in my reply, his dismissal of my 37 page technical paper[8] with only three paragraphs of unsubstantiated rhetoric hardly qualifies as serious scholarship. I went on to answer each of Humphreys' objections, point-by-point, in the remaining nine pages of my paper using original material that had never before been published.[5] Contrary to Humphreys' expectations,[1] my response was not an article with "*minor tweaks and new dates*" used to make it "*appear to be more recent*". Rather, it is Humphreys who has resorted to these tactics by publishing a perspective article[1] in this journal that is merely a minor tweak of his non-peer-reviewed web article.[7] After waiting almost a year for Humphreys' response, I was hoping for a reply with a little more substance.

A major point of contention is the thermal history of the Fenton Hill geothermal site (New Mexico, USA). A key reference cited by both Humphreys and myself is the thermal modeling work of Harrison, Morgan and Blackwell.[9] Figure 1 is a reproduction of figure 9 from their paper. It depicts the modeled thermal response at three different depths to a transient heating event 24,000 years ago. For the sake of discussion, consider only the thermal history at a depth of 2,900 m (solid line).
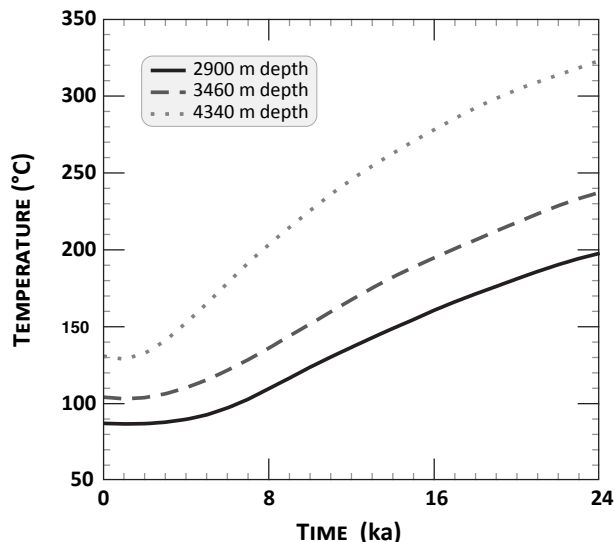
Figure 1. Thermal histories of Fenton Hill, New Mexico predicted by a transient heating event for three different depths (figure 9 from Harrison *et al.*, ref. 9).

My interpretation of the figure is that zero on the time axis represents the beginning of the heating event 24,000 years ago, since the temperature of 87ºC is consistent with the initial condition of a background geothermal gradient of 30ºC/km at a depth of 2,900 m. Furthermore, 24 ka on the time axis represents the present, since the temperature of 197ºC corresponds to the currently measured temperature at a depth of 2900 m. At no point in time does the temperature exceed the current temperature of 197ºC.

In contrast, Humphreys claims[4] that

"Later studies [Harrison *et al.*, 1986; Sasada, 1989] add a more recent pulse of heat and have past temperatures being higher, 110 to 190ºC more than today's levels just 24,000 years ago, and higher before that [Harrison *et al.*, 1986, p. 1906, figure 9]."

Here is my question: *In figure 1, where does the temperature at 2,900 m (solid line) exceed the current temperature of 197ºC by more than 110 to 190ºC?* Although Humphreys boasts in the thorough peer review of his work, neither he nor one of his seventeen reviewers and editors has answered this, and many of my other serious questions, after four years of

both private and public dialogue. I am awaiting his reply.

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2009, www.asa3.org/ASA/education/origins/ helium-gl4.pdf, accessed 15 April 2010. I had previously informed Humphreys of this reply through CMI in May 2009.

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Russell Humphreys replies:

In a letter to this journal,[1] Gary Loechelt tries to rescue the uniformitarian world-view ("all continues just as it was from the beginning", 2 Peter 3:4) from the peril into which the Radioisotopes and the Age of the Earth (RATE)[2] helium evidence for a young world puts it. There are three things to note about Mr Loechelt's letter:

(1) It shows that even five years after the RATE research initiative ended, old-earthers are still worried about it, including the helium project[3] that I led.

(2) It is the only one of over a dozen Internet critiques[4] of the helium project that has been published in a scientific journal (this one).

(3) It points out an error of mine, for which I'm grateful. However, as this reply will show, the helium data continue to support the short biblical timescale just as strongly as before.

Mr Loechelt wants to explain how the tiny zircons (radioactive crystals of zirconium silicate) we

Figure 1. Valle Grande, New Mexico, USA, is inside Valles Caldera, a 16-km-wide volcanic crater. The borehole from which RATE's zircons came is just outside the caldera's rim.

Photo courtesy of US National Park Service.

studied could have retained most of their nuclear-decay-generated helium for 1.5 Ga deep underground in hot, dry rock near a large volcanic crater in northern New Mexico, USA (figure 1). Helium leaks (or diffuses) rapidly through and out of hot minerals, but more slowly out of cold minerals.

For both our creation model and a uniformitarian model of helium leakage, I assumed the temperatures in the rock were always the same as they are today. For the creation model, that is a good assumption, because heat diffuses so slowly through dry rock that temperatures would not change significantly during the nearly four and a half millennia since the Genesis Flood ended.

In the helium chapter of the RATE "results" book,[3] I wrote that assuming constant temperatures is generous to old-earthers, because it appears that uniformitarian models of heat in the area have past temperatures underground being higher than today. Loechelt disagrees. He wants much lower temperatures in the past. But I will show in my last section below that even if we grant him *lower* temperatures than he requests, zircons as old as he wants would still not retain nearly as much helium as we observe.

In the RATE results book, I cited three papers in geoscience journals to show that according to uniformitarian views, past temperatures were higher. I'll discuss each one below, showing that Loechelt unfortunately misses their overall message.

1978 heat models

The earliest of my references was a 1978 paper by C. D. Kolstad and T. R. McGetchin,[5] both with Los Alamos National Laboratory. It modeled the thermal history of Valles Caldera, a volcanic crater nearly 16 km in diameter near Los Alamos, and the surrounding hot dry granitic basement rock. The study included Fenton Hill, not far outside the western rim of the caldera, the site of borehole GT-2 that provided our zircon samples (figure 2). The authors mathematically modeled the heating that would be produced by the large body of magma (molten rock) that, rising from deep below, caused the large eruption and caldera collapse. The usual nuclear dating methods, combined with uniformitarian assumptions, led them to believe the eruption occurred 1.04 Ma ago. Creationist geoscientists think it happened shortly before or after the end of the Genesis Flood, about 4,350 years ago, according to the chronology of the Hebrew text of the Bible.
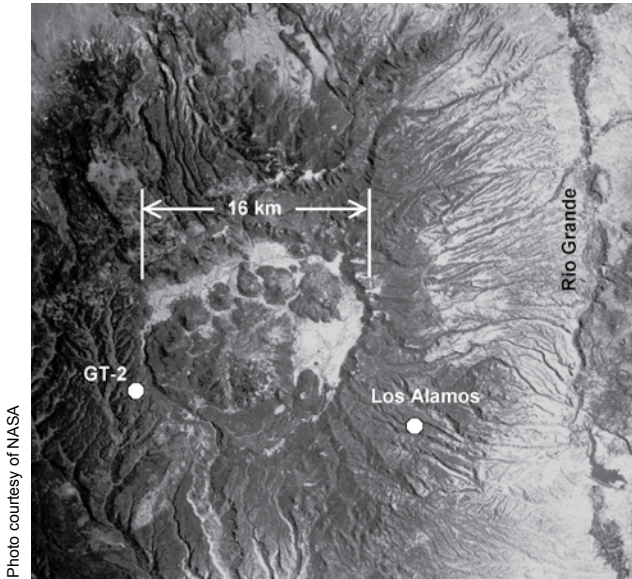
Figure 2. Valles Caldera photographed from space shuttle. Annotated by Humphreys. Vegetation is dark colored.

Kolstad and McGetchin did not know the lateral extent of the magma when it stopped rising, so they made two models. One had the magma chamber (or pluton) being 8 km in radius, with the edge being under the rim of the caldera, 2.8 km to the east of borehole GT-2. The other model had the chamber being 12 km in radius, extending out beyond and beneath GT-2. After a million years, most of the magma would have cooled and crystallized, according to their model in their figure 7.

My figure 3 here reproduces their figure 12, showing the temperatures at various depths in borehole GT-2 for the million-year period. I've changed the labeling of the time axis to show years instead of square-root years. The axis shows time after the eruption, so that zero is the time of the eruption and 1.00 Ma is the present.

The 12-km model, the solid lines in the figure, has the temperature at 3 km depth being over 800°C a million years ago, and then decreasing gradually down to the 200°C observed today. So this model claims much higher past temperatures than today.

After this study, deeper boreholes (down to 4.35 km) in the same vicinity did not show evidence of the magma chamber having been directly beneath GT-2, as Kolstad and McGetchin had supposed in their 12-km-radius model. Their figure 11 showed in detail their other model, the one with the chamber having only an 8 km radius. They re-drew one of the lines, for 3 km depth, as a dashed line in their figure 12, my figure 3 here. Notice that it would give a present temperature of 150°C, fifty degrees lower than the observed temperature at a 3 km depth. So their 8-km-radius model suggests that the magma chamber would heat the borehole, but that another source of heat would be needed to raise the temperatures farther up to today's values.

1986 heat model

In 1986, T. Mark Harrison, Paul Morgan, and David D. Blackwell, geoscientists at three US universities, published a new model.[6] This was my second reference. In their paper they proposed that a small source nearby, say an intrusion of magma, heated the Fenton Hill boreholes relatively recently in a "transient" way. They constrained their model, which has seven adjustable parameters, with their interpretations of the diffusion and retention of (potassium-40-produced) argon 40 in feldspar samples from the boreholes. Such retentions are supposed to give the temperature history. Unfortunately, the interpretations would be affected drastically by the acceleration of nuclear decay and of cooling that RATE was hypothesizing. That complication made their paper less useful to me, so I did not read it carefully. Thinking of Kolstad and McGetchin's models, which show a *decrease* of temperature with time, I assumed that figure 9 in the paper (Loechelt's figure 1) was showing "years before present" and therefore was also showing a decrease of temperature. But, as Mr Loechelt implies, I was reading the graph backwards. Instead, the graph asserts that about 20,000 years ago, the temperature at 2.9 km depth started rising from an 87°C baseline up to the temperature today, 197°C.

The "baseline" of 87°C will prove to be important below. Harrison *et al*. got it from the temperatures in the Sun borehole, which was about 10 km south of GT-2 and farther from the caldera rim, about 6 km. They appeared to ignore the 8-km-radius model
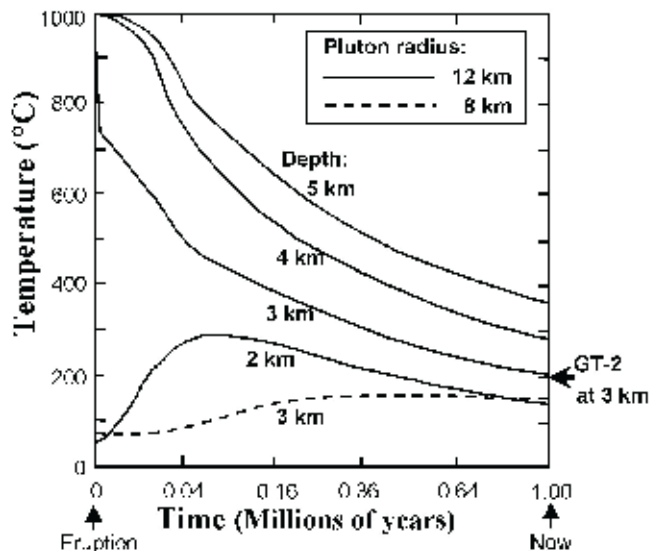
Figure 3. Heat models from Figure 12 of Kolstad and McGetchin (1978), showing decreasing temperatures in borehole GT-2 at various depths. Timescale gives alleged millions of years after eruption.
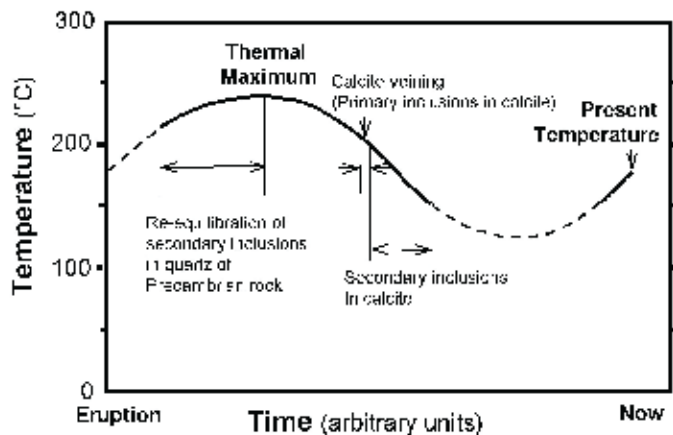
Figure 4. Sasada's figure 9, schematically showing measurements of past temperatures in borehole GT-2 at 2.6 km depth.

of Kolstad and McGetchin, which suggests that the main eruption would raise the "baseline" temperature to at least 150°C. With the latter temperature to start with, the transient source of heat of Harrison *et al.* would need to provide only an additional 50°C to bring the borehole up to today's temperatures. Instead, the authors (and Loechelt) appear to believe that the temperature at 3 km depth was around 87°C for almost the entire age of the basement rock, an alleged 1.5 Ga. If there had been no vulcanism nearby, their assumption would be consistent with the average geothermal gradient (the increase of temperature with depth) in continental crust, about 20°C/km.[7]

1989 paleo-temperature measurements

A few years later, Masakatsu Sasada, of the Geological Survey of Japan, published *data*, not models, for past temperatures in borehole GT-2.[8] That was my third reference. Sasada studied fluid inclusions in calcite veins and quartz in the rocks from that borehole, at depths of 1.876 km and 2.624 km. The study gives the temperatures experienced by the rocks when the various types of inclusion formed. But it gives only the sequence of the thermal events, not the actual times they occurred. My figure 4 here shows Sasada's figure 9, which schematically shows his results for

the 2.6 km depth. He associates the thermal maximum, at about 230°C, with "the heating from the magma reservoir which erupted the Bandelier tuff [rock formed by ash from the main eruption]." He cites a reference putting the latest such eruption at 1.12 Ma ago, a little earlier than Kolstad and McGetchin's age of 1.04 Ma. Then the temperature declined to an undetermined minimum, at the bottom of the right-hand dashed curve in the figure. Finally it rose relatively recently (say, tens of millennia ago) from the lowest inclusion-recorded temperature of 152°C up to the 178°C observed today at that depth, 2.6 km.

The best uniformitarian picture of temperature history

Clear observational data trump theoretical models any day of the week. Sasada's observation-based graph (my figure 4) provides an accurate overall picture of past temperatures in the borehole. But the basic model of Kolstad and McGetchin (my figure 3) would probably give Sasada's broad 230°C maximum at 2.6 km depth if it were to use a magma chamber radius somewhere between 8 and 12 km, say at 9 or 10 km. That would put the edge of the magma chamber closer to, but not under, borehole GT-2. We could guess that at a 3 km depth, the model's temperature curve would look much like the solid (12-km-radius model)

line for a 2 km depth. It would rise relatively rapidly to a broad maximum (higher than 230°C for the 3-km depth) and then decrease slowly toward a temperature dozens of degrees lower than today's 197°C at a 3km depth.

Then, Sasada's data show, the temperature rose relatively rapidly by several dozen degrees to today's values. That would fit a model much like that of Harrison *et al.*, that of a recent nearby intrusion of molten rock about 24,000 years ago, if they had started from a baseline of, say 170°C (extrapolated from Sasada's 152°C at 2.6 km depth), instead of 87°C. That would mean the intrusion would be much smaller, or perhaps farther away. Then, most of the heat in the borehole would be residual heat from the magma causing the main eruption.

If I were uniformitarian, here is how I would put the three papers together into my best estimate of past temperatures at a 3 km depth:

> Temperatures rose to a broad maximum, say about 250°C, about 0.9 million years ago, declining slowly to a minimum of about 170°C twenty thousand years ago, then rising rapidly to today's 197°C.

So in reporting uniformitarian temperature views, Loechelt was right about a recent temperature increase, but wrong in ignoring previous higher temperatures lasting for (an alleged) many hundreds of millennia.

Explaining the large helium retentions

As our RATE papers showed,[9] the observed helium retained in the zircons at 2.9 km depth was 17% of the helium originally deposited (deduced from the lead in the zircons). Figure 5 here shows our experimental and predicted model diffusivities (which overlap nicely) for the zircons from borehole GT-2. These are the data whose implications Loechelt and other old-earthers find so disturbing. We can use these data to test three new uniformitarian temperature models for the helium retention. They would give:
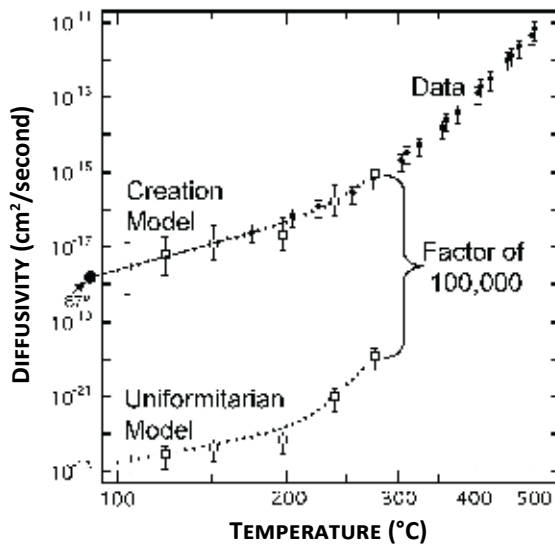
Figure 5. RATE measured and modeled zircon diffusivities.

1. *Best uniformitarian estimate* (see above): Regardless of how low the temperatures might have been before the volcanic eruption, the broad maximum of 250°C, lasting for hundreds of millennia, would wipe out most of the helium that might have accumulated in the previous 1.5 Ga. The steady decline to 170°C during a half-million years would also be significant. The brief spike up to 197°C during the last 20 millennia would be insignificant by comparison. The whole estimated temperature history would leave less than 0.002% of the helium.[10] That is far lower than the observed 17% retention. Assuming no diffusion losses before the eruption, and a constant 197°C since then, would give a 0.004% retention, which is still far too low.

2. *Long chill*: Assume that temperatures were as low as the "baseline" value of Harrison *et al.*, 87°C, for the *entire history* (allegedly 1.5 Ga) of the zircons, except for the last year before the cores were extracted, when the temperature would suddenly rise to the observed 197°C. Then use the "87°" point on the left part of figure 5.[11] That gives a retention of only 0.00064%,[12] far below the observed retention, 17%.

3. *Deep freeze*: Assume the temperature was *minus* 100°C from 1.5 Ga ago until 1 Ma ago. Then it would rise to 87°C, staying at that level until the last year before extraction, when the temperature would rise to 197°C. The result is a retention of less than 0.5%,[13] still much lower than the observed 17%.

Model 1 shows that if the volcano erupted the alleged one million years ago, essentially no helium would remain. Model 2 is generous to uniformitarians, because it completely ignores heat from vulcanism in the area. It gives even lower retentions. Model 3 bends over backwards to accommodate Loechelt, using unbelievably low temperatures, yet it still gives far too little helium remaining. These results show that his hope to explain the large observed helium retentions with low temperatures is utterly futile. The zircons are just too leaky, and 'geologic ages' are far too long. The helium data still strongly support the biblical timescale of 6,000 years.

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4. Humphreys, D.R., Critics of helium evidence for a young world now seem silent, *Journal of Creation* **24**(1):14–16, April 2010. Also see Humphreys, R., Helium evidence for a young world continues to confound critics, 29 November 2008; creation.com/helium-evidence-for-a-young-world-continues-to-confound-critics. To the list of Internet critiques in this article, one could add Loechelt's Internet responses to it, which he cites above in his references 5 and 6. As he mentions above, on 5/18/2009 he sent an e-mail to the editors of this journal, merely giving links to his two new articles. The editors forwarded it to me, asking if I wanted to reply on the CMI website or in *Journal of Creation*. I replied to the editors that in the new (non-peer-reviewed) articles Loechelt pointed to a complex theoretical analysis he did in a previous article (also not peer reviewed) to bolster his claim that we didn't get rid of all of the "loose" helium (my term) in the zircons. However, my earlier reply (this reference) to the same issue is still quite adequate. I suggested to the editors that further replies from me should await Loechelt publishing a peer-reviewed article, especially one with a new experiment (not theory) to test his claims.

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9. Humphreys, ref. 3, table 1, p. 29.

10. Crank, J., *The Mathematics of Diffusion*, Oxford University Press, Oxford, England, p. 104, 1979. Ignore helium production in past 1 Ma as small compared to previous 1.5 Ga. Then use Crank's eq. (7.3) to show that for time-varying diffusion coefficients $D(t)$, one can replace $D\,t$ in solutions with the time integral of D. Doing that in eqs. (12) and (13) of Humphreys 2005, p. 50, assuming $D = 0$ before 1 Ma ago, and using values of D from figure 5, gives a retention less than 0.002%.

11. Humphreys, ref 3, pp. 34–36. I got the 87°C diffusivity, about 2×10^{-18} cm²/sec, by simply extrapolating the "defect" line (often seen in diffusion data for natural minerals) in figure 5 here a little down from our lowest point of 100°C. The line is established by the experimental data, and confirmed by diffusivities predicted using observed retention data in our creation model. Erratum: the second exponential in eq. (3) should have a minus sign, as does the first exponential.

12. Humphreys, ref. 3, p. 53. Use the 87°C diffusivity for D and a t of 1.5 Ga (4.7×10^{16} sec) in eq. (16).

13. Humphreys, ref. 3, p. 50. The -100°C temperature, though ridiculously low to any geoscientist, would effectively stop diffusion (pp. 61–62), allowing helium to accumulate for nearly the entire 1.5 Ga. Use the 87°C value for D and a t of 1 Ma (3.1×10^{13} sec) in eqs. (12) and (13).

Is Hatshepsut the biblical 'Queen of Sheba'?

I read with interest what Patrick Clarke has written in his attempt to discredit the identification of Hatshepsut with the Queen of Sheba.[1] Most of what he has written is devoted to discrediting Velikovsky. I would agree with him that Velikovsky was stretching things in trying to identify the name Sheba with part of the name Hatshepsut. But he has a point in observing that the Hebrew text does not say 'Queen of Sheba' but 'Queen Sheba'. If it is 'Queen Sheba' it is not apparent what is meant. If Sheba was a place it is true that most scholars identify it with Yemen in Arabia, but according to Genesis 10:7 Sheba was a grandson of Cush and the land of Cush was directly south of Egypt.

However, Josephus and Jesus Christ were a lot closer in time to these events than we are. The former wrote,

> "There was then a woman, queen of Egypt and Ethiopia. When this queen heard of the virtue and prudence of Solomon, she had a great mind to see him. Accordingly she came to Jerusalem with great splendour and rich furniture.[2]"

Ethiopia is here translated from the word Cush and refers to Nubia directly south of Egypt, a nation the Egyptians frequently invaded and ruled over. Jesus called her "the queen of the south" Matthew 12:42, a term which applies to Egypt.

I never met Velikovsky himself though I did spend time talking to his daughter Shulamit. Velikovsky was a brilliant scholar and is to be congratulated for being the first to ring the alarm bells on the traditional chronology, but I do not agree with all he wrote. He frequently tried to make a play on names which I consider to be unnecessary and sometimes confusing. But to discredit Velikovsky does not discredit the reduced chronology he advocated.

I would also point out that a reduced chronology is not dependent

on the identification of the Queen of Sheba with Queen Hatshepsut. Chronologically it matches, and it would be nice if it is valid, but the crucial issue is not the identification of Hatshepsut with the Queen of Sheba but whether the Third Intermediate Period (TIP) dynasties were successive or contemporary with other dynasties. If Hatshepsut went to East Africa rather than to Jerusalem it makes no difference to the validity of the revision.

Clarke's criticism is mostly negative, citing lack of evidence rather than evidence that would contradict the revision. Arguments from silence can never be regarded as conclusive. He himself wrote, "absence does not prove anything".

Clarke wrote "The Bible indicates that her principal motive was to test Solomon 'with hard questions', and not to obtain goods through an oracle of her god, as the Egyptian text recounts." True, but it is unthinkable that the Queen of Sheba would arrive empty handed, and oriental custom would require an exchange of costly gifts.

Clarke says "Velikovsky's 'revised chronology' has been rejected by nearly all mainstream historians and Egyptologists", but it would not be correct to claim that no reputable scholars support the reduced chronology. Clarke refers to Peter James and his book, *Centuries of Darkness*. Professor Colin Renfrew of Cambridge University wrote an introduction to that book in which he said,

> "The revolutionary suggestion is made here that the existing chronologies for that crucial phase in human history are in error by several centuries, and that, in consequence, history will have to be rewritten … I feel that their critical analysis
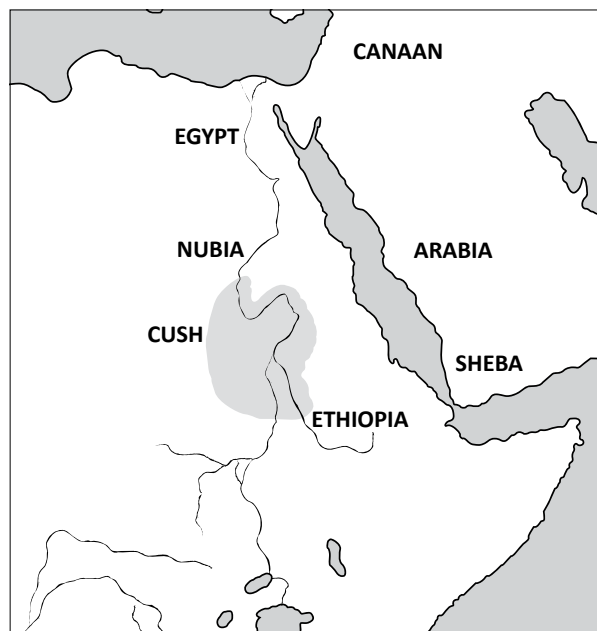

Figure 1. Location of Cush during pharonic times