Earth Surf. Dynam. Discuss., 3, C125–C144, 2015
www.earth-surf-dynam-discuss.net/3/C125/2015/

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.

M. C. Fuchs et al.

fuchsm@student.tu-freiberg.de

Received and published: 17 June 2015

[12pt]report

Response to interactive comment ESurfD 3, C17–C26, 2015 - Anonymous Referee #1

MS title: *Millennial erosion rates across the Pamir based on ^{10}Be concentrations in fluvial sediments: Dominance of topographic over climatic factors*
by: Fuchs et al.
17 June 2015

We thank the reviewer for the critical comments and recommendations to improve the manuscript. In the revised version, we follow the major and minor points raised as stated in detail in our responses below. Major changes include:

- erosion rates are changed to denudation rates
- amended title: Denudation rates across the Pamir ...
- we expanded the sections on glaciated / snow and ice covered areas of our basins
- expanded the explanation on our linear regression analyses
- shortened the introduction chapter focussing on the Pamir
- shortened the material and methods chapter
- new supplementary material file including parts from material and methods which present standard procedures and two result figures (former numbers 3 and 4)

Few minor recommendations became irrelevant during editing as a consequence of addressing the comments of both reviewers.

General comments:

#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.

REPLY: *First of all we recognise that the focus on heavily is highly misleading. We did not intend to convey the idea that glacial erosion is dominant in the catchments but that glacial melts contribute to a large extent to superficial water. We have amended the wording accordingly. Additionally, we expanded the discussion of the effects of glaciated areas on our calculated erosion rates in the discussion chapter on spatial variations to account for the important points raised by the reviewer. We included that glacial contribution to sediment yield is still under debate (e.g., Norton:2010go, Godardetal2012) and consider possible effects on CN concentrations and production rates. We note that*
- retreating glaciers suggest low efficiency of erosion (overall since the last 100 kyrs (e.g., Abramovski et al. 2006, Roehringer et al. 2012), since the Little Ice Age 15 % in the western Pamir and 3 - 5 %) in the central and eastern Pamir (Aizen 2011), while also no or slight gain is reported as Karakorum-Pamir anomalie for 1999 - 2011 (Gardelle et al. 2013)
- the glacial extend based on MODIS satellite data do not account for any possible millennial-scale retreat, but integrate permanent snow and ice covered areas - further data is needed to quantify the sediment contribution from the glaciers and refine the absolute magnitudes of the presented rates.
- moraines and glacial deposits exist in the catchments. Nonetheless they are much older than the time span evaluated in the present study. These remnants are mainly on the plateau where erosion is minimal. Contribution to river sediments is thus marginal.
- other meteorological factors, such as temperature and glaciated area do not show any significant relationship with suspended sediment yields on an annual basis, which would therefore suggest subordinate relevance of climate (Pohl E., and Gloaguen Richard, per. comm.)
We argue that is not any systematic influence because glaciated areas and erosion

rates do not correlate.

#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 (10^1 to 10^4). 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.

REPLY: *As this is the first study in the entire Pamir and with extremely difficult samples (very low quartz content) we consider that this study will contribute to the understanding of surface processes in the region. We intend to trace variations in erosion along the Panj looking at the increasing basin and wether this influences erosion estimates and if there is any evidence for long basin internal sediment storage. We accounted for the small data set and possible single values that deviate from the indicated relation (outliers) by using a robust linear regression model that includes resampling techniques. We added the relevant information in the manuscript.*

#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.

REPLY: *We have reformulated this paragraph. Indeed, we did test also altitude, basin size, glaciated area and different measures of relief to explain the variation of erosion rates. We then focussed on more meaningful parameters in the manuscript, figures and tables, and selected predictors that are independent to avoid co-linearity. Tested parameters compare to the ones stated in the literature. We did not include vegetation that is generally scarce and depends in the dry Pamir on precipitation. Human impact is spatially extremely limited. Differences in in an already homogeneous lithology within the orogenic belts are averaged by the large size of most basins.*

The introduction is, regarding the subject of the study tepid and also digresses from the region of interest.

REPLY: *Thank you for this critic. We agree. We reformulated the introduction with focus on our aims and the Pamir. We note that our CN based erosion rates are the first for the entire Pamir region and hence, we employ data from neighboring regions to summarize the state of the art and the current research questions.*

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.

REPLY: *Ok, we shortened the material and methods section and shifted the part which we consider to be standard to the supplement.*

The coefficient of determination (RËĘ2) is repeatedly treated as an overall quality measure; even for directly comparing regression models fitted to response distributions to very different predictors.

REPLY: *We agree with the reviewer that R^2 alone is not sufficient to test the significance of correlation. On the other hand we do not propose to provide a predictor ranking. We solely intend to discriminate the description potential of climatic and geometric parameters. The coefficient of determination is a well know estimate to test predictors although in sensu strictu it should only be applied to normally distributed data. Because of the limitations and the small data set we used a robust linear regression model. We decided for R^2 for comparability to other studies that use the same measure. We added details to the text for clarification.*

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.

REPLY: *The explanation for the transient landscape is indeed provided in Fuchs et al., 2014. As far as we know, there is no contradictory study. It is beyond the scope of this manuscript to explain the evidences that led to the interpretation of river captures. Those are the central part the cited paper (Fuchs et al. 2014). The river captures are non of the central predictors for the variation in erosion rates but are employed to give a possible or likely explanation for the mismatch between basin-wide erosion and fluvial incision at the Pamir margins.*

On the other hand we reformulated this paragraph in order to provide room for alternative explanations.

#4: ... 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.
REPLY: *We carefully edited the manuscript according to the raised general points of concern and implemented changes as stated above.*

Specific comments:

PAGE 3 - ABSTRACT

Line 3 // . . ., whereas the relative contributions . . . tectonic and climatic factors: Word missing?
REPLY: *Thanks, we inserted "of" ... tectonic and climatic factors*

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).
REPLY: *Ok, changed accordingly throughout the manuscript.*

Line 17 // Unclear. Consider rephrasing! Perhaps "Dry conditions and low slope

angles are unfavourable for sediment transport and consequentially, erosion on the plateau.“
REPLY: *Ok, we re-phrased the sentence.*

Line 18 // Consider talking about high rates, not about high erosion (resp. denudation).
REPLY: *Thanks, amended accordingly.*

Line 18 // . . . predominant: Predominant in terms of what? Consider rephrasing.
REPLY: *Ok, we did re-phrase the sentence to: "The highest rates coincide with areas that receive precipitation predominantly from the Westerlies during winter."*

Line 21 // . . . in Pamir: in the Pamir.
REPLY: *Ok, thanks.*

Line 24 // Consider rephrasing. Perhaps “suggesting” or “pointing to”.
REPLY: *We assume "implying" should be re-phrased? We re-worded to "evidence". OR "propose" (see reply to next comment)*

Line 24 // . . .We propose. . .: Consider rephrasing this sentence (and split into two)!
REPLY: *Ok, we re-phrased the sentence and split into two*

PAGE 4 - INTRODUCTION

Line 9 // . . . the orogen: Which orogen? : an orogen
REPLY: *Not relevant any more as the sentence has been changed following the general comment #3(2).*

Line 17 // . . . in evolving mountains: mountain evolution?
REPLY: *Ok, amended.*

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

Line 19 // Sure? Are the authors aware of the work of Carrapa, Thiede, Sobel and others?
REPLY: *Yes, thank you. We re-phrased the sentence. We include their work and clarify the long-term scale of millions of years as they all focus on tectonics and exhumation histories, but give only few quantitative insights into erosion and how respective rates were calculated (or wether just taken to be equal to rates of exhumation).*

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.
REPLY: *Ok, we re-phrased the paragraph to focus more on the Pamir.*

PAGE 5

Line 22 // . . . depositional sites. . .: Not sure what you address here. Sediment storage? Consider rephrasing.
REPLY: *The phrase is deleted in the revised manuscript according to the detailed comment above (Line 24 ff)*

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?
REPLY: *The statement was requested by the editor before open access acceptance of the manuscript as a discussion paper, so we prefer to keep it.*

PAGE 8

Line 7 // The setting. . .: Which setting?
REPLY: *Ok, we re-worded to "position".*

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!
REPLY: *Ok, we added a supplementary material file and shortened the subchapters.*

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
REPLY: *Thank you for pointing at this misleading wording. We added a short phrase for clarification that we aim at meaningful production rates based on the basin area exposed to the cosmic ray flux. We did not intend to convey the impression of negligible shielding from snow and ice but for total shielding in respective areas and hence, excluded respective areas prior to topographic shielding calculation (presented in this section). The drawbacks of excluding snow and ice covered areas (e.g. zero ^{10}Be grains that dilute the nuclide concentration in the river's sediment) are included in later sections and especially in the discussion.*
Additionally, we give more details on how representative the MODIS data are at millennial scale.

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.
REPLY: *Ok, we wish to highlight the shape of much longer basins than they are wide. We re-worded to "preferential east-west lengthened shapes".*

Line 11 // . . . changes in controlling factors. . .: Factors controlling what? Be more explicit!
REPLY: *Ok, we re-phrased to "factors controlling erosion".*

Line 13 // . . . gently decrease. . .: Gently? Maybe "slightly"; Sharply? Maybe "markedly"
REPLY: *Ok, changed accordingly.*

Line 16 // . . . total altitude differences . . .: Is this basin relief? Then term it basin relief.
REPLY: *No, we calculated basin relief based on quartiles (upper and lower) of altitudes. We consider the absolute altitude range of basins to be misleading as they only give the information that increasing basins will have an increased "absolute relief" when considering rivers of the same drainage network.*
We changed "altitude differences" to "altitude range" throughout the manuscript.

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?
REPLY: *We expanded the explanation of what it means to exclude glaciated areas in the materials and methods chapter (see also reply to comment page 13, line 5).*
We use the term "permanent snow and ice cover" for glaciated areas to stay consistent with the source data we used for calculations compared to evidences of glaciated areas in the past. MODIS is able to detect both, but not to differentiate between ice and snow.

Additionally, most of the glacial deposits remain on the extremely low eroding plateau and thus have an extremely limited contribution to river sediments. We added phrases in the methods section for clarification of the use of "glaciated areas" in our manuscript and we expanded the results section to give more details on the effect of excluding glaciated areas on production rates and resulting erosion rates.

Line 17 // . . . but. . .: except
REPLY: *Ok, we assumed this referred to line 23 and changed "but" accordingly to "except".*

PAGE 18 – BASIN-WIDE EROSION RATES

Line 17 // The basins do not reveal anything; your data, i.e. the denudation rates do perhaps.
REPLY: *Ok, thanks. Changed accordingly.*

Line 26 // Please see previous comment
REPLY: *Ok. Changed accordingly.*

PAGE 20

Line 7 // . . . slope scaled the. . .: Word missing?
REPLY: *Yes. We inserted "to".*

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.
REPLY: *Ok, we re-phrased the text: "... analyses is standard means of a ..." .*

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.
REPLY: *Ok, we re-phrased the sentence for clarification. We do not only refer to relief as a measure of the altitude range within a certain area, but wish to include also slope gradient to highlight the scale dependence of such measures. We changed "differences" to "variations".*

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 RËĘ2 into consideration). When looking at RËĘ2 we are looking at the portion of the variance of the response variable (y) that is explained by the model.
REPLY: *Ok, we re-phrased the sentence and hope we could clarify what was not clear.*

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.
REPLY: *We re-phrased the text and hope we met the point raise by the reviewer.*

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.
REPLY: *Ok, so we re-worded to relative terms: relatively lower and higher.*

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.
REPLY: *Ok, we inserted the information that we used an additive model and that we applied it to test for additive effects of the linear relations found in the first place. The data set is small for more sophisticated regression models and the additive linear regression model explains already more than 90 % of the variance in erosion rates.*

Line 7 // . . . Including more components result in. . . : resulted
REPLY: *Ok, changed accordingly.*

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.
REPLY: *Ok, changed accordingly.*

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 shortterm environmental fluctuation decreases with increasing nuclide concentration, i.e. decreasing denudation rate resp. increasing averaging timescale.
REPLY: *Thanks! We implemented the comment.*

Line 15 // . . ., which is not indicated along the Panj. . . .: Not clear. What do you refer to here? Which are said indications?
REPLY: *Ok, we added a few words for clarification.*

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.
REPLY: *Ok, we inserted "angles" and also added the numbers of so called steep slopes.*

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.
REPLY: *Ok, we re-phrased the sentence in combination with the sentence before.*

PAGE 23

Line 3 // . . . Alichur Dome based. . .: Word missing?
REPLY: *We do not find where a word could be missing in: "...They estimated erosion rates of ... and ... between the Shakhdara and Alichur Dome based on geometry..."*

Line 8 ff // The erosion rates of < 0.5mmyr?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 ∼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.
REPLY: *Ok, we re-phrased the text section and refer now explicitly to the difference between plateau und margins without referring to the Pamir average.*

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.
REPLY: *Ok, we expanded relevant sections in the manuscript to give more details on the effects of excluding glaciated areas from erosion rate calculation.*

PAGE 24

Line 15 // . . . OSL-based incision rates. . .: Be more explicit here. Perhaps “reported OSL-based incision rates from the XYZ region”
REPLY: *Ok, changed accordingly.*

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

REPLY: *Ok, we re-phrased to clarify that we refer to changes of conditions over time such as cyclic glaciations.*

Line 18 // . . . The discrepancy between rates implies . . .: Which rates? Help the reader to follow your train of thoughts.
REPLY: *Ok, we inserted the information.*

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.
REPLY: *We argue that this is a rather general consequence that does not preclude many further implications. The rate of incision defines how fast the base level of a basin is lowered. When basin-wide erosion is lower than this base level lowering, then this means that hillslope processes cannot catch-up with this speed.*

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.
REPLY: *Ok, we follow the suggestion of the reviewer and inserted "out of the predictors that we looked at".*

Page 27

Line 4 // The millennial, . . .: Aren't > 50% of your rates below 1 kyr?
REPLY: *Ok, deleted. Correspondingly, we changed the title.*

Line 13 // . . . variations. . .: Isn't it rather the variance?
REPLY: *Ok, thanks, changed accordingly.*

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.
REPLY: *Ok, we edited the figures for better readability and shortened the sample IDs as suggested.*

Fig. 2 // Labelling too small
REPLY: *Ok, we enlarged the labelling.*

Fig. 3 // Shift to Supplementary Material (together with the largest parts of the Material and Methods section)
REPLY: *Ok, we shifted the figure to the new supplementary marterial.*

Fig. 4 // No need to be shown in the main text. Consider shifting to Supplementary Material
REPLY: *Ok, we shifted the figure to the new supplementary marterial.*