Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2017-42-RC2, 2017

Interactive comment on "Extracting information on the spatial variability in erosion rate stored in detrital cooling age distributions in river sands" *by* Jean Braun et al.

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Received and published: 30 October 2017

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Review of "Extracting information on the spatial variability in erosion rate stored in detrital cooling age distributions in river sands", by Jean Braun, Lorenzo Gemignani, and Peter van der Beek For consideration for Earth Surface Dynamics.

Recommendation This paper provides an approach for decomposing erosion rates from detrital cooling ages collected from multiple tributaries. The approach is innovative but the quantitative formulation is difficult to follow and the implementation has

major problems that undermine confidence in the results. To be blunt, I have no idea if the proposed formulations give the "right answer". I highlight these problems in my general comments below, and I follow with some specific comments. The paper is not suitable for full publication in its present form, but I think some careful revisions could transform the paper into an important contribution.

General Comments (see Specific Comments below for more details) 1) The paper starts out with a clever idea, to use detrital cooling ages from multiple tributaries to resolve relative modern erosion rates for each of the tributaries. The starting point is excellent.

2) The paper claims to be the first to use detrital thermochronometric data as a tracer for estimating modern erosion rates. This tracer approach has already been introduced by McPhillips and Brandon (2010) and Ehlers et al. (2015). The specific contribution here, using detrital thermochronology as a tracer from multiple nested catchments, is a new and important.

3) There is actually a lot of literature on the formulation and solution of mixing models. I would expect a brief summary of that literature, and also some discussion about advantages and disadvantages of previous methods and the new method presented in the paper. One analysis that I like is in Menke (2013, p. 10-11, 189-199).

4) The main contribution of this paper is a computation procedure that uses observed detrital cooling ages collected from tributary catchments and along the trunk stream of a large drainage to estimate average relative erosion rates for each of the tributary catchments. In other words, the estimation involves inverting the data to find best-fit solutions (expectations and confidence intervals) for the relative erosion rates. Inverse estimation is a well-established field and it makes sense to structure the problem in terms of this methodology. To do so requires a clear definition of the model equation and error function, and the determination of a computation method to optimize the unknown parameters relative to the observed data, using either least squares or like-

lihood. The estimation suggested in the paper provides no tie to statistical or inverse methodology, so it is difficult to know if the estimates will be correct.

5) The paper lacks any testing of the estimation method. The usual approach is to design a synthetic data set with noise, and use that to see if the estimation method recovers the parameters used to generate the synthetic data set. A successful test would show that as the size of the synthetic observed data is increased, the parameter estimates would asymptotically approach the "true" parameter values used to generate the synthetic dataset. I encourage this kind of test to be added to the paper.

6) I don't know why, but the authors decide that they can estimate the best-fit result and the uncertainties using a Monte Carlo simulation. They refer to this simulation as a "boot strap" estimation of uncertainties, but that is incorrect (see specific comment #4 below). In fact, they are using this simulation to estimate both the expectations and the uncertainties for the parameters. They note that they prefer the modes, and not the means, of the Monte Carlo distributions as estimators for the relative erosion rates. I understand their preference in that the Monte Carlo distributions are asymmetric, but they provide no evidence to show that the modes or the means work at all. In the end, it would make sense to solve the inverse problem directly, rather than rely on Monte Carlo distributions. Note that the bootstrap method is very useful non-parameteric method for estimating uncertainties. For the problem here, it probably makes sense to estimate bootstrap confidence intervals (see Carpenter and Bithell, 2000 for details), which require no assumptions about the shape of the bootstrap distribution.

7) There is no discussion of the structure of this estimation problem. Is it overdetermined, underdetermined, or mix determined? One is left to wonder if the constraints (eqs. 15, 16) are handled in a way that is consistent and unbiased with respect to the estimation problem. What is the structure of the errors, and how are the errors accounted for in the estimation algorithm? There is a vagueness about the estimated quantities, whether they are absolute or relative erosion rates. This point should be stated upfront and maintained in consistent way throughout the paper.

8) It is not clear what quantities are being estimated. In the formulation, it would seem that Ck,i are the primary parameters to be estimated (section 2.4), and the relative erosion rates are derived from these parameters. The values for Ck,i are bounded to the range [0,1], which means that their range is truncated on both sides. Constraints are introduced in the formulation (eqs. 15, 16) but there is no assurance that this strategy will give the right answer. In statistics, the well established approach is to remove the truncations by transforming the parameters to a new scale. The logit transform is used for parameters that are bound to [0, 1], where logit(x) = ln(x /(1-x). A positivity constraint for erosion rates can be introduced by a log transform. These strategies commonly result in symmetric Gaussian-like distributions for the parameters, which means that the best-fit solution and confidence intervals are typically well defined. The authors have the view that it is somehow better to fit "raw binned age data" (p. 3, line 10), rather than a probability density function. The binned data are not "raw" in that they are smoothed by the box function used for the binning. The topic of kernel density estimation (KDE) was first established in the mid 1950's has been well defined since about the mid-1980's. What is clear is that the box function used in estimating a histogram is just one type of kernel function. A Gaussian is a much better kernel function for estimating a density distribution. It would make no difference if one used a histogram versus a density distribution for this problem. Silverman (1986) provides a general review of estimating density functions, Brandon (1996) show an extension of the KDE method for use with grain ages with specified standard errors, and McPhillips and Brandon (2010) show how to combine estimated probability density functions to get a relative density function for tracer thermochronology. All of this approach is completely consistent with the formulations proposed in this manuscript. Note that Vermeesch's (2012) paper on grain age distributions provides nothing new to this issue of density estimation.

9) The authors have an application paper, Gemignani et al, 2017, which was published in August in Tectonics. The paper considered here makes no mention of this paper. It is important to provide some explanation of how that paper relates to this contribution.

Specific Comments 1) p. 2, lines 27-28: The paper states that previous publications have not taken advantage of the ability of thermochronologic data to resolve both past and present erosion rates. In fact, McPhillips and Brandon (2010) was entirely devoted to showing how thermochronology can be used as a tracer to estimate modern erosion rates. Ehlers et al. (2015) also has a similar application.

2) p. 3, lines 9-10, 29-30: Not clear why bins are better why to represent the density of the data. The authors imply that the bins can be tuned to an 'event of given "age"', but there is no explanation about why this capability is important or even desired. In addition, there are the usual questions about histograms: How many bins should be used?, How wide should the bins be?, etc.

3) p. 6, Incremental Formulation: This section provides another solution for the estimation problem. It would help if there were some explanation about why a second approach is needed.

4) p. 9, line 12: The numerical estimation is described here as a bootstrap, but the method used is not the bootstrap (Efron and Tibshirani, 1986), but rather an ad-hoc procedure. I am puzzled here because the bootstrap calculation is very simple (replicate data sets produced by random sampling with replacement of the original data set), and it has well defined properties for estimation of uncertainties. In contrast, I have no idea if the ad-hoc procedure used here (randomly removing 25% of the data) is able to provide reliable estimates of uncertainties.

5) p. 9, line 27: It would help to explain here why the closure temperature for Ar muscovite is cited here, given that this information is not used in the paper.

6) p. 12, fig. 4: The horizontal axes have no tic values or axis labels, and the vertical axes are also unlabeled.

7) p. 13, lines 5-9: The estimation method seems to be rather unstable.

8) p. 14, figure 5: This figure is hard to understand. It is my guess that the gray scale

represents, not the estimated erosion rate, but rather the estimated relative erosion rate. Is that correct?

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