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Interactive comment on "A linear inversion method to infer exhumation rates in space and time from thermochronometric data" *by* M. Fox et al.

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Dear Esurf Editors,

The comments highlight key parts of the manuscript that were unclear. First, we provide an explicit description of potential scenarios where our method is inappropriate. Therefore, we have added more text in the discussion highlighting the potential and limitations of our approach. Second, through linearizing the problem we have introduced potential features and limitations of the results that need to be highlighted. To highlight the linearity of the problem, we show the evolution of closure depths during the iterative process. We have also modified the text to explicitly state that we do not

equate posterior covariance to true model uncertainty. A full analysis requires varying the imposed model parameters and this provides a complete ensemble of acceptable exhumation rate histories.

In addition, an extensive literature related to inferring time temperature histories from fission track data was not included in the manuscript. This was to keep the introduction brief and focus on techniques that infer exhumation rates from thermochronometric data. As there has been considerable work in this field focusing on developing new inverse methods, a section detailing these methods has been added.

Thank you for your consideration,

Matthew Fox

Response to Reviews

Response in *italics*

Reviewer 2: Roderick Brown

Over the last quarter century or so, since the publication of Dodson's seminal paper on the closure temperature concept for geochronologic systems, the field of thermochronometry has grown expansively, both in terms of techniques available and applications. Using the data to estimate exhumation rates has been central of most applications, and the development and advancement of low temperature techniques such as apatite fission track analysis and (U-Th)/He analysis has underpinned much of this work. The topic of this paper is therefore of broad interest, as it offers an efficient technique for deriving models of spatially and temporally variable exhumation rates from regional (geographically dispersed) data.

However, in my view there is a potential dilemma with the rationale underpinning the technique and how the regional model is formulated. The first issue is that the closure temperature concept as defined by Dodson (1973), while elegant and simple to implement, is often not strictly appropriate, especially for the low temperature systems of apatite FT and (U-Th)/He. This concept of how thermochronometric systems behave relies on monotonic cooling and does not therefore allow for any history which is not monotonic. This introduces a dilemma because if you do not know what the thermal history of a sample is it is not possible to decide whether the closure temperature concept is appropriate for interpreting that sample or not. Although in some cases utilising multiple thermochronometers (with variable temperature sensitivities) may ameliorate this problem to some extent, I suggest it is not a pedantic point for the following reasons. The major advances in thermochronometry in the last few decades have arguably been in establishing the understanding of how various systems work in detail and thus being able to recognise and quantify the degree to which a measured "age" is partially reset or not. For low temperature systems/techniques that are the subject of this paper this

is particularly true.

Our analysis is not suited for scenarios where the Dodson's concept is not applicable and where ages from multiple thermochronometric systems have not been obtained. Our analysis is suited to places where ages have been obtained using multiple thermochronometric systems and from a range of elevations (or depths), and when the spatial pattern of exhumation rate is not known. If ages have been obtained from a range of elevations using the same thermochronometric system, our results are relatively insensitive to the closure depth and thus Dodson's approximation. We have made this clearer in the manuscript.

We have added a paragraph in the introduction that provides information about inferring cooling histories from thermochronometric data. In particular, we focus on methods that exploit the temperature dependent annealing of fission track lengths, as this has received the most attention. We deliberated including a section on FT modeling in the manuscript as many of these approaches focus on inferring cooling histories, not exhumation rates. In order to convert cooling histories to exhumation rates is not trivial and requires additional assumptions and/or thermal models. However, we acknowledge that there have been major advances in inferring space-time variations in cooling rates that are relevant to the manuscript.

For example, in the fission track approach the distribution of track lengths within a sample provides the basis for constraining the temperature trajectory through the so called partial annealing zone and the 4He/3He step heating approach can be used in the same way to identify samples that have partially degassed (U-Th)/He "ages". This is important because the closure temperature concept, as described by Dodson (1973) and utilised in this paper, cannot be applied to samples that have partially reset ages. If the sample thermochronometric "age" is partially reset, by prolonged and non-monotonic residence within the partial annealing or retention zones respectively, then the age has no simple relationship with the depth to any isotherm and the rate/s of exhumation over that interval. This is precisely why most other inversion approaches for deriving exhumation histories from thermochronometric data ustilise not only the measured age, but also the information that most directly links to the rate and style of exhumation such as track length distributions or 4He/3He step heating profiles. Not including these measurements seriously limits the practical viability of the proposed approach to regional data. A simple hypothetical situation can be used to illustrate this dilemma. Consider a sample with an apatite FT age of say 12 Ma. This sample may have cooled effectively instantaneously (i.e. very rapid rate of exhumation) at circa 12 Ma or it may have experienced a protracted period of cooling (and possibly reheating) over a period of 100 Ma or more depending on the exact trajectory through the partial annealing zone. Assuming that the sample cooled monotonically after cooling below a notional closure temperature for the apatite FT system would possibly yield excellent fits to the measured ages but yield spurious T-t/exhumation solutions in the later case. The approach as described here may however be more robust in the case of collocated sets of samples (at different depths/elevations), such as the Denali case history. Here, the additional information inherent in using multiple samples can overcome the ambiguity problem because the shape of the age vs elevation profile to some extent encodes the variability and style of exhumation. Even here though, there are cases where the age vs elevation gradient does not reflect the exhumation rate in any direct manner such as when the age gradient is set by a protracted period of no exhumation and partial annealing/degassing, or even slow burial.

We acknowledge that complex burial and reheating histories cannot be inferred using our approach. For such scenarios, fission track length data or 4He/3He step degassing data have proven to be invaluable. We did not include these data in the analysis at present. We have modified the text to clearly state that we are unable to infer reheating events and do not exploit information contained in the track length and 4He/3He measurements.

We agree that the case of inferring a unique time variable exhumation history from a single FT age of 12 Ma (without track length data) is impossible. Sensitivity tests isolating model parameters (thermal model, prior exhumation rate, time interval length) will highlight the lack of resolution, and the large uncertainty, over the last 12 Ma. Different exhumation histories could then be excluded based on the analysis of the track length data. Alternatively, additional information, obtained through the analysis of track length data, could be incorporated into the prior model. In summary, our analysis would not be able to distinguish between the two end members highlighted, and this would be shown to the reader through the analysis of calculated resolution. Furthermore, this problem is not one that we would attempt to resolve with a single FT age. However, there are many orogenic settings where ages have been obtained across a wide range of elevations using multiple thermochronometric systems. In these scenarios our approach will be more useful.

Given the above discussion I feel some mention and discussion of the modern approaches to thermal history/exhumation inversion, developed over the last 25 years since Dodson's work, is warranted, in fact it is essential for a paper on this topic in my view, if only to clarify why the authors consider the Dodson concept to be routinely applicable and viable. Some additional experiments, conducted on synthetic data sets perhaps, to quantify and investigate the scale of any errors introduced for situations where samples have experienced protracted and complex exhumation histories would also significantly improve this paper, especially for data sets where multiple thermochronometry methods are not available. The synthetic data illustrated in Fig. 9 arguably is too simplistic as it does not include samples that have experienced non-monotonic trajectories through the respective temperature ranges for each system.

We have modified the text to highlight cases for where our analysis is not suited. These include scenarios where the Dodson's concept is not applicable, for example during burial and reheating events, and where ages from multiple thermochronometric systems, or elevations, have not been obtained. As we state above, our analysis is suited to places where ages have been obtained using multiple thermochronometric systems and from a range of elevations (or depths), and when the spatial pattern of exhumation *rate is not known. We highlight this point explicitly in the revised manuscript. Furthermore, a paragraph detailing the development of inverse methods to infer cooling histories from fission track measurements has been included.*

Reviewer 1: Malcolm Sambridge.

This is a well written paper describing a linear discrete inversion scheme (the stochastic inverse (Franklin, 1970, Tarantola and Valette, 1982, cited) applied to thermochronological data to estimate exhumation rates. The text is clear and all relevant details look to be present. Numerical examples are nicely presented and the study looks convincing. I have some general and specific comments that may aid a revision.

As is explained the problem being addressed is mathematically nonlinear due to the coupling of exhumation rates with closure depth. Both are to be constrained by the data but the treatment is to separate the two as knowledge of the latter converts the former into a linear problem. A coupled nonlinear inverse problem is then treated as a linear one for fixed closure depth. Although there is some discussion on closure depth computation, I was left unclear how and whether the solution of the coupled problem relates to the full solution of the nonlinear one, both in terms of estimating parameters and determining uncertainty and resolution which are all based on linear theory. While it is often a good idea to exploit such internal linear relationships in an inverse problem, e.g. in iterating to a best fit solution, the real problem is actually nonlinear and there may be no guarantee that such an iteration converges to the solution of the nonlinear problem, or, more importantly, whether the linear uncertainty analysis on a subset of the unknowns reflects the true picture for all unknowns.

We have included a figure in the text that highlights how closure depth evolves during the iterative process. As we calculate closure temperature using a cooling dependent approach, the problem is more linear than expected. The increased exhumation rate leads to higher geothermal gradients. If closure temperature were not recalculated, closure depth would therefore be significantly shallower. However, the increased cool-

ing rate results in increased closure temperatures and increased closure depths. The degree to which this effect is important depends on the degree of temporal changes in exhumation rate. There may be scenarios where the solution does not converge, however, this is not the case here.

Here, we calculate the relationship between exhumation rate and closure depth, Fig 1, using the same thermal parameters as used for the reference model in Section 2.31. Closure depths for apatite fission track (AFT), and (U-Th)/He in apatite (AHe) are shown as solid lines. The dashed line shows surface geothermal gradient as a function of exhumation rate. The relationship between exhumation rate and closure depth is weakly nonlinear, solid black lines. However, the predicted modern surface geothermal gradients vary from 25 – 140 $^\circ$C/km, dashed line. In most scenarios the modern geothermal gradient can be measured to within 20°C/km. In addition, in many scenarios long term exhumation rates can be estimated using alternative techniques. Based on these considerations, perturbations to the closure depth during the iterative process are likely to be small, provided the a priori exhumation rate and thermal model are carefully chosen and calibrated to measured geothermal gradients.

A classic example of how uncertainty based on fixing some unknowns maybe misleading appears in the problem of local earthquake location where uncertainty contours of a hypocenter (based on the a posteriori model covariances) for a fixed origin time can be perpendicular to those when the origin time is allowed to vary (e.g. Figure 4 of Billings et al., 1990).

We have modified the text to state that the posterior covariance matrices do not convey the true uncertainty. Here we test the sensitivity of the results to the full range of imposed model parameters (thermal model parameters, time interval length, prior mean exhumation rate, and the prior variance). The range of results obtained during this analysis highlights, to some extent, the true uncertainty and is larger than the uncertainty based on the posterior covariance matrices.

The stated justification for the new algorithm here, as written, seems rather weak. Pre- vious authors have apparently attempted the same problem with fully nonlinear search techniques and fully three dimensional numerical models, as outlined in the introduction. I think there is room and value in the literature for a linearized approach, especially in quantitative comparison to the fully nonlinear approaches, however I do not think it appropriate to dismiss computationally intensive alternates on the grounds of efficiency. Here the algorithms are computationally cheaper because of the introduction of additional simplifying approximations which may in principle limit the results. As mentioned above, the influence of the linearizing approximations on the results has not been quantified. I think the argument for the present approach is best placed in its use of standard and well established tools of discrete linear inference which act as a useful comparison to fully nonlinear schemes and 3-D forward models.

3D thermo-kinematic models, coupled with non-linear inverse methods, have been used extensively in the literature over the past 10 years. However, these approaches are best suited to cases where exhumation rate can be parameterized using block uplift, topographic evolution and/or prescribed fault kinematics. In scenarios where exhumation rate varies as a function of space and time (and the spatial pattern is free to vary through time) a very large number of model parameters is required to describe the function. This quickly becomes impracticable to infer model parameters using nonlinear methods and the resulting posterior probability density function is difficult to analyze.

There is no discussion of the work of Gallagher and co-workers (e.g. Gallagher et al. 2006 and subsequent papers) who as I understand it have been applying fully nonlinear Bayesian sampling methods to similar inversion problems in thermochronology for some time. I would have thought this highly relevant to the present study, if not in detail of the actual data type being considered, then in the style of approach which seems suited to the problem studied here. Fully nonlinear techniques avoid the limitations of introducing linearizing approximations as is done here. The linear Stochastic inversion framework considered in the present manuscript is related to the fully non-

linear Bayesian framework, with quantities like a posterior covariance and resolution matrices appearing in both. Furthermore in the Bayesian framework of approaches like Stephenson (2006a,2006b) etc non-Gaussian marginal probability density functions can be determined by sampling which is missing in the treatment proposed here. I would have thought that a fully nonlinear treatment of the problem using Markov chain Monte Carlo algorithms would be the best approach, assuming computational cost was not prohibitive. Some discussion comparing the nature of the problems faced and the solution approaches seems appropriate.

We have included two new sections. First, a section in the introduction describing the analysis of fission track length data and how it has been used to infer cooling histories. Second, a section in the discussion highlighting that the analysis of the posterior covariance matrix only provides estimates of uncertainty if the linear assumption is adequate and stresses the need to explore a range of imposed model parameters to get a more complete understanding of uncertainty.

The conversion of tT-paths to exhumation rates requires thermal models and introduces a further complexity. For this reason we had not included this work in the introduction. The potential of these methods to infer exhumation rates would be straightforward, but may require solving a thermal model thousands of times, which may be very computationally expensive. Assuming this is not prohibitive, then a fully Bayesian Reversible jump Markov chain Monte Carlo algorithm may be more appropriate in many scenarios.

It is not clear to me that the reader gets a clear impression of the differences between resolution and a posteriori model covariance matrices. This could be explained a bit better. For example it may be useful to explain that there is a inherent trade off between model resolution and model variance in linear discrete inversion. They are both useful in characterizing what can be resolved in a linear discrete inverse problem, and its welcome that the authors take such care to calculate and discuss them. However I find new comers to the field mayd have difficulty in grasping these concepts when first introduced, and possibly unfamiliar readers will also. Some references to texts such as

Aster et al. or even the more dated Menke (1987) might be useful.

More text has been added to clarify this part of the text.

The final a posteriori model covariance matrix determined in the proposed algorithm is naturally a combination of the prior model covariance matrix and the data itself. Care must be taken in its interpretation as a measure of 'model uncertainty' when the inverse problem is under-determined, as is the case here. A well known, and key issue is that model uncertainty estimates such as displayed in Figure 4 are not just a function of the data but also the prior information imposed. As expected, in the example of Figure 4, early exhumation rates have poor resolution indicating that parameters are not independently resolved, (seen as white rows in the Figure 4a), however, the corresponding rows of the model covariance are diagonal, indicating that model errors here are uncorrelated. This can appear counter-intuitive until one recognizes that the latter merely reflects the prior model correlation matrix because the data have no resolving power in this region. In short its only the differences between the posterior and prior model covariance matrices that reflect the information content of the data. Usually prior and posterior should be displayed next to one another. More importantly, while its tempting to interpret model covariance as a measure of probabilistic 'solution error' this is not possible in under-determined problems unless the prior actually reflects actual probabilistic information on the solution, prior to collecting the data. I see no discussion justifying the prior here and so it appears more like the usual 'prior of convenience'. The present paper does not commit any crimes in this respect but since covariance matrices based on a convenient prior seem to be use, one should guide the reader in how to interpret model covariance matrices and their relation to the prior.

We have explained the difference between posterior covariance matrices and resolution matrices in the revised manuscript. Please see comment above about how we attempt to highlight true uncertainty by varying imposed model parameters.

Interactive comment on Earth Surf. Dynam. Discuss., 1, 207, 2013.

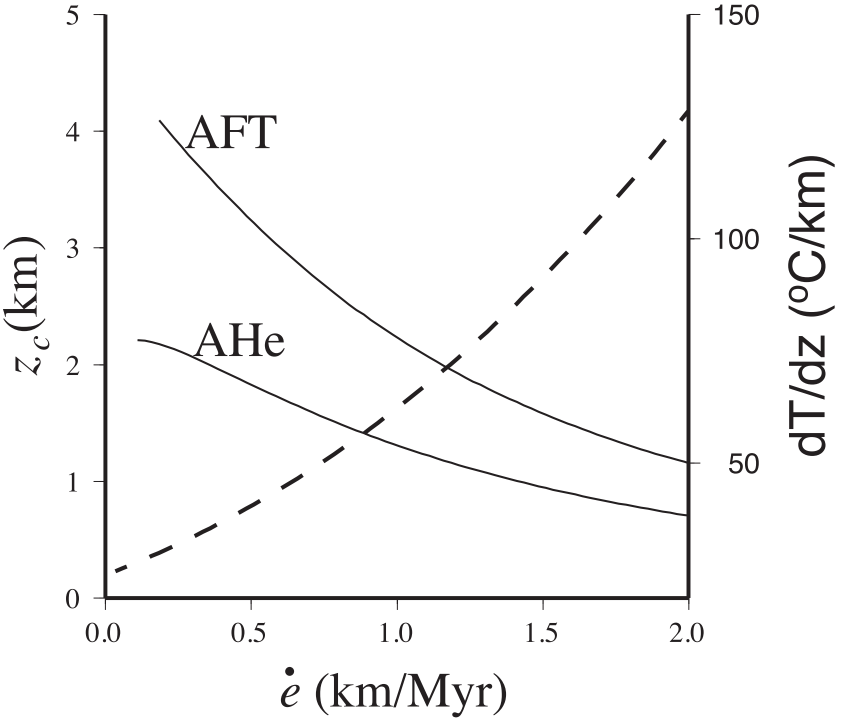

Fig. 1.