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Earth Surface Dynamics Discussions

Interactive comment on "Millennial erosion rates across the Pamir based on ^{10}Be concentrations in fluvial sediments: dominance of topographic over climatic factors" *by* M. C. Fuchs et al.

Anonymous Referee #1

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In their manuscript "Millennial erosion rates across the Pamir dominated by topographic factors", Fuchs et al. present a set of 11 catchment-averaged in situ Be-10 derived denudation rates from the Panj River and its major tributaries in the Pamir Mountains. These rates, ranging from 0.05±0.01 to 1.45±0.56 mm yrˆ-1, have been derived from samples representing catchments of very different size (84 to ∼72,000 kmˆ2) partly hosting extensive glacial cover of up to 55%.

The authors use these rates and their spatial distribution in order to determine meaningful predictors of denudation rates for the Pamir. Using linear regression analysis techniques they find slope steepness to be the most decisive predictor. Building on this, the authors perform multiple regression analysis, finding an additional share of

precipitation in the variables explaining the distribution of their response variable, i.e. denudation rate.

Fuchs et al. compare their denudation rate estimates to previously determined rates of river incision, which are based on OSL dating ages. Said OSL-based incision estimates suggest that river incision rates are up to tenfold higher than catchment-averaged denudation rates in the region. From this apparent mismatch the authors infer transience of the landscape. To explain this, the authors prefer river captures to having caused a strong base level drop and a subsequent incision signal along the Panj River.

General comments:

Fuchs et al. for the first time present cosmogenic-Be-10 derived catchment-averaged denudation rates from the Pamir Mountains. Without doubt these centennial to millennial denudation rates might be of high value for the scientific community, because they bridge a temporal gap between long-term (10ˆ6 yrs) and historical (10ˆ2 yrs) denudation rate estimates. But for all that, regrettably the manuscript appears immature and needs fundamental revisions for the following reasons:

1) The study hinges on catchment-averaged denudation rates that have been derived from partially heavily glaciated catchments. The authors argue to account for this by excluding glaciated areas from nuclide production rate calculation. This is unsatisfying because the glaciated areas will still contribute important amounts of grains to the samples. However, these grains not necessarily will contribute in-situ produced Be-10, i.e. may significantly dilute the Be-10 concentrations, therefore modulating and corrupting the denudation rates that the study is based on. This point deserves serious rethinking because it makes the study highly vulnerable.

2) The study is based on a set of (just) 11 Be-10 derived catchment-averaged denudation rate estimates. The underlying river sand samples represent catchment areas across several orders of magnitude (10ˆ1 to 10ˆ4). Also the derived denudation rates – and hence integration timescales – are highly variable. The authors should consider

accounting for this by fitting their statistical approach to what they have for analysis, for instance by applying statistical resampling techniques.

3) Regrettably, the line of argument is often not to the point and misleading, making the manuscript partially hard to read and to understand. Some exemplary points are:

Having looked at not a handful of possibly denudation-influencing parameters – practically just at precipitation and slope gradient – the authors end up at the notion of a dominance of topographic over climatic factors regarding denudation rates in the Pamir. Indeed, it is not an easy exercise to obtain meaningful predictor data for vast areas such as the Pamir. All the more the authors should be cautious with their statements, which often leave no space for alternatives. This is a general issue throughout the entire manuscript. # The introduction is, regarding the subject of the study tepid and also digresses from the region of interest. # The Be-10 part of the Material and Methods section is overinflated. There the authors describe Be-10 standard methods in very detail, stretching the manuscript hereby unnecessarily. # The coefficient of determination (Rˆ2) is repeatedly treated as an overall quality measure; even for directly comparing regression models fitted to response distributions to very different predictors. # River capture is the main explanatory argument for surmised transience of the landscape under investigation. Unfortunately, the authors do not show any indication for that. They just give one stale reference to Fuchs et al., 2014, which is definitely not fueling the prominence of the argument.

The manuscript should not be accepted for publication in its current condition, firstly for methodical reasons. Certainly it would greatly benefit from being carefully edited regarding argumentative rigorousness, and language.

Detailed comments (specific and technical):

PAGE 3 - ABSTRACT

Line 3 // . . ., whereas the relative contributions . . . tectonic and climatic factors: Word

missing?

Line 7 ff // . . . quantify basin-wide erosion rates from cosmogenic 10Be concentrations: The authors may consider rephrasing to denudation rates throughout the manuscript (cf. von Blanckenburg, 2005, EPSL; Dunai, 2010).

Line 17 // Unclear. Consider rephrasing! Perhaps "Dry conditions and low slope angles are unfavourable for sediment transport and consequentially, erosion on the plateau.“

Line 18 // Consider talking about high rates, not about high erosion (resp. denudation).

Line 18 // . . . predominant: Predominant in terms of what? Consider rephrasing.

Line 21 // . . . in Pamir: in the Pamir.

Line 24 // Consider rephrasing. Perhaps “suggesting” or “pointing to”.

Line 24 // . . .We propose. . .: Consider rephrasing this sentence (and split into two)!

PAGE 4 - INTRODUCTION

Line 9 // . . . the orogen: Which orogen? : an orogen

Line 17 // . . . in evolving mountains: mountain evolution?

Line 18 // . . . the factors behind: Ambiguous! What are factors? This is very general and unspecific. Consider rephrasing!

Line 19 // Sure? Are the authors aware of the work of Carrapa, Thiede, Sobel and others?

Line 24 ff // Within the following paragraph you lose contact to your study (area). Indeed, there is a knowledge gap regarding denudation processes on millennial timescales in the Pamir. Therefore: Consider focusing more on the Pamir, and on the problems that you are going to look at in your study; unnecessary to give excessive general introduction to cosmogenic nuclide applications, and to the calculation of catchment-averaged denudation rates. Focusing on the aims of the study in the Pamir

will make the introduction much stronger and to the point.

PAGE 5

Line 22 // . . . depositional sites. . .: Not sure what you address here. Sediment storage? Consider rephrasing.

PAGE 6

Line 19 // . . . This paper is. . .: Consider rephrasing because you nourish the impression of selling your work the salami-slicing way. Why not just refer to previous studies and cite Fuchs et al. 2013, 2014?

PAGE 8

Line 7 // The setting. . .: Which setting?

PAGE 9 – MATERIAL AND METHODS

Line 5 ff // Regarding subsections 3.1 – 3-3: Consider shifting large parts of these subsections to a Supplementary material file!

PAGE 13

Line 5 ff // Shielding by permanent ice and snow cover: This is a central methodical point! Except for sample TA08N, shielding by ice and snow is far beyond any negligible value. Your study stands and falls with meaningfully dealing with ice shielding. Please see comments to p17, line 14 and p23, line 26ff

PAGE 15 - RESULTS

Line 8 ff // . . . strong east-west elongations. . .: Consider rephrasing! What about “preferential latitudinal orientation”? Elongation is not the best term in this regard.

Line 11 // . . . changes in controlling factors. . .: Factors controlling what? Be more explicit!

Line 13 // . . . gently decrease. . .: Gently? Maybe “slightly”; Sharply? Maybe “markedly”

Line 16 // . . . total altitude differences . . .: Is this basin relief? Then term it basin relief.

PAGE 17 – EROSION RATE PARAMETERS

Line 14 ff // Again, this is crucial to the entire study and deserves to be explained better. By analysing grab samples of river sands you are consequentially also looking at sediments with low (or no) Be-10 concentration that stem from glaciated areas, which you excluded from the analysis. Also you do not differentiate between glaciated areas and areas with snow cover. Is this just a verbal issue or do you put snow cover on a level with ice cover?

Line 17 // . . . but. . .: except

PAGE 18 – BASIN-WIDE EROSION RATES

Line 17 // The basins do not reveal anything; your data, i.e. the denudation rates do perhaps.

Line 26 // Please see previous comment

PAGE 20

Line 7 // . . . slope scaled the. . .: Word missing?

Line 14 // . . . deliver. . .: ? Maybe “Linear regression is standard means to. . .” Remark: Since you are dealing with a quite small, but highly variant data inventory your regression analyses may gain robustness by applying e.g. a bootstrap scheme, i.e. by resampling.

Line 17 // The basin-wide erosion rates are proportional to altitude difference within basins, but highlight the scale-dependent relation between relief estimates and erosion rates.: altitude difference? Again, is this relief? Anyway, this sentence has to be rephrased because it does not make sense.

Line 19 // The basin relief (BR, Fig. 6a) shows no correlation, while reducing the window size of the local relief (LR, Fig. 6a) to 1km yields an R2 of 0.68.: Hard to understand. Consider rephrasing. Remark: Be more precise here (and in general when taking Rˆ2 into consideration). When looking at Rˆ2 we are looking at the portion of the variance of the response variable (y) that is explained by the model.

Line 21 // The highest correlation to erosion rates is attained with basin slopes. Using the median slopes yields an R2 of 0.73 and the 0.75 quartiles an R2 of 0.81 (Fig. 6b). The correlation of erosion with slopes suggests that the slope-weighted erosion rates for the inferred sub-basins GUNT, BARlow, ISHs and ISHn suite the primary relationship found in regression analyses (Table 3).: Not very handy. Consider rephrasing.

PAGE 21

Line 1 // . . . regardless of low or high precipitation. . .: Regarding precipitation we are not talking about orders-of-magnitude differences, but rather about a 1 : 1.4 ratio.

Line 6 // We performed a multiple linear regression analysis. . . : You assume additive effects of slope gradient and precipitation. Other regression models are thinkable. It might be useful to explain to the readers why you go for additive model.

Line 7 // . . . Including more components result in. . . : resulted

Line 16 // As stated above, the 10Be-based erosion rates average over the time interval needed to erode the characteristic attenuation depth of about 60 cm.: Redundant. Remove.

PAGE 22 - DISCUSSION

Line 2 // Changes in conditions during this period are likely but the large areas of low slopes formed by sediment-filled valleys of the inner Pamir are indicative of low erosion persistent over long time scales.: Also important → the influence of short-term environmental fluctuation decreases with increasing nuclide concentration, i.e. decreasing denudation rate resp. increasing averaging timescale.

Line 15 // . . ., which is not indicated along the Panj. . . .: Not clear. What do you refer to here? Which are said indications?

Line 16 // . . . the generally high slopes. . .: high slopes? Or rather high slope angles resp. gradients? You also should make clear how high slope (angles) may be related to sediment transport over long distances.

Line 25 // Their high effects on sediment delivery to river channel decrease fast within time intervals at decadal scale or longer (Wolman and Miller, 1960; Korup, 2012).: Ambiguous. Consider rephrasing.

PAGE 23

Line 3 // . . . Alichur Dome based. . .: Word missing?

Line 8 ff // The erosion rates of $< 0.5 mm yr^{-1}$ are low compared to the Pamir average and most other Panj as well as tributary basins. This delineates areas with long-term steady-state on the plateau of Pamir from marginal basins undergoing a transient stage with higher erosion rates of ~ 0.7 at a millennia scale.: Hard to follow. Which rates are you talking about?, and BTW is the difference between $\sim$0.5 mm yr-1and 0.64 (or even 0.7) mm yr-1 worth talking about? These rates overlap within errors, right? Consider rephrasing.

Line 26 // In particular, the sediment delivery from glaciated areas requires attention. Such areas contribute sediments that likely experienced negligible 10Be productions rates. Excluding glaciated areas (Fig. 6a) lowers the production rates on the basin scale but this may be insufficient in the case of large quantities of glacial sediments in the sampled material.: In this short paragraph you touch the main drawback of your study. Please see other comments regarding this point.

PAGE 24

Line 15 // . . . OSL-based incision rates. . .: Be more explicit here. Perhaps “reported OSL-based incision rates from the XYZ region”

Line 17 // . . . indicate dominant control from local rather than temporal factors. . .: Temporal factors? What do you mean?

Line 18 // . . . The discrepancy between rates implies . . .: Which rates? Help the reader to follow your train of thoughts.

Line 18 // The discrepancy between rates implies that the basin-wide erosion does not balance the lowering of the local base levels induced by the intense fluvial incision of the Panj at the Pamir margins.: Said discrepancy might have many other implications. No need to be so decisive here.

PAGE 26

Line 8 // Steep slopes are also the primary factor controlling erosion in Pamir Since you did not check any thinkable control on denudation in the Pamir you should consider being less absolute in your wording. Out of the set of predictors that you have been looking at slope gradient performs best.

Page 27

Line 4 // The millennial, . . .: Aren't > 50% of your rates below 1 kyr?

Line 13 // . . . variations. . .: Isn't it rather the variance?

FIGURES

Fig. 1 // A) Black labelling on dark background hard to be read; A, B) font too small, enlarge significantly, BTW What for do your readers need the for numbers right after "TA" in the sample IDs? If unnecessary: remove, save space and show sample IDs in the maps.

Fig. 2 // Labelling too small

Fig. 3 // Shift to Supplementary Material (together with the largest parts of the Material and Methods section)

Fig. 4 // No need to be shown in the main text. Consider shifting to Supplementary Material

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Interactive comment on Earth Surf. Dynam. Discuss., 3, 83, 2015.