Author reply to comments on esurf-2014-29: "High natural erosion rates are the backdrop for enhanced anthropogenic soil erosion in the Middle Hills of Nepal"

We thank both reviewers for their thorough and constructive reviews of our manuscript. As a preface, we apologize for the long delay in the submission of this reply, which resulted from the substantive and time-consuming work we have undertaken to address the comments. We have now revised the manuscript taking into account the suggestions made in the reviews, and we feel that the revisions represent a significant improvement. Below, we provide a detailed point-by-point reply to each of the comments raised in the reviews, and we highlight how we have modified the manuscript to take these into account.

The original reviewer comments are in plain text**, our replies are in bold text, and related changes to our manuscript are in *bold italics*.**

Anonymous Reviewer #1

This paper by West et al. aims to quantify the impact of agricultural activities on erosion rates in the Nepal Middle Hills. The paper provides new data on natural and modern erosion rates for mountainous environments, and adds new and interesting information to the debate on the impact of humans on sediment fluxes.

We thank the reviewer for this positive perspective on our paper, and we appreciate the range of insightful suggestions in the review that follows. We reply to the specific comments in detail below and have made associated changes in our revised manuscript.

Overall Comments

p. 939, l.25 and following. As land use is one of the factors that affect soil erosion rates in the Nepal Middle Hills, it would be good to have more details on the various land use types that might affect soil erosion processes. In particular, agricultural terraces are known to reduce erosion rates when they are well maintained. Can you give more details on the type of terraces, their maintenance, and the land use characteristics?

We thank the reviewer for pointing out the lack of relevant details about land use characteristics. We have significantly expanded the discussion about land use in the Likhu region in our revision of Section 2 (the "Study Site" section), including details about bari and khet terraces:

"Natural land cover in the Likhu catchment is predominantly sub-tropical and tropical hardwoods at lower elevations and mixed broadleaf forest at higher elevations, with some mixed pine forest of Pinus roxbourgii (Gardner and Gerrard, 2002; Shrestha et al., 2006). Agricultural practices in the Nepal Middle Hills include rain-fed bari terraces, which have sloping surfaces and are used primarily for maize production, and irrigated khet terraces which are flooded and used primarily for rice production. Bari terraces in the Likhu are gently sloping (~5°,

usually sloping outwards and/or sideways), and about 2-10m wide, with typical riser heights up to a few meters (see details in Gardner and Gerrard, 2003). Riser angles are typically >60°, though angles on slumped risers are lower. Crops on bari terraces depend on monsoon rainfall and are usually cycled between maize (Zea mays L.), sometimes interplanted with legumes (grown May-August), and millet (Eleusine coracana L., grown August-December). Khet terraces are frequently built with bunds and used to grow irrigated paddy rice (Oriza sativa L.), sometimes followed by wheat (Triticum aestivum L.) or other winter crops (Shrestha et al., 2006). Wooden plows and hand hoes are used on both bari and khet lands; fertilizers include both farmyard manure and chemical fertilizer. Some of the forested lands are "degraded" due to grazing and partial deforestation, particularly harvesting of Sal (Shorea robusta) wood, and some of the land formerly in cultivation has been abandoned and is now degraded shrubland. Other forested lands, particularly on steep slopes, have remained relatively pristine (Gardner and Gerrard, 2002; Shrestha et al., 2006)."

p. 940, l.11-15: The authors compare 10Be-derived denudation rates with other erosion measures that were derived from previous studies. To compare these erosion measures directly, there are several methodological hurdles: 1) Spatial scale, as it is known that erosion rates measured at plot scale are not directly comparable with catchment-wide erosion rates (See literature on Catchment Area – Erosion rate relationships); 2) Time interval, as it is also known that erosion rates that integrate over very short time periods lack exceptional events, and often underestimate the average erosion rates, and 3) Methodological constraints, as the sediment rating curves of gauging stations only track suspended sediment load while the 10Be-derived denudation rates include dissolved, and particulate load (transported as suspended and bed load). While the time issue is discussed in the text, the other two issues need more attention.

We found this to be a particularly helpful comment that has stimulated us to try to more clearly present our discussion of these methodological issues. In response to this comment as well as input from Reviewer #2, we have substantially restructured and expanded our Discussion in Section 5. The new Section 5.2 now includes a specific sub-section (Section 5.2.2) discussing the issue of dissolved and bedload contributions to the total denudation rate:

"5.2.2. Dissolved and bedload contributions to total denudation: Suspended sediment flux measurements do not include either dissolved mass losses or particulate transport in bedload, both of which are part of the total denudation flux captured by $^{10}Be_{qtz}$. In the Bore and Chinnya, dissolved fluxes are low relative to the total denudation (dissolved fluxes are less than ~10% of the total denudation flux; West et al., 2002), so this is not expected to be a major factor in our analysis. Bedload transport may comprise a significant portion of the total denudation flux in mountain systems. Measurements in the High Himalaya of the Marsyandi River basin suggest bedload may account for as much as ~35% of the total denudation flux (Pratt-Sitaula et al., 2007), such that total annual to decadal

erosion rates may be ~50% higher than suspended sediment fluxes. If the sediment fluxes measured in the Bore underestimate total present-day erosion by approximately this magnitude, actual total present-day rates may be slightly higher than long-term rates (as in Fig. 4a), but the differences would still be lower than the ~10x increases in present-day erosion over the long-term observed elsewhere. Moreover, it is unclear whether erosion in the Nepal Middle Hills is in fact characterized by as much as 35% bedload transport. Bedload transport may be less important in other mountainous environments (Lane and Borland, 1951), and Hewawasam et al. (2003) observed similarity in total sediment delivery to reservoirs (which includes bedload material) and suspended sediment fluxes in the tropical highlands of Sri Lanka, suggesting relatively low bedload transport there."

We also include a paragraph in Section 5.2.1 about the effect of spatial scale, as highlighted by the reviewer: ***"Land use is not the only factor that may differ between erosion rates derived from $^{10}Be_{qtz}$ and those determined from other measurements such as sediment fluxes. Erosion rates often vary as a function of spatial scale (Covault et al., 2013; de Vente et al., 2007; Milliman and Syvitski, 1992; Saunders and Young, 1983; Bierman and Nichols, 2004), potentially complicating comparison of plot-level soil losses with catchment-wide denudation rates. The $^{10}Be_{qtz}$-based denudation rates and river suspended sediment fluxes for the Bore are at comparable scale, but the results from terrace plots are not."***

p. 941 : 10Be production rates. The 10Be production rates were here calculated based on the mean altitude of the basins. 10Be production rates are highly elevation-dependent. In this steep mountainous environment, it would be more thoughtful to use a pixel-based approach to calculate production rates (or to use the median elevation instead of the mean elevation for calculations). What about topographic shielding, and the correction for lower production rates because of topographic shielding?

We have now calculated production rates using a pixel-by-pixel approach, including calculation of topographic shielding at each pixel. We use the newly calculated production rates in the revised manuscript, along with updated erosion rates. Interestingly, the updated values are not significantly different from those we reported previously; although our analysis suggests there is a small topographic shielding effect (of 3-6%) in these catchments, the resulting decrease in production rate is largely offset by slightly higher catchment-average production rates calculated using the pixel approach. The text of the methods section has been revised to describe the approach we now use.

When comparing 10Be denudation rates with present-day erosion rates, the authors often use the mean values of the present-day erosion rates (e.g. Figure 5, and text). In areas with strong anthropogenic pressure, it is known that erosion rates are often highly skewed (few measures with very high erosion rates). Why not comparing the median values of the erosion rates, to correct for this bias?

The reviewer raises a good question. In most cases in our original submission, we were using median values when reporting averages of present-day erosion rates. We apologize that this was not clear from our text and have worked to clarify this point throughout the manuscript in revision. One exception was for the bari plot soil losses from Gardner and Gerrard (2003), reported in Section 4.2. We now use the median value but note that this is not significantly different from the mean value we used previously (mean and 1 standard deviation of 560±340 t/km2/yr, versus median and 68% confidence interval of 460+469/-138 t/km2/yr).

Specific Comments

p. 938, l. 5-10: The authors state that the human impact on erosion rates can also be quantified by comparing erosion rates before/after agricultural activities. In many environments, such comparison will not hold valid data on the impact of agricultural activities on soil erosion rates. Soil properties change as a result of agricultural land use, and decades of agricultural land use in mountainous environments often lead to a decrease in soil depth, increase in stoniness, and stone or rock pavement. As such, various studies have shown that soil erosion rates are often lower after a long phase of agricultural land use, because of rock pavements.

We thank the reviewer for raising this important point; this sentence in our Introduction was not worded clearly. To avoid confusion, the revised text has been rewritten as: *"One additional source of information may come from comparing sub-annual to decadal sediment fluxes with erosion rates integrated over longer periods of time (e.g., Hewewasam et al., 2003; Vanacker et al., 2007; Vanacker et al., 2014). … Insight into catchment-scale effects of land use may then emerge by comparing $^{10}Be_{qtz}$-derived erosion rates with sediment fluxes measured over more recent agricultural times (e.g., 1-10+ years). Specifically, present-day sediment fluxes may be increased relative to the long-term erosion rates, or they may be reduced, if historical agriculture has led to pervasive depletion of fine soil material."*

p. 941, l. 5-7: Can you give the blank values of the 10Be analyses?

We now report the Be-10 blank at the end of Section 3.2: *"Blanks run together with the samples had $^{10}Be/^{9}Be$ values of 1.447 x 10^{-15} and 2.624 x 10^{-15}, compared to samples ranging from 1.476 x 10^{-14} to 5.286 x 10^{-14}."*

p. 942, l. 10-16: Erosion/Denudation rates are now given in two different units (mm/yr and t/km2/yr). Can you give all the erosion/denudation rates in the same units (e.g. t/km2/yr)? The only exception could be Table 2 where you give the 10Be denudation rates in mm/yr and t/km2/yr.

We thank the reviewer for highlighting this issue. Reviewer #2 raised a similar point. We originally used distinct units because the cosmogenic denudation rates

measure a linear denudation rate, while sediment fluxes determine mass loss. However, we appreciate that use of two sets of units was confusing in a paper aimed at making a direct comparison between the two sets of rates! Throughout the revised text, we now discuss erosion and denudation data in t/km2/yr, except in Table 2 where we initially report the cosmogenic rates. The figures now include multiple axes with both sets of units, so that readers who are less familiar with the use of t/km2/yr are able to more readily interpret our data from the figures.

p. 947, l. 3-9: Soil erosion rates on agricultural terraces are reported to be low. This is somehow expected, as one of the principal aims of these terraces is to form flat surfaces by tillage erosion as to reduce soil erosion in the agricultural plots.

The reviewer is highlighting exactly one of the points we were trying to convey in our manuscript. We have reworded the opening paragraph in what is now Section 5.2.1 to emphasize this message: *"The long-term denudation rates are also indistinguishable from the soil loss rates from plots on well-maintained bari terraces, but in contrast soil loss rates from degraded lands are higher (Table 4). Terracing is intended to reduce soil erosion (cf. Gardner and Gerrard, 2003; Smadja, 1992; Tiwari et al., 2009), so a lower rate of sediment losses from terraces compared to long-term denudation rates is expected. Rapid soil loss from degraded lands has also been widely observed (e.g., Gill, 1991; Gardner and Jenkins, 1995; Merz, 2004); the comparison presented here confirms that this anthropogenically-associated loss exceeds the rate of long-term background erosion in the Likhu."*

Figure 3: In this figure, the authors compare their results on the Middle Hills in Nepal with previous studies in Sri Lanka by Hewawasam et al. (2004) and Ecuadorian Andes by Vanacker et al. (2007). The comparison is very interesting, but it would be good to make a differentiation according to the type of land use or vegetation cover. The study in the Andes showed that the human impact on erosion rates highly depends on the vegetation cover in the catchments, which was confirmed in a recent study on the Spanish Cordillera (Vanacker et al., 2014). For well-vegetated catchments (under agricultural land use), no clear acceleration of erosion was observed.

We thank the reviewer for pointing out the 2014 study by Vanacker et al. We have now added the data from the Spanish Cordillera to the cross-plot of short- versus long-term rates (now Figure 4). We have also added a second panel to this figure that explores the role of changes in vegetation cover on the observed difference between sediment fluxes and background erosion rates, expanding on the framework of Vanacker et al. (2014). We discuss this comparison in the revised Section 5.2.1: *"Vanacker et al. (2014) observed little difference between short- and long-term erosion rates in agricultural catchments of the Baetic Cordillera of Spain and suggested that the difference between present day vegetation cover (V) and natural vegetation cover (V_{ref}) exerts a primary control on the extent of agricultural enhancement of erosion. Compared to the data from Spain and*

Ecuador, the data from the Likhu Khola points to relatively lower agricultural enhancement of erosion for a given change in vegetation cover (Fig. 4b).”
Anonymous Referee #2

The manuscript tackles a very interesting subject with high societal significance. … In particular interesting I find the authors effort to evaluate grain size specific 10Be concentrations of their samples.

We thank the reviewer for noting the relevance of our work and for providing extensive, detailed comments on our manuscript. We found many of these suggestions helpful in significantly improving our paper during revision. In other cases, we disagree with this reviewer and explain our rationale in detail below.

Overall, I find the basic concept of the manuscript appealing but considering the little new data and the large amount of literature data I am hesitating to call this contribution a review article rather than a research article.

We are perplexed by this criticism. We admittedly provide a limited number of new Be-10 analyses. We do not pretend that this paper is a comprehensive attempt to assemble a large new cosmogenic dataset for the Himalaya. We note in particular that the primary guideline for a research article in *Earth Surface Dynamics* is the reporting of “substantial and original scientific results,” and we emphasize that the number of new measurements is not, on its own, a good benchmark for scientific substance or originality. Our new cosmogenic measurements are both internally consistent and similar to results from previous studies in the same region, so we do not need additional new cosmogenic data for our analysis, beyond those we report. By combining a few new measurements with a synthesis of previous data from multiple sources, we are able to gain new insight into an important scientific problem. In this regard, we are encouraged to note that Reviewer #1 remarked that we provide “new and interesting information.”

Maybe the authors can revise their manuscript to better highlight the new findings of this manuscript. E.g. figure 5 to my knowledge seems to not provide any new information, it is very well known that degraded land (forest and scrubs) in the Middle Hills of Nepal and also in other mountain regions have a much more negative erosion balance than native or proper cultivated lands.

We apologize that our original submission came across as not adequately acknowledging previous findings about rates of soil loss from degraded lands. We thought that the extent of prior work on this subject was clear from our manuscript text and from the citations to the extensive previous research on this subject. We agree that the observation of greater erosion from degraded lands is not inherently new, nor is it counterintuitive. Our study confirms this established idea in the case of Nepal, using a new technique, and we present a new quantitative comparison of rates. We thus disagree with any implied notion that we are not presenting “new findings.” Our study is the first (as far as we know,

and the reviewer points to no other studies suggesting otherwise) to explicitly evaluate agricultural erosion rates under different land use types in the Nepal Middle Hills in the context of long-term cosmogenic denudation rates (this is not to say that cosmogenic rates have not been reported from Nepal previously, which they clearly have, but not in this context). We are adopting a similar approach to that used elsewhere to shed light on agricultural erosion, as we discuss in our paper. We view the addition of new analysis from the Himalaya as a novel contribution and are encouraged that Reviewer #1 appears to agree.

Nonetheless, in response to the reviewer's comment, we have worked to very significantly revise the text of the Introduction to more explicitly highlight previous work, including the statement that, *"high rates of soil loss have been observed in association with degraded lands in the Middle Hills region (e.g., Gill, 1991; Gardner and Jenkins, 1995; Merz, 2004)."* We hope that our revision makes it more clear that we acknowledge there is already a *"rich literature on soil erosion (e.g., Gardner and Jenkins, 1995; Shrestha et al., 1997; Blaikie and Sadeque, 2000; Merz, 2004; amongst many others)"* as well as *"no less exhaustive efforts investigating erosion and denudation rates across large-scale climatic and tectonic gradients in the central Himalaya (e.g., Wobus et al., 2005; Gabet et al., 2008; Lupker et al., 2012; Andermann et al., 2012; Godard et al., 2014; amongst others; see Fig. 1)."* We also make it clear that our novel contribution lies in *"detailed comparison of erosion rates over relatively short and long time scales in small, well-studied agricultural catchment systems [which has] thus far not been widely explored in this region."*

In addition, we have added a new figure (Fig. 1) that displays existing erosion data from around Nepal and helps to highlight the role of our contribution. We hope that the reviewer agrees that the rewording of the Introduction and the reference to this new figure better highlights the novel aspects of our study and does a better job of placing this contribution in the context of previous work. Similarly, in revising our Abstract, Discussion and Conclusions, we have tried to make it clear that we are contributing one piece to a puzzle that has already seen tremendous previous research effort. Though we disagree that our work is not novel and relevant, we thank the reviewer for stimulating us to better clarify its context.

To enrich the findings and to make them more significant, it would be interesting to report on the total area affected by farming, not only in the Likhu Khola catchment but also in the wider area of the Middle Hills. In particular it would be very interesting to document how land use and its subdivisions have changed between 1992 (time of the suspended sediment and erosion plot analysis) and 2002 (time of the 10Be sampling respectively) and how much these changes could modify overall erosion budgets. Land use classifications from multispectral imagery are straightforward and standard tools are included in all common GIS engines.

We thank the reviewer for this constructive suggestion. Although we agree that a systematic analysis of land use change in the Middle Hills of Nepal would be a very interesting avenue for investigation, we view such a nation-wide analysis as lying beyond the scope of the present study. We hope that our paper may stimulate follow-up studies of land use change across Nepal, such as that suggested by the reviewer.

The authors should also check the work on land use and erosion in the Middle Hills carried out within the PARDYP project (http://pardyp.icimod.org/) managed by ICIMOD around the year 2000. The data is summarised in the PhD thesis of Juerg Merz http://lib.icimod.org/record/7484 and publications referenced therein.

We agree with the reviewer that we could have done a better job in highlighting the relevance of prior work on soil erosion in Nepal, including the results of the PARDYP project in the Jhiku Khola. In our substantial revision of the Introduction, we have put greater emphasis on the importance of this literature: *"In this study, we focus on the Middle Hills of the Nepal Himalaya, where there is a rich literature on soil erosion (e.g., Gardner and Jenkins, 1995; Shrestha et al., 1997; Blaikie and Sadeque, 2000; Merz, 2004; amongst many others), and where the concept of dwindling Himalayan soil resources and the associated "Theory of Himalayan Environmental Degradation" (Eckholm, 1975; Ives and Messerli, 1989) have been widely discussed (e.g., Asia Development Bank and ICIMOD, 2006; Sitaula et al., 2005)."* We have also added further citations to Merz's thesis and other relevant papers elsewhere in the Introduction.

With respect to land use changes, it is necessary to discuss when farming actually has started in this area of Nepal, especially with respect of the integration period of the 10Be analysis. A natural question would be, could this time span already impact the long term erosion evaluation by cosmogenic nuclides and how? E.g. I could imagine one scenario that terracing and urbanisation activities have lead to a quick loss of the upper soil horizon and thus, of the long exposed and 10Be enriched soils, leaving behind less well exposed and lower concentrated soils. These remaining low concentrated sediments could falsify the "natural" background erosion signal towards unnatural higher erosion rates, although it is clearly an anthropogenic signal.

The reviewer raises an interesting question. We discussed this issue (whether agricultural activity should affect cosmogenically-derived rates) in our manuscript briefly in Section 5.3 and at length in the Supplement (Section S3). We also note that this problem has been considered by previous work, which we cite. We remarked (p. 949, lines 15-19) that "The uncertainty introduced because of anthropogenic reworking of soils due to agricultural activity (e.g., von Blanckenburg et al., 2004; Brown et al., 1998) depends significantly on (i) the depth of agricultural reworking, and (ii) the depth of natural background soil mixing (see Supplement Section S3)."

Since it may not have been clear to the reviewer that this text in our original submission was addressing the question of whether loss of soil from land use could bias inferred long-term rates, in our revision we have reworded these sentences: *"Anthropogenic reworking of soils due to agricultural activity may influence long-term erosion rates calculated from cosmogenic nuclide concentrations. For example, if land use has led to the loss of upper soil horizons, removing the ^{10}Be-enriched surface material, the lower ^{10}Be soils supplying sediment to the streams today may yield a higher inferred erosion rate than is characteristic of the "natural background." The uncertainty introduced as a result of such land use effects (e.g., von Blanckenburg et al., 2004; Brown et al., 1998) depends significantly on (i) the depth of agricultural reworking, and (ii) the depth of natural background soil mixing (see Appendix section A3)."*

Given the importance of this analysis for interpretation of our results, we have also now moved what was originally part of the Supplementary information into an Appendix that is now part of the main manuscript.

We hope that our revised wording more clearly conveys our attention to the question raised by the reviewer. Importantly, for the Bore and Chinnya, we find that the calculated long-term erosion rates would be relatively insensitive to likely depths of soil loss. Moreover, we note that the rates we observe in the Bore and Chinnya are similar to rates from elsewhere in the Middle Hills, across a wide range in land use. If anthropogenic erosion were affecting the cosmogenic rates, we would expect to see noticeable differences between these sites, as we state in the text of Section 5.3.

On the methodological side, I am somewhat confused by the soil production determination. The values in the discussion section (p 952, l 1-8) arrive totally out of the blue. The method and assumptions to estimate soil production should have at least been presented in the method section.

We view our calculation of catchment-wide rates of soil production as an interesting exercise, and one therefore worth including in the manuscript. However, we emphasize that this is not critical to our main conclusions, so it could be removed. Our preference is to keep this analysis in the manuscript, while clearly acknowledging its limitations. We hope that our significant revision of Section 5.5 accomplishes this goal. We have not copied the full section of revised text here for brevity.

In revision, we have significantly expanded discussion of our conceptual approach and method of calculation, and we more explicitly set out our assumptions. We have now included an equation and outlined explicitly how we derive the value for each term, and thence how we calculate soil production rates. Since this calculation is part of our interpretation and analysis, we view this description as being appropriate for Section 5.3 and do not think it needs repetition in the Methods section of the paper.

The authors discuss (p 951, l 27 onwards ". . . the difference between the long-term denudation rate (. . .) and the denudation rate driven by mass wasting should be the average rate of soil production . . .") a vague assumption for soil production. First, I am not sure there is only two mechanisms of erosion in the middle Hills. What about weathering, sheet erosion, road construction, etc.?

We do make assumptions in our calculation, including that the denudation system is at an erosional steady state at the catchment scale. We apologize that these assumptions were not more clearly discussed in the original submission, instead being stated only implicitly in this part of the Discussion, and we have reworded the text with this in mind, i.e., to be more clear about the assumptions.

In particular, we have clarified in this section that we are not considering mass losses in dissolved form (the weathering contribution to denudation) because these are a small part of the total budget for the Likhu catchments (as we discuss in Section 5.1). We have now also made it clear that erosional processes such as sheet erosion are mechanisms for transport of soil, so they are effectively part of the contribution of sediment from soil production as defined in our conceptual framework (and as is consistent with previous work). Finally, road construction is not part of the long-term steady-state budget, so we are confused as to why the reviewer has referred to such a process in this context.

Secondly, it is not really clear to me where the mass wasting evaluation is coming from. I think to make such assumptions the authors need to make a much more thoughtful evaluation, including more observations, in order to base parts of their conclusions on these results. In particular I am bothered by assuming ". . . background natural fluxes from mass wasting . . ." (p. 952, l. 2-3), since mass wasting, also in the Middle Hills, is a stochastic process making year to year comparison very difficult and thus need to be averaged over much longer time spans to derive a real background value. Lupker et al. (2012, EPSL), report for the whole Narayani Catchment (of which Likhu is part of) changes in 10Be erosion rates from one year to the other by a factor two and attribute this to localised and catastrophic sourcing by mass wasting.

We have expanded our previously brief discussion of the methods used in determining the mass wasting fluxes, as part of a significantly expanded part of Section 3.1 in the Methods portion of the manuscript: *"3.1. Short-term erosion rates:* *Data on short-term (annual to decadal) erosion rates were assembled from previously published measurements of soil loss from plots (Gardner and Gerrard, 2001, 2003), quantification of sediment transported by streams (Brasington and Richards, 2000), and mapping of the distribution and volumes of landslides, slumps, and debris flows over multiple years (Gerrard and Gardner, 1999, 2000, 2002). The methods used in these previous published studies are summarized briefly here. Plot studies of soil loss were conducted in 1992 and 1993 using existing agricultural ditches at the base of terrace risers to channel runoff and sediment into collection drums. Multiple terrace plots were selected and studied in order to capture a representative range of agricultural practices. Runoff and*

suspended sediment were determined after storms from material in the collection drums (Gardner and Gerrard, 2001, 2003). Stream sediment transport was measured by installing gauging stations with pressure transducers monitoring stream stage height. Sediment concentrations were determined both from depth-integrated sampling at regular (bi-weekly) intervals supplemented by storm event sampling, and by turbidity loggers. Fluxes for 1992 were determined from rating curves (Brasington and Richards, 2000). Mass wasting fluxes were measured during 1991, 1992, and 1993 by identifying, mapping, and measuring all slope failures over this time period in the field (Gerrard and Gardner, 1999, 2000, 2002). Most failures were either slumps or debris slides. Dimensions of failures were mapped in the field and used to calculate volumes lost by mass wasting each year. Connectivity of failures to the stream system was used to determine a sediment delivery ratio to streams of 50% on average over the three years studied (Gerrard and Gardner, 2000)."

We do agree that temporal variation in mass wasting means that it is not possible to demonstrate that these estimated rates are representative of the long-term, and this may influence our calculation of soil production rates in Section 5.4. We acknowledged this implicitly in our original text but have greatly expanded this discussion in the revision, and we hope that we are now very clear that our analysis should be interpreted in this light: ***"There are several reasons that the estimates of F_{MW} may not accurately reflect long-term mass wasting rates, thus biasing our calculation of soil production rates. Present-day mass wasting estimates may be enhanced due to land use (Gerrard and Gardner, 2002), and if actual long-term mass wasting fluxes are lower, actual soil production rates would be higher. On the other hand, the episodic nature of landslides, particularly in a seismically active region (e.g., Keefer, 2004), may mean that the present-day mass wasting volume under-estimates long-term rates by not capturing storm- or earthquake-triggered landslides, in which case soil production rates would be lower than our estimates. More comprehensive mapping of landslides over time could provide the foundation for more robustly determining denudation by mass wasting in the Middle Hills (e.g., Hovius et al., 1997). However, this would require time series of high-resolution imagery, or other means of deriving landslide maps, that are not available in this study. We regard the mass wasting fluxes used here as an initial first-order estimate allowing us to consider the balance of soil production as a proportion of total denudation in the Lhiku. We emphasize that further work, including direct measurement of soil production rates on hillslopes in addition to more work to constrain landslide rates, would help to provide better constraints."***

In fact, we spent a considerable amount of time during the revision process working to develop an improved calculation of mass wasting fluxes using landslide maps from the Middle Hills region (this work was a large part of the reason for the delay in submitting this reply and our associated revisions). Unfortunately, at this stage we have not been able to assemble sufficient time-series of high-resolution imagery to make further calculation feasible within the

scope of this study. We certainly agree that this would be a valuable target for future work and hope that this paper will stimulate such further research.

Although in previous publications (Hewawasam et al., 2003 and Vanacker et al., 2007), suspended sediment fluxes have been taken as true integral values of the total erosion flux. I think a discussion of comparability of sediment fluxes vs 10Be denudation rates is necessary in the Himalayan context. Especially because the timing of sampling of 10Be (2002) and the suspended sediment measurements (~< 1992) used in this manuscript do not overlap. The publications above use a much more complete dataset, including soil pit samples and sedimentation rates in retention basins (bedload, depth integrated concentration profile, etc.).

In response also to a similar suggestion made by Reviewer #1, we have expanded the discussion of the comparison between sediment fluxes and Be-10 derived denudation rates (see reply to Reviewer #1, and the expanded and reorganized Section 5.2 of our manuscript, which for brevity we are not copying here). Since erosion rates derived from cosmogenic nuclides average over thousands of years, the disparity between times of sampling should not confound our analysis. Reviewer #2 is correct that Hewawasam et al. (2003) included limited data (n=3) from sediment infilling of reservoirs, in addition to their suspended sediment yields (they found that the observed ranges overlap). Vanacker et al. (2007) used sediment fluxes based on fill rates behind check dams. We do not have exactly the same types of data, but we have a range of other information that was not available in these other studies, including sediment fluxes, soil loss rates from plots, and estimates of sediment supply from mass wasting. In our study, we are also able to make a comparison with rates observed across Nepal, using a range of different methods, and we find consistency with our results from the Likhu.

Last, I found the back and forth between mm yr-1 and t km-2 yr-1 through the manuscript a little bit confusing. It would be better to single this down to one unity.

Reviewer #1 made a similar comment; we thank both reviewers for letting us know that the use of mixed units was difficult to follow. We apologize for the confusion caused and have worked to correct it throughout the manuscript. We originally used distinct units because cosmogenic denudation rates are a measure of linear denudation rate, while sediment fluxes reflect mass loss. Throughout the revised text, we now discuss erosion and denudation data both in t/km2/yr, except in what was Table 2 (now Table 3) where we initially report the cosmogenic rates, and so we provide both units there.

Further comments on the manuscript:
* For a non native speaker the title seems to contradict the conclusions on p. 954 l. 4-6 (and through the whole text).

We have reworded the title and removed the word "enhanced." The new title is:

"High natural erosion rates are the backdrop for present-day soil erosion in the agricultural Middle Hills of Nepal"

* p. 937, l. 27-28: What about local references on soil plot erosion?

We have added a reference at this point in the manuscript to some of the prior work in the Likhu Khola catchment (Gardner and Gerrard, 2003), as well as a reference to the Merz 2004 thesis, mentioned by the reviewer above, which summarizes similar work in the nearby Jhiku Khola.

* p. 938, l. 5-18: If erosion intensity has changed over time, how does this fit the strong hypothesis to calculate erosion rates from cosmogenic nuclides in river sand samples, that "Erosion in the catchment is constant over the period over which the cosmogenic nuclides average denudation." (Dunai et al., 2010, page 121)?

We have now reworded the Introduction to acknowledge this point, which we had discussed elsewhere in the manuscript at length but which we agree is helpful to state upfront in the Introduction (3rd paragraph of Section 1): ***"Land use can change observed $^{10}Be_{qtz}$ itself, but these effects are often relatively small (see further discussion in Methods Appendix), such that in many cases this method effectively captures "natural background" rates not strongly affected by agricultural activity."***

We refer the reviewer and readers to the extensive discussion we presented in the Supplement, now moved into an Appendix of the main text, which deals explicitly with the question about how changes in erosion rate may affect denudation rates inferred from cosmogenic nuclides. We also note that we are not the first to tackle this problem (see citations in our Appendix).

* p. 940, section 3.1.: This section should be entitled "Short-term, and anthropogenic erosion rates". I generally struggle with the synonymous use of "short term" and "anthropogenic" in this manuscript. This paragraph needs references!

We apologize for the confusion that was apparently caused by this heading. We absolutely agree that "short term" and "anthropogenic" are not synonymous. We have modified the section title to now read simply "Short-term erosion rates." This was the only instance we could find where we used these terms together, but in our revision we have made an effort to distinguish their use as clearly as possible throughout the manuscript. We now repeated the relevant references in this paragraph as requested, and we have significantly expanded this section with additional details, as requested by the reviewer above.

* p. 940, section 3.2.: Why was the Likhu Khola sample not grain size specific analyzed? The fact that there was only little coarse grained material in the Likhu Khola sample might introduce a sampling bias. If the headwaters source a rather wide distribution of granulometries this should be also found in the main trunk.

We did not analyze multiple grain sizes from the Likhu mainstem sample because, as we stated in Section 3.2, "There was little coarse-grained material in the Likhu main stem sample, so only the 0.25–0.71 mm fraction was processed."

There are many reasons that may explain why the Likhu main stem sample did not include more coarse-grained material, including local sorting effects. The sample collection procedure was not intended to capture a representative grain size distribution for any of the sites. A proper grain size assessment of river sediment would require much more detailed methodology than is generally adopted when sampling for cosmogenic nuclides from river sands. The principle of river sediment Be-10 analysis (as described in detail in several of the references we cite) is that fluvial mixing leads to effective averaging. Indeed, we find little offset between grain sizes where we have measured different size fractions, so we doubt that there is a strong grain size bias. Moreover, we note that the Likhu sample is only provided for reference and is not included in the main analysis and interpretation in our paper.

* p. 941, l. 10: Portenga and Bierman (2011) is not a good technical reference here. Granger et al. (2013, GSA Bull.) would be better.

At the reviewer's request, we have removed the Portenga and Bierman reference from the list of citations in this sentence and added the Granger et al., 2013 *GSA Bulletin* paper that provides a discussion of application of Be-10 in geomorphology.

* p.943, section 4.2: I find it a little pretentious to present the data from previous publications in the results section. I propose to have this in a separate section, e.g. entitled: Literature review.

We apologize to the reviewer for the apparent offense caused by including a novel data compilation within a section labeled "Results." We did not in any way intend to imply that these were results from our own field efforts, and we thought that our manuscript was very clear in describing all sources of information and data that have been used.

We have removed "results" from the title of Section 4, so that this is now a section titled simply "Erosion rates." We hope that this change removes any confusion or apparent misappropriation of previously published results as being from our own work.

* p. 943, l. 13: It would be important to include the area size contribution of each land use type for each catchment (maybe in table 3).

We thank the reviewer for pointing out that it would be useful to include this information in tabular format. After considering the best format, we found that

Table 3 was already crowded and have now added a new table (now Table 1) that summarizes key information about each catchment, including contribution of each land use type, as well as catchment areas and mean slopes.

* p. 944, l. 6-10: What is the link between the MCT and anthropogenic erosion?
* p. 944, l. 19-21: The fact that erosion rates vary in the order of two must not necessarily be the reason of tectonic/relief distribution, but can also derive from stochastic sediment input, e.g. Puchol et al. (2014, Geomorph.) or Lupker et al. (2012, EPSL).

These two comments both concern the same paragraph. For clarity and to help avoid confusing readers, we have very significantly rewritten this text and moved it to its own section in the Discussion (new Section 5.1). We do not mention any link between the Main Central Thrust fault and anthropogenic activity. Rather, we present different alternative explanations that are not related but are both possible. We hope that this is clear from the way we have now structured the revision. We have also now included in this discussion the possibility that stochastic sediment inputs may contribute to explaining the observed spatial variability. The new section is as follows:

"5.1 Spatial variability in erosion rates in the Likhu Khola

Rates of long-term erosion inferred from $^{10}Be_{qtz}$ are similar for the Bore and Chinnya, both on the southern slopes of the Likhu valley, across different grain sizes. In contrast, the $^{10}Be_{qtz}$-derived erosion rate for the Likhu mainstem is significantly higher. Mass wasting losses were also found to be higher in the Likhu valley as a whole, compared to the Bore (Gerrard and Gardner, 1999, 2002). Based on 1992-1993 precipitation measurements at rain gauges distributed throughout the Likhu Khola, there is no evidence for systematically greater rainfall on the northern slopes (Gardner and Jenkins, 1995). There are multiple possible other explanations for differences in denudation rate between southern and northern slopes of the Likhu:

(1) Differences in $^{10}Be_{qtz}$-derived erosion rates might result from reworking of cosmogenically shielded alluvial by the Likhu mainstem (cf. Wittman and von Blanckenburg, 2009), or from the stochastic supply of sediment with variable ^{10}Be concentrations to this site, for example due to different tributaries (cf. Lupker et al., 2012) or to mass wasting sources (cf. Niemi et al., 2005; West et al., 2014; Puchol et al., 2014). These explanations would not, on their own, also explain differences in mass wasting rates between northern and southern slopes.

(2) The different rates of mass wasting might be attributable to differences in land use effects, with more land degradation on northern slopes. This idea was suggested by Gerrard and Gardner (1999, 2002) and is evident in the soil erosion modeling analysis of Shrestha et al. (2004). If land degradation is hastening mass wasting on northern slopes, it might contribute to the lower $^{10}Be_{qtz}$ in the Likhu mainstem, explaining both sets of observations.

(3) Alternatively, the observed difference may be related to the presence of the Likhu fault that runs east-west through the middle of the valley (Figure 1). If

this fault accommodates deformation, for example associated with the Main Central Thrust (MCT), then higher tectonically-driven denudation rates might be expected on the northern slopes (cf. Godard et al., 2014; Wobus et al., 2005). This might be expected to increase both the observed mass wasting rates and the inferred long-term denudation rates. The $^{10}Be_{qtz}$ denudation rates in the Bore and Chinnya are similar to other denudation rates in the Middle Hills of Nepal, while the rates for the Likhu Khola as a whole approach the higher rates observed in association with more rapid tectonic uplift in the Nepal High Himalaya (e.g. Godard et al., 2014; Wobus et al., 2005). The northern slopes of the Likhu do not have obviously different relief structure (e.g., river channel steepness), as might be expected for a different uplift and erosion regime (cf. Godard et al., 2014; Scherler et al., 2014). However, non-linearity in such relationships mean that differences in topographic parameters associated with the ~2x difference in denudation rate might not be easy to identify across the Likhu."

* p.944, line 23 onwards: Indeed there is no study so far comparing SSC to CN erosion in the Nepal but a comparison between different publications can be made. Just to give a few possible publication combinations:
* For the Khudi Khola: Gabet et al. (2008, EPSL), Gallo & Lave (2014, Geomorph.), Puchol et al. (2014, Geomorph.), Godard et al. (2012, JGR)
* For larger catchments: Lupker et al. (2012, EPSL), Andermann et al. (2012, EPSL)

We already included a comparison for the Khudi Khola in the text of our original manuscript (Section 5.4). This comparison is complicated by (i) the higher rates of mass wasting in the Khudi Khola, the mixed tectonic position of the catchment (i.e. because the catchment area encompasses both Middle Hills and more rapidly-eroding High Himalaya), and the relatively limited extent of agricultural activity (bearing in mind our main question in this paper concerns how agricultural activity affects erosion rates). Nonetheless, in our revision we have added a new figure (what is now Figure 5) that shows the comparative data from the Khudi. We have also added a new Section 4.4 to note that we make the Khudi Khola and large river basin comparison later in the manuscript: *"<u>4.4 Comparative data from other Himalayan regions:</u> In addition to the variety of data from Middle Hills locations discussed above, datasets from large Himalayan rivers and from the Khudi Khola catchment, which has a predominantly High Himalayan catchment area, allow comparison of sediment fluxes and $^{10}Be_{qtz}$-derived denudation rates more widely across Nepal. These comparisons are discussed in Section 5.3, below."* In the process of discussing the Khudi, we have tried to make it clear that this is not a site of analogously intense agricultural activity to the Likhu, to try to avoid confusion for readers.

We have also added a new paragraph in Section 5.3 that discusses the comparison of the large river data (see below).

* p.945, l. 13: What represents the +-11mm yr-1? It would be more representative to calculate the average erosion rate as area weighted mean. This would also prevent problems of a skewed distribution.

We assume the reviewer is referring to the +/- 0.11 mm/yr (not 11 mm/yr) uncertainty that we quote for the mean value of the cosmogenic-derived denudation rates from previous studies in Nepal. These uncertainties were the standard deviation of the distribution of observed erosion rates.

In our revision, we have reported the median rather than the mean value of the distribution, along with the 68% confidence interval: *"The data from these catchments (n=20, catchment area = 4-110 km^2) yield a median denudation rate of 494+565/-182 $t.km^{-2}.yr^{-1}$,(68% confidence interval)."* As noted by Reviewer #1, reporting a median value may be more representative than the mean when the distribution is non-normal (although in this case, the mean and median values were not distinguishable within uncertainty).

It is not clear to us why an area-weighted average would prevent problems from a skewed distribution, unless differences in area were the main source of skewness in the erosion rate data, which is speculative and seems unlikely to us. In any case, for this dataset the area-weighted median and means are identical to those not weighted by catchment area.

* p. 945, l. 18-19: ". . .and application of the same production scheme may be expected to bring results even closer together." this assumption is highly speculative, please proof.

We have removed this statement since it is not relevant to our analysis, and since the calculated rates are indistinguishable within uncertainty. We have reworded our text so it is still clear that there may be differences in calculated rates due to using different production schemes: *"Comparison of $^{10}Be_{qtz}$-derived rates between different studies should be interpreted in the context that reported rates were not all calculated with the same $^{10}Be_{qtz}$ production scheme."*

* p. 945, l. 26 onwards: The explanation that the high variability of the erosion plot analysis might derive from the background erosion rate is highly speculative and contradictory, especially for terrace farming types.

For clarity, we have removed the statement about background denudation rate from this sentence, so it now reads: *"This variability may partly reflect different land use practices at the different sites that have been studied."*

* p. 946, l. 7: The Siwaliks should have much higher erosion rates than the Middle Hills since a significant portion of the tectonic shortening is accommodated here and rocks are very weak.

We have removed the statement about the Terai potentially having low denudation rates and replaced this sentence with a more general statement:

“Since these different physiographic regimes may be characterized by considerably different background erosion rates (e.g. Wobus et al., 2005; Godard et al., 2012), the sediment yields from the large river basins do not provide a robust comparison to the long-term rates for the Middle Hills.”

* p. 946, l. 10: Although it is difficult to work out the contribution of the Middle Hills the total Himalayan erosion budget, 10Be (Lupker et al. 2012) and SSC (Andermann et al. 2012) erosion rates do compare very well for large areas.

We have added a paragraph at the end of Section 5.3 discussing the comparison of data from large river basins: *“Similarly, the $^{10}Be_{qtz}$ denudation rates and sediment fluxes from larger river basins in Nepal are generally similar in magnitude in most cases (e.g., when comparing $^{10}Be_{qtz}$-derived rates from Lupker et al., 2012 and sediment fluxes from Andermann et al., 2012; see Fig. 1). These basins span across multiple physiographic regimes, which are characterized by distinct erosion rates (with typically higher erosion rates in the High Himalaya relative to the Middle Hills; Wobus et al., 2010; Godard et al., 2014). This heterogeneity means that it is not straightforward to isolate a single variable such as the effect of land use from these data, but the first-order similarity is consistent with the range of other observations including our results from the Likhu.”*

* p. 946, l. 13-15: Considering the very high spread of nearly two magnitudes of erosion rates, I doubt n=8 is a robust set of observations. Consider here also the use of an area weighted average.

We agree that the sample set of 8 rivers with suspended sediment data from the Nepal Middle Hills is small, particularly given the spread in the data. We provide this data for reference, noting that it shows an interesting similarity to the data from the Likhu that is the primary focus of our attention. We mention the full range in the text and provide the percentiles on the distribution when quoting the median value, so readers are able to evaluate the uncertainty on this estimate.

The area-weighted mean for these data is not distinguishable from the unweighted mean value, nor are the medians significantly different.

* p. 946, l. 15-18: Chalise & Khanal (1997) do not report on how many years and how many measurements were included in their calculations. The erosion rates in Andermann et al. (2012) are calculated with a rating curve from long river discharge records.

We have added a statement to the text to note that Chalise and Khanal do not provide the same detail of their methodology as Andermann et al.: *“Important methodological details such as the length of record and the number of measurements were not reported by Chalise and Khanal (1997), so it is difficult to identify reasons for these differences, and the Andermann et al. (2012) dataset*

***does not include enough basins that drain exclusively the Middle Hills to allow an independent estimate."* We have also added a new figure (Figure S1) to the Supplemental file that shows the direct comparison of the two sediment flux datasets.**

* p. 947, l. 24: I find Kirchner et al. not a very good citation here. The integration times of both methods is comparatively very short (not even one interglacial) and can not integrate the "high magnitude, low-frequency" events. In particular the settings of Kirchner et al. are very different to the Himalayas. Furthermore, Andermann et al. (2012) demonstrates that most of the annual sediment flux is related to moderate events and not to peak floods.

Although the setting studied by Kirchner et al. was different from the Likhu Khola, the principle that sediment flux measurements may miss large events with long recurrence intervals is a generally relevant one, and was one of the primary conclusions of the Kirchner et al. study, so we continue to view this as an appropriate reference.

* p. 5.3, section 5.3: The stochastic sourcing of sediments and its impact on the 10Be signal needs to be discussed here, e.g. Lupker et al. (2012) and Puchol et al. (2014).

We have added the references to the Lupker et al. paper and included related discussion in Section 5.3: *"Non-steady state effects may also arise in river systems due to stochastic variability in sourcing of sediment, as suggested for large rivers draining the High Himalaya in central Nepal (Lupker et al., 2012). In particular, variation over time in input of sediment from source areas with distinct elevations and thus distinct ^{10}Be production rates may lead to variable $^{10}Be_{qtz}$. The Bore and Chinnya are both first-order catchments, and these systems are not large enough to expect large changes in sources from different tributaries. Instead, variation in sediment inputs in these catchments is most likely to be associated with mass wasting activity, as discussed above. Variable sourcing might be one explanation for the lower $^{10}Be_{qtz}$ and thus higher inferred erosion rates in the Likhu mainstem, as discussed below."*

In the part of Section 5.3 related to stochastic inputs from mass wasting, we have updated the citation of Puchol's thesis and now refer to the Puchol et al. (2014) paper which was not available online when we originally submitted our manuscript.

Godard et al. (2012, JGR) show very nicely how concentrations of 10Be change from the high Himalayas to the Middle Hills, this spatial aspect should be discussed too.

We have already discussed the spatial variation in Be-10 denudation rates between the High Himalaya and the Middle Hills in a couple places in our manuscript, for example at the end of Section 4.2, where we mention "significantly higher denudation rates in the more rapidly uplifting High Himalaya (Godard et al., 2014; Wobus et al., 2005)." The addition of our new Figure 1

displays this feature as well. It is not clear to us why this is relevant to repeat in Section 5.3, where we are discussing potential non-steady state effects and their influence on the estimates of long-term denudation rates. Spatial variation would not be expected to generate bias in our erosion rate estimates for the Bore and the Chinnya, which are small catchments situated entirely within the Middle Hills, as we make clear in the text.

* p. 951, l. 8-10: This statement has been made several times already through the manuscript.

The sentence questioned by the reviewer states that, "The implications of our results are that most of the sediment carried by the Likhu Khola, and by other similar rivers draining the Nepal Middle Hills, does not come from agricultural land degradation but rather from naturally high rates of landscape denudation." Nowhere prior to this point in the manuscript have we made a direct summary statement about the implications of the results of our study for the source of sediment carried by Himalayan rivers. We agree that this statement reflects a major theme that runs through our paper, arguably our central finding. Since we see this message as an important conclusion of our study, and since it provides a natural segue to the next section of discussion, we prefer not to remove this sentence at this point in the manuscript. However, if the editor feels that this statement is superfluous, we would be willing to reword this section.

* Figure 1: It would improve the figure to include the channel network and the catchment outlines. The zoom in the left panel is too high. A larger subset would help the unfamiliar reader to orient himself better. E.g. a map stretching from the Terai to the Tibetan Plateau.

We appreciate the reviewer's suggestion that a map covering a larger area would be helpful; this comment has stimulated us to add a figure (now Figure 1) that shows the larger Nepal region and allows us to display the sites where data has been collected in other studies and used for comparison in our paper. We also carefully considered the reviewer's suggestion of adding a channel network to what is now Figure 2, but we have decided that such an addition is unnecessary, especially given that the shaded relief and elevation colormap make it quite clear where the drainage network lies. Adding vectors displaying the channels and catchment boundaries would crowd the figure and obfuscate otherwise clear display of the information without significant benefit.

* Figure 2: What happened with the DEM in the left panel? Please use a adequate DEM without artifacts. Stretch color range to the min-max elevation range of this subset. How much do the different land use types (left panel) contribute to the total catchment area? What about co-referencing the map to the digital elevation model?

We thank the reviewer for spotting issues with the display of the DEM for the Bore Khola in what is now Figure 3. We have fixed the problematic display of the

DEM. We have also changed the elevation scale, as suggested. The reviewer has already asked for the land use proportions, which we provide in what is now Table 1.

* Figure 3: It would be interesting here to give a better overview on how sediment fluxes from different settings compare to agricultural dominate catchments. Therefore, more data could be plotted into this graph, e.g. Carretier et al. (2012, Geology), Andermann et al. and Lupker et al, Meyer et al. (2010, Int. J. Earth Sciences). . .

The reviewer suggests an interesting idea. The purpose of the cross-plot of short- versus long-term denudation rates (what is now Figure 4) is not to provide a synoptic, global view of Be-10 denudation rates vs. sediment fluxes. Other researchers (e.g., Couvault et al., 2013, *Journal of Geology*, as cited in our paper) have recently presented such a global-scale analysis and considered its implications. Our attention in this contribution is focused specifically on considering land-use effects, so we have selected data that as best as possible isolate this variable. The impressive work from Meyer et al., 2010 (in central Europe) and Carretier et al., 2012 (in the central Andes), both noted by the reviewer, focus on a range of other factors.

* Table 2: Can you explain how the bi-directional uncertainties of denudation rates have been derived?

The source of the uncertainties is described in the footnote of the table: “errors reflect 16th and 84th percentiles of Monte Carlo distributions (n = 10000).” The Monte Carlo procedure is described in the methods of the main text.

* Table 3: Please include the contribution of the different land use types to the total area.

This information about land use area in each catchment proved difficult to accommodate within this table of results but has now been included in the new Table 1, as discussed above.