Response to the review of J. Braun. Original comment in black, responses in red.
Richard Ketcham, 19 June 2025.

I have read the technical note entitled: "Incorporating topographic deflection effects into thermal history modelling" by Richard Ketcham with great interest. I believe this is a very useful contribution to the field of quantitative thermochronology, especially because it is incorporated in a widely used software for the interpretation of thermochron data (HeFTy).
I thank the reviewer for the endorsement.

The note proposes semi-analytical solutions, i.e., combining approximate analytical solutions to the 2D heat equation to empirical relationships derived, for the most, from numerical (finite element) solutions of the same equation. These solutions provide useful corrections for the cooling history of rocks being exhumed to the Earth's surface that are then used to compute thermochronological ages and other quantities to be compared with observational constraints.

I have a few points that I believe need to be addressed (all of a technical nature) and that would greatly help future users of the software in their understanding of the method and the advantages and limitations of its use in the interpretation of their data.

1. The corrections that are proposed here are meant to represent the effect of a finite amplitude, potentially time-dependent, topography on the geometry of the underlying isotherms, including the effect of vertical advection. As demonstrated by the authors these effects can be substantial. However, little mention is made of the effect of horizontal advection, which can be, in many cases, dominant over other effects. Indeed, motion of rock particles parallel to dipping faults can cause them to experience cooling paths that are drastically different from those obtained assuming pure vertical advection. Example of this can be seen in our interpretation of thermochron data in the Southern Alps of New Zealand (I refer here to the PhD works of G Batt and F Herman under my supervision many years ago). I do not suggest here that this perturbation be added to the current note/work, but that this fact should be made clear to the user.
I had intended for this point to be covered in the final sentence of the discussion, but had not directly mentioned horizontal advection, so I've adjusted the text to do so in the revised version.

2. The proposed correction(s) are all based on the assumption that the topography has a single wavelength. However, surface topography is often composed of more than a single wavelength. It would be very useful to better describe what the user should use as a topographic wavelength and amplitude in a given situation. For this, it would be useful to indicate that each terhmochronometric system (as defined by its closure temperature or temperature sensitivity) is sensitive to different topographic wavelengths because the depth (and thus the temperature) to which the topographic perturbation propagates is strongly (exponentiallY) dependent on the wavelength. This warrants a short paragraph in this report and, potentially in the user interface of the software, to help the user decide what wavelength and which amplitude is to be used in a real case application.
This is an excellent point. A short paragraph has been added to section 4.1 on data entry in HeFTy.

3. Alternatively, the software could be adapted to use a multi-wavelength topography but this would require that the corrections (for each wavelength and amplitude) be simply combined (added?). This would require additional work to implement and could be quite useful, but potentially beyond the scope of this short note. I leave it to the author to appreciate whether this should be done.
This is an interesting suggestion, but it is indeed beyond the scope of this note. It's not clear if there is a straightforward solution using the approach developed here; I imagine one would have to enter not only the wavelengths and amplitudes but also offsets of the different components.

4.My last point, and maybe the most critical one, concerns the time-dependent solution. If I understand well the author proposes to use the steady-state solutions derived analytically and empirically to cases where the topography grows with time by simply using a time varying value for $H_0$. As shown by the author, this seems to provide relatively good results but my suspicion is that this is because the rate of topographic change remains relatively low compared to the rate of heat diffusion, i.e., the cases he explores must be at relatively low Peclet number. I suspect that a very rapid incision event (that would make the topographic relief grow rapidly with time) would produce a thermal response that cannot be adequately approximated by a series of steady-state solutions. Again, I do not suggest to the author to improve his solution but to warn the users of the limitation (in terms of how fast topography can change) of the method proposed here.
This study did not use steady state solutions, except to set up the initial conditions. The 1D model is used to characterize main part of the time-varying component of the system. Evidently this was not brought out clearly enough, so I've moved some text from section 3.3 to 3.4, added a bit more, and expanded the title of 3.4, to emphasize that this is a dynamic solution.
I also added a test case for very rapid incision (3 km of relief with an 8 km wavelength in 2 million years) and documented the larger discrepancies that arise (which are still modest if considered in terms of vertical or temporal displacement). New text places this test in the context of advising care and considering using other approaches when working beyond the parameter space used to generate this model.
In the course of making these changes, I also found a typo in equation (11), where a minus should have been a plus; this is fixed in the revised text, but the underlying code was/is correct.

I also have a few minor comments:

a. I have tried to reproduce the predictions of this semi-analytical approach to compare them to solutions of the heat equation obtained by Pecube but failed to do so because the value of some constants, or the type or value of basal boundary conditions were not clearly given. It would be good if the author could include all the information necessary to reproduce the results shown in the note.
The basal flux is added in the revised text.

b. I do not think there is an analytical solution of the heat equation in Fox et al, 2014.
The Fox citation without an analytical solution has been removed (they were actually both 2014 papers, and one was erroneously cited as being from 2013).

c. It is not clear what the "relative vertical rotation" mentioned at line 38 is; a few words of explanation would be useful (or a small sketch).
Citation added for where diagram and explanation provided.

d. On line 78, there is a reference to "Finite element modelling"; this sounds like it requires a reference or it should be modified to "Our finite element modelling, shown in Figure X", if this is indeed the case.
The revised text clarifies that this was the finite element modeling performed from this study. The temperature offset is illustrated in Figure 2.

e. On Line 106 the statement that "a constant basal gradient condition never converges to a steady-state with continuous erosion" is not correct, in my opinion. In 1D, the solution is:
$$T(z) = T_0+\frac{G\kappa}{u}e^{Lu/\kappa}(1-e^{zu/\kappa})$$
where $G$ is the assumed basal gradient at depth $L$.
The equation provided by the reviewer may be incomplete, as when I attempt to plug in some numbers it does not produce the desired result; in particular, its derivative (G e^(L z u/kappa)) does not produce a gradient of G at depth L unless the erosion rate u is negligible. However, assuming that there's another solution, this may be a question of how one defines and enforces the condition. Mancktelow and Grasemann (1997) claim that a steady state solution is only possible if the temperature approaches a constant value at depth, and my statement was essentially agreeing with this; with a constant-flux boundary condition and no other enforcement, the basal temperature rises continuously. But, if one sets a constant temperature at depth, eventually a stable steady state gradient will arise associated with it. To resolve the ambiguity, I've replaced my statement with Mancktelow and Grasemann's.