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Interactive Comment

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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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

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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.

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).

The stated justification for the new algorithm here, as written, seems rather weak. Previous 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.

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 non-linear 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.

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.

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

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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.

I see no obvious errors in any of the mathematics.

Malcolm Sambridge

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