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Climate of the Past
Discussions

Interactive comment on "Surface paleothermometry using low temperature thermoluminescence of feldspar" *by* Rabiul H. Biswas et al.

Anonymous Referee #1

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General comments:

The authors are investigating an interesting question–whether luminescence signals from K-feldspars in bedrock might archive changes in recent temperatures at Earth's surface. More specifically, they ask whether we can resolve recent changes in temperature periodicity. This question is important and worth pursuing.

I commend the authors for the layout of this study. Their approach involving sensitivity analyses, calibration to sample specific kinetics and attention to climatic complexity have resulted in an interesting manuscript with potential for significant scientific impact.

However, the present work needs significant clarification and expansion to yield a ro-

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bust estimate of past temperatures. Specifically, the authors must determine how changes in amplitude, period, and mean temperature influence luminescence signal growth and depletion in a holistic way. Currently, the treatment is partial. Once this is done, the authors should give a more direct comparison of actual and predicted temperature histories in order for the reader to better examine the predictive success of the model.

These points and others are detailed below.

1/22: "Earth's climate fluctuates in a cyclic way" While there are many internal cycles to climate systems, this characterisation might be too simplistic, especially at the timescales involved here (~10ˆ1 - 10ˆ2 kyr), where abrupt periods of change are common. Temperature changes during the Holocene, for example, can hardly be approximated as cyclic.

1/35: "equivalent diffusion temperature that is always higher than the actual mean temperature" This statement, while true, gives the false impression that this is an intractable bias. For a system with well-characterised diffusion kinetics, the relationship between a given temperature history (e.g., a forward model) and the EDT is well known. In other words, paleothermometry using the He-3 paleothermometry technique must rely upon comparisons against prescribed temperature histories in the same way as paleothermometry with luminescence techniques. This is not a comparative disadvantage of the noble gas technique, but a similar limitation as faced in the current study.

2/6: The distinction between thermochronology and paleothermometry is not entirely clear in the language of this study. If what you aim to resolve is the temporal variation of temperature through time, you are describing thermochronology. If instead, you mean to resolve a past temperature which is representative of some time period (the measurement of which will be affected by seasonal variability and so on), then what you are describing is paleothermometry. I would encourage more precision when you describe these concepts.

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2/12-21: Please add corresponding references for these observations.

2/36ff: The physical meaning of this model is unclear. This is obviously of fundamental importance, as the kinetic model that is chosen will determine all predictions of past thermal history.

From Eq. 1, it would seem that the authors expect to model some number of individual traps, each with a singular values for D0, E, s, s-tilde, rho', a, and b. All of these traps are modelled as disconnected.

And yet, the transition in parameter values from one measurement temperature bin to another is smooth. This is true for D0, for rho', for E, for s, and for b within Fig. 1. This observation strongly suggests continuity in the underlying kinetics, not only for trap depth(s) but for the system as a whole. To fit each measurement bin as a separate and disconnected trap seems suspect. A unified treatment would be preferable.

Another issue to address is whether the same recombination centers are accessed by this distribution of traps during athermal fading. If so, as would seem unavoidable to some degree, rho should be kept constant (the density of centers being a property of the material). rho' can then be related to the underlying activation energy via the alpha term. This should be attempted for internal consistency.

According to the second term on the RHS of Eq. 1, it seems that the authors model thermally-activated recombination locally (since the term is dependent upon the nearest neighbor distribution). If so, then observations of signal loss at room temperature could also be caused by this pathway. This deserves comment.

3/29-30: Can we be confident that the kinetic parameters pertain to geologic timescales? Specifically, is there good evidence or reason to think that mixed order kinetics are predicted at low temperatures and long timescales (natural) as well as high temperatures and short timescales (lab)? Competition effects, for example, could easily produce observations of $b > 1$ for lab measurements whereas the concentration of charge activated on natural timescales would be orders of magnitude smaller.

4/19 and Fig. 3: I find this figure a little difficult to interpret. In particular, the way that you have defined 'memory time' should be a bit clearer. If I've understood the meaning of this metric correctly, one suggestion would be to compare everything against 't-change = 100ka' or to extend the x-axis to include 't-change=150ka.' By doing this, you could then visually show the meaning of the 't-memory' by comparing two horizontal lines, one at t-change=150ka (or 100ka, depending), and the other at the asterisk height. You could then annotate this difference as 20%.

It would be good to consider also the influence of measurement uncertainty. Accurately resolving the difference between [n-bar] = 2.0e-3 and 2.4e-3 would likely involve a lot more relative uncertainty than discerning between, say, [n-bar] = 0.5 and 0.6.

4/30-31: '10 - 100 kyr timescales' This should be a bit more specific. During the Quaternary, the 100kyr and 41kyr periods were most dominant (e.g., Raymo et al., 2006; Hinnov, 2013).

Fig. 4: This is a nice figure. Please adjust so that the 'thermometer' labels correspond to each panel. For example, in panels j-l, if the reader uses the labels on the far right-hand side, they might conclude that the bottom-most series in panel k refers to the 220-230C chronometer.

5/7: 'implies a gradient' I would say this behavior more accurately 'results' from a gradient in thermal stability, given that the kinetic parameters are imposed and known.

5/9: 'the thermal stability decreases with increasing temperature.' I disagree with this statement. The thermal stability for a given TL thermometer is known and fixed in your setup. So it is not the stability that is changing, but rather the probability of detrapping.

5/10: 'the higher temperature TL thermometers remain relatively insensitive to such periodic temperature forcing.' This is misleading. The insensitivity of the higher-T thermometers reflects the mean temperature values that you have chosen. If the temperature oscillated about a higher mean value, the same periodic filling/emptying behavior would be seen with the high-T thermometers. This is evident in Fig. 4 panels j-l, where different thermometers oscillate with comparable magnitude when a range of mean temperatures are tested.

5/13: ’10 ka to 1 Ba’ If Ba represents ’billion years,’ please change to ’Ga.’

5/15: ’For P « tau’ This comparison must be qualified. The lifetime (tau) will depend on a chosen, singular temperature value. Choosing a singular temperature value for oscillating temperature requires some simplification that is not described (e.g., mean temperature? EDT?). Please clarify this issue.

5/16: ’This result implies that smaller periods (<1 ka) do not influence trapped charge equilibrium levels in an oscillating fashion and cannot be differentiated from the trapped charge population resulting from an isothermal condition.’

This statement is incomplete and only conditionally true. For argument’s sake, assume that the 200C TL thermometer has a lifetime of 10 ka at 0C. If the ambient temperature oscillated with an arbitrarily large amplitude (say 100C to make the point obvious) but with a period of only 1 ka or 100 yr, you would find that the fractional saturation would oscillate in response to the temperature forcing, depleting completely and then partially regenerating.

This won’t happen if the temperature period is much smaller than the growth timescale (D0/Ddot). If that is the case, then the thermal imprint upon the sample approaches a steady value determined by the maximum temperature experienced.

Temperature amplitude and the relationship between the forcing period and sample growth timescale both matter. To make this comparison between lifetime and period, these factors must be incorporated.

5/19: ’remains correlated’ and ’deviates from...temperature forcing’ The meanings of these statements are unclear. n-bar behavior is distinct between isothermal and os-

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cillating temperature histories for all periods; it is not an issue of matching and not matching, except for the highest-temperature systems, which are insensitive to the temperatures prescribed here. Please be more specific with these observations and, following from the previous comment, please do incorporate growth timescales, as these are of obvious relevance here.

5/24: 'Therefore, temperature variations can be reconstructed...' Just to reiterate, you must demonstrate the complex relationship among mean temperature, temperature amplitude, trap stability and regenerative timescales before attempting to reconstruct temperature variability. Additionally, what has been shown in Fig. 4 is that, for a given amplitude, different periods leave different imprints upon the shown thermometers. You have not yet demonstrated that you can accurately reconstruct differences in variability. Moreover, the results from Fig. 5 (panels b, c) seem to indicate that you cannot easily differentiate between various amplitudes or periods.

5/31-33: 'This ensures that complex thermal histories...can be reconstructed.' Following from the previous comment, this is not yet demonstrated.

6/11: 'considering that the other parameters are identical.' Unclear what this means. From Fig. 1, it would seem that the kinetic parameters other than the thermal parameters (E, s) vary between the thermometers. Or do you mean something else?

Fig. 7: This is not the most informative way to show your predictive ability. Unlike with thermochronology studies, where the T-t path is really the predicted feature, what you are more accurately doing is predicting the temperature minimum and amplitude (e.g., ll. 5/24-29). So, it would be much more informative to see these values, actual and predicted.

Additionally, the current figure makes it appear as if you are able to resolve the fine structure of the T-t series, which of course you are not.

7/16-18: '[Fig. 7a,b,c shows] it is possible to recover all three thermal histories within the 1-sigma confidence level.' My previous comment will apply here as well. What matters for this experiment is the degree to which you are able to predict amplitude and minimum.

To demonstrate that you could recover an arbitrary thermal history well, you would need a different test.

8/7: 'This restrict...' Grammar

8/15: This dose range, with upper doses at 0.9 and 1.9 kGy may not be sufficient to observe saturation in K-feldspar TL. Please demonstrate that these signals are saturating or add greater doses to better constrain lab saturation intensity.

8/33: 'no effect for doses below 100 Gy' Wouldn't the far more important question be whether there is sensitivity change above 100 Gy? After all, the majority of given doses are above 100 Gy and these responses allow you to determine the saturation values.

9/13-15: 'The rationale here is that all temperatures follow the delta O-18 data, but the amplitude...and mean temperature are unknown.' Please make clear that this is a stated assumption and not an inference from studies (unless it is, in which case state that). I think is not obvious that the climate signal on Mont Blanc would mirror a Greenland ice core signal, so this assumption probably warrants justification.

Fig. 9: Is Fig. 9 referenced in the main text?

Fig. 9 and 10: As with Fig. 7, please recast to compare the predicted and actual values for the amplitude and base temperatures as these are the variables being investigated.

9/28: 'can constrain thermal history of ~50kyr.' I do not think this has been demonstrated yet, as an extension of my comments regarding pg. 5.

Also, unclear what 'A higher temperature fluctuation' means and whether you've actually shown this.

10/14: 'At those depths [of 7 and 62 cm], mean temperature should be constant.' Certainly, there will be seasonal temperature variability at a depth of 7 cm. Is this what you meant to say?

10/17: For inverse modeling of a natural sample, the time-temperature histories were completely random' If I have understood the previous text correctly, the histories are very much not random, but are tied to the Greenland delta O-18 temperature proxy with variability only in amplitude and initial temperature. Please reword.

Discussions generally: I won't comment much on these inferences about past climate systems at this stage, because I think it will be very important to first demonstrate model success in capturing simple variations within a periodic forcing model, which, at this stage, has not been done.

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