Dear Editor,

Please find our responses to comments by referee #3 below in italics.

Thank you,
AP Ballantyne

General comments:

1) The paper aims to address the biogeochemical response of alpine lakes to recent changes in dust deposition. The topic is interesting and within the scope of BG. However, the conclusions achieved are not based on a proper consideration of the processes involved, and the methods, assumptions and description of the calculations are not sufficiently complete and clearly outlined to allow a proper evaluation of the results. There are three major issues in that respect: (i) spatial and temporal variability is not properly evaluated (or sufficiently explained) in the experimental design, this compromises all quantitative issues; (ii) the discussion of nutrient limitation based on sediment C:N ratios and total P in bulk sediment are based on assumptions that are not valid, sediment C:N:P stoichiometry do not reflect the actual stoichiometric constraints of the algae during growth in the water column or on benthic substrates; (iii) the title and conclusions go far beyond the actual content of the study, the two tarn studied are scarcely representative of most common alpine lakes, the authors should indicate and consider that they are dealing with a very particular subset of alpine lakes, those extremely shallow and small located in non-vegetated watersheds. Bellow I develop these issues in detail and suggest possible actions, which require an in-depth revision of the manuscript.

Detailed comments:
2) Title: It goes far beyond the particular case the paper describes. In that respect it is misleading. I suggest being more specific. The paper do not discuss the general response of alpine lakes to dust deposition, it describes two very particular cases in a very particular region of the planet with a lot of circumstantial evidence. There are three aspects that should be considered in particular when planning a new title: 1) geographical location; 2) size and depth of the lakes (the case described covers an extreme case within the alpine lake variability; and 3) soil development.

Title has been revised to read:

Biogeochemical response of two small alpine lakes to recent increase in dust deposition in the Southwestern, US

Note that we had a hard time fitting soil into the title and still having it make sense. However, I think that the new title is much more specific and better describes our study.

3) Introduction: The authors use a wealth of citations related to stoichiometric aspects. However, their case bases on sediment organic matter. There is not a straightforward connection between the literature cited and the present study in that respect. Sediments undergo an important diagenetic process. The characteristics of this process are as relevant as the initial composition of the organic matter accumulating. The whole issue of diagenesis is omitted in this paper from introduction to discussion. I think it is a mistake. Particularly, because diagenesis imply also a feed-back to the underlying water and, more importantly in this case, with the top sediment, which in a lake <1m deep is formed by a biofilm of both algae and heterotrophic microorganisms.

The introduction of this paper has been thoroughly revised to present previous research on nutrient subsidies and atmospheric deposition. We present the concept of nutrient stoichiometry only in the context of relative N:P atmospheric deposition. Diagenesis has also been presented in the discussion as a possible mechanism explaining the patterns that we observe in the sediments as well as potential

changes in climate that may have affected lake level and thus the relative contributions of benthic vs. pelagic productivity.

4) Study sites: Site description is poor. Please provide geographical coordinates. Which is the maximum depth of the lakes? Where the coring site was located within the lake? Are there studies that have evaluated the spatial heterogeneity in sedimentation rates in these lakes? In such a shallow lake differences in a few meters can be large depending on the initial topography of the substrate and how the basin has been filling during the ontogeny of the lake. If snowfall is high, water level oscillations must be relevant, either vertically or horizontally, how can they affect the sedimentation rate in your particular sampling point? Is there temporal or permanent outflow? Are they seepage lakes? How many inflows are they? Do water flow during thawing form channels on the sediment surface? These and other details are vital to evaluate whether changes in sedimentation rates at the coring point can be taken as representative for the whole lake.

Site descriptions has been improved. Geographical coordinates have been added. We specifically selected lakes that showed no sign of sediment disturbance or variable sedimentation rates (e.g. sediment forming deltas). Sedimentation rates are variable within all lakes. However, we tried to control for this by coring in the center of lakes that showed no obvious signs of variable sedimentation rates, such as dominant inflows (Fig. 1). Points corresponding to where the cores were taken have also been added to Figure 1.

5) Sample collection: Bedrock types? What do you mean exactly? In table 1, Sr standard deviation is similar to average, this means bedrock types can highly differ in composition also for other elements. Does this variability correspond to these different bedrock types? Was the relative coverage of each type evaluated to eventually weighing the comparison with sediments and dust accordingly?

We acknowledge that there was heterogeneity in the underlying geology of these watersheds, which is not uncommon for volcanic formations. This is why we sampled the representative bedrock types. Although we do not know the percent coverage of each of these bedrock types, there was no dominant bedrock type and thus the range of geochemical values spans the possible range of geochemical values within the watershed. This has been revised to read:

"Because the volcanic geology was not homogenous, each of the watersheds were sampled for representative bedrock types for geochemical analyses. For both Senator Beck and Porphyry, we collected 4 bedrock samples from throughout the catchment."

The standard deviation of Sr was mistakenly reported as 225.25, the actual standard deviation is 22.25, which is less than 10 % of the mean.

6) Sample collection: Here you mention that outer weathering rind of all bedrock samples was removed before analysis, later you mention that there were no significant differences between weathered and non-weathered parts. Could you clarify this issue? It is quite unusual a complete congruent chemical weathering of the rocks? You should show that this is the case. If not, you should consider discussing potential effects of climate fluctuations on weathering rates and characteristics, perhaps to eventually conclude that they must be irrelevant for some of the issues addressed.

This has been clarified in the revised draft. The outer weathering rind was removed for the analysis of rare earth isotopes of Sr, Nd, and Sm. Unfortunately, this analysis is very expensive and time consuming so we did not analyze the weathering rinds as well. However, we did analyze both the inner rock and the outer weathering rind for elemental analysis. Initially, we did not notice a difference in elemental composition between the inner rock and outer weathering rind, so we decided to combine all the data. Based on these comments we have re-analyzed the data and done t-tests on all elemental concentrations (inner vs. outer) for both watersheds. None of these t-tests yielded significant differences (all p-values > 0.11), so we have decided to use the combined data for our mixing model

and estimates of dust enrichment. Although there were some notable differences between certain elements, such as depleted P in the outer weathering rind relative to the inner rock, our sample size was not sufficient to statistically verify these differences. Therefore our data do not necessarily prove or disprove incongruous weathering in these watersheds, which was not really the objective of this research. This has also been added to the revised text.

7) Sample collection: How many dust deposition events did you finally measured? How spatial heterogeneity in snow accumulation and dust concentration was evaluated? How much can be the error in the final flux estimation? Has been the interannual variability considered in other studies? If the number of dust events is low, interannual variability can be huge.

A total of 30 dust events were documented from 2003 to 2007. Widespread dust events were corroborated by changes in the radiative balance of the snow (Painter et al., 2007). There was considerable variability in the magnitude and frequency of events between years ($\bar{x}$ = 1.3 g m^{-2} ; σ = 0.9 g m^{-2}). These values were then extrapolated over the entire year to estimate annual mass fluxes of dust. Because these watersheds are snow-covered for almost 9 months of the year, extrapolating from the dust on snow events to the total annual flux is probably pretty close. We are currently measuring total wet and dry deposition at this site using passive samplers and the estimates are fairly comparable. This information as well as the reference (Lawrence et al., 2010) have been added to the revised text.

8) Dating: I do not think that sedimentation and accumulation rates based on Pb210 and C14 can be easily compared. You basically address to different time windows, particularly with such a small number of C14 dates. Therefore, any conclusion based on this comparison is weak. I think that at most, you can use C14 dates to estimate an average sedimentation rate for the Holocene as a reference for current sedimentation rates estimated with Pb210. However, you cannot demonstrate recent changes because they do not occur within you PB210 date period. So, in fact, you don't have a proper temporal framework to address that issue.

We agree, we can really only compare Holocene background sedimentation accumulation rates inferred from ^{14}C with contemporary sedimentation accumulation rates inferred from ^{210}Pb. We really have no way of determining exactly when a change in sediment accumulation rate may have occurred. We have already published a paper on the topic of increased dust loading to these watersheds (Neff et al., 2008) and thus the focus of this paper is to really look at the biogeochemical implications of this increased dust loading to alpine ecosystems.

9) Dating: I am also quite concerned that these lakes may dry out episodically. The C14 is too poor for evaluating this point during Holocene. Therefore you should be cautious about this point or provide indirect evidence that this has not been the case. However, concerning the period dated with Pb210, we would be able to evaluate this point if greater detail of the age model is provided. I think that specific plots for Pb210 dating should be included, with the measurements and the age and sedimentation model.

Although episodic drying is a concern for the lakes investigated in this study there is no evidence to suggest a hiatus in sediment accumulation. We have separated the two sediment accumulation figures and added individual ^{210}Pb plots for each lake as insets (Figs. 2 and 3). These plots show no reversals or anomalies in the ^{210}Pb or the ^{14}C dates which might indicate that these lakes had at one time dried out. We cannot say definitively that no drying out has occurred between the ^{210}Pb and the ^{14}C dates but this problem is not unique to our study or these lakes. Furthermore, even if a drying out event had occurred between the ^{210}Pb and the ^{14}C constrained intervals it would not change the central conclusion that dust has always been an important influx to these systems and it has increased over the last 200 years.

10) Biogeochemical analyses: First paragraph in this section should clarify whether the elemental composition correspond to the total sediment or some specific fraction.

In particular, it is unclear which P fraction is considered. This is a key point when comparing with C and N.

The 'Biogeochemical analyses' of the methods section has been clarified and has been split into three sections corresponding to the three fractions of material we investigated. For the elemental analysis, including P delivered by dust, we focused on the inorganic fraction. However, we have clarified in the methods section that only organic carbon and nitrogen are lost during this digestion and that any P originally bound to organics remains in solution because there is no gas phase of P. Therefore the P really represents the total fraction of P. For the analysis of C and N and their stable isotopes we focused on the organic fraction, which should represent most of the C and N present in the sediment. Finally, for the rare earth isotopes we focused only on the mineral fraction.

11) Biogeochemical analyses: What does a subset of samples mean? Please indicate here or in the table how many samples were analyzed and to which levels they correspond in the case of sediments.

The number of samples analyzed and their depths have been indicated in the methods text and in table 2.

12) Certain harmonization in the way to indicate the products used is necessary (e.g. H2O2 and hydrogen peroxide, are both used in this section).

Thanks for pointing this out this has been changed to H_2O_2 for consistency.

13) Statistical and geospatial analyses: The way of constructing the age-depth model appears quite sophisticated in this section, but looking at figure 2, we can see that it ends up in a linear interpolation between C14 dates and linear fitting through the Pb210 dates. The combination produces a shoulder just at the intersection of the two dating procedures. In my opinion this two dating systems cannot be amalgamated in a single age-depth model. Temporal resolution is so different that they address fundamentally different time windows, and therefore variability cannot be compared, since the one with less resolution (C14) produces smoother results (higher averaging).

Combining ^{14}C and ^{210}Pb into a single age model is conventionally done- see reference from referee #1 (Ilyashuk et al., 2011) and many other studies. However, the ages and sedimentation rates interpolated between the ^{14}C and ^{210}Pb intervals are probably not meaningful and we make no attempt to interpret changes in this section of the core. For instance the slightly reduced sedimentation rates during this interpolation interval are an artifact of the age model, but a simple linear age model would also produce slightly reduced sedimentation rates during this interval in order to reconcile the ^{14}C and ^{210}Pb. All age-depth models use some mathematical differencing function to make inferences about the age of sediments, this is essential if one wishes to interpret continuous data in a chronological framework. Our approach is not perfect, but I think that it is probably more realistic than a standard linear interpolation scheme and furthermore It does not signicantly alter our conclusions it simply allows us to place our conclusions in a chronological context.

14) Results: In this section there is some mixing of results and discussion. I will list specific points below, but I think the manuscript requires a general re-writing clearly distinguishing between facts (results) and more or less justified speculation (discussion).

In the current draft we have separated results from discussion.

15) Changes in the flux of dust to alpine catchments: 194 line: Changes in the mass flux to sediments in the recent past: As I mentioned it is not correct to compare sedimentation rates based on Pb210 dating and C14 dating in this case. Coarse temporal resolution averages over longer periods, missing the details of short term fluctuations. From the data reported, some more correct sentence should be: "during the last 150 years sedimentation rates have been higher than the average rate throughout the last 3 millennia".

This text has been changed according to the referee's suggestion. We have also added simple statistics (e.g. t-tests) indicating significantly higher sedimentation rates inferred from ^{210}Pb than those inferred from ^{14}C.

16) 200 line: Increase in density: The increase in density does not occur regularly in Porphyry as we could expect from a compression process in homogenous material. What does it happen around 2200 years BP? Is there any interpolation artifact?

Thanks for pointing this out. This was in fact an artifact and has been corrected in the current revision (Fig. 2)

17) 203-211 lines: The change in accumulation rates coincides with the change in dating method. I do not think you can talk about a change in accumulation rate with this data.

Although we cannot talk about the exact timing of the change in accumulation rate, there has been a clear increase in sedimentation accumulation rates in these systems (Neff et al., 2008). To test this assertion we did simple t-tests on the sedimentation rates inferred from ^{210}Pb and ^{14}C. For Senator Beck there was a significant difference in sedimentation rates inferred from ^{210}Pb and ^{14}C (p-value = 0.04) and for Porphyry there was an even more significant difference in sedimentation rates inferred from ^{210}Pb and ^{14}C (p-value = 0.0009). These statistics have been added to the revised text. Thus there has been a significant increase in sedimentation in both of these lakes and these increased sedimentation rates result in increased sediment accumulation rates, despite a decrease in sediment density up core.

18) 212-213 lines: I agree that sediment isotopic signal is closer to dust. However, I will like to see more details on the variability between samples for each compartment. How many samples did you analyze? Have you statistically tested the differences? Can you discard differential weathering due for instance to dryer climate which could enrich 87Sr/86Sr ratio? Table 1 indicates huge differences in Sr concentration between samples, is there any isotopic pattern related to this differences? Please provide more information and discussion about this issues.

We have updated the table to include sample sizes and we have also added a supplemental figure showing changes with depth in each lake as well as the size fractions. We have done conventional statistical analyses using basic t-tests to show that $^{87}Sr/^{86}Sr$ values of sediments are statistically indistinguishable from dust $^{87}Sr/^{86}Sr$ values and we have included them in our Bayesian mixing model. Much of the variability in the $^{87}Sr/^{86}Sr$ sediment data is attributed to differences between size fractions, whereas most of the difference in $^{87}Sr/^{86}Sr$ of the dust samples is probably due to changes in dust source region (Neff et al., 2008). Lastly, the observed variability in $^{87}Sr/^{86}Sr$ isotopes of the underlying geology are due to bedrock heterogeneity. This variability highlights the problems of using just one isotope or a conventional 2-member mixing model to distinguish between sources. We have added to the discussion the possible explanation of drought and its effect on weathering, although we argue that increased drought would lead to increased dust deposition, which is also enriched in $^{87}Sr/^{86}Sr$.

19) Biogeochemical response to changes in dust loading: 226-241: lines. Causal links are here introduced without supporting evidence. The results section has to be more descriptive of the observed patterns and the interpretation in a dynamical context has to be placed in the discussion section. All in all, concerning nutrient limitation and stoichiometric issues, there is a very weak aspect. The C:N of organic matter in the sediments do not only depends on the conditions where this organic matter was produced but also about the decomposition and diagenetic process that has suffered. In addition, you are mixing data on C and N in organic matter with total P in the sediments (from the methods section I think I have to assume that). The P in the sediments has very different biogeochemical pathways than organic C and N, including potential dissolution, mobility and diffusion to the water column. Therefore, discussion about the limiting nutrient has to go beyond simple comparison of C, N and P ratios.

We have revised the results discussion, such that only patterns and associations between elements are presented. The interpretation of these patterns and associations has been moved to the discussion section.

20) 242-259 lines. The main difference in organic dC13 within a lake is related to the pelagic and benthic environments. This is not considered at all here, and in a so shallow lake production on top of the same sediments should be highly relevant. The high fractionation in benthic systems is largely due to diffusive boundary layer effects. Thinner boundary layers, in more windy conditions for instance, will let to less discrimination. Relative increase in pelagic production will also produce the same result. Therefore, concluding that the changes in dC13 reflect higher productivity is highly speculative, alternative hypothesis have to be considered and discussed. In addition to this production aspects, potential effects of diagenesis should also be considered.

We agree that there are many factors that influence $\Delta^{13}C$ of organic matter in lake sediments and these factors are addressed in the currently revised discussion. We consider all these factors in eliminating possible explanations for the observed patterns in $\Delta^{13}C$.

21) 260-274: I think that elemental composition should go in a different section than isotopic C and N issues.

Results on elemental concentration have been separated into a different paragraph than results on isotopic composition of C and N.

22) 281-282. Please note that the Fe, Cd, P and Sr different behavior in Porphyry and Senator Beck is also reflected in the PCA. These elements change the direction of their loading in the first axes. I suggest commenting first on figure 7 and later about fig. 6.

Thank you for pointing this out. We have added comments on the different patterns observed in Fe, Cd, P, and Sr in the PCAs for our two lakes. These figures and their associated text have also been switched in the text.

23) 286-308. In this paragraph you use updated names for diatom taxonomy, whereas in fig.8 you use traditional names, please harmonize. Fig. 8 reports relative abundance for a few species, whereas in the main text you talk about abundance. Did you evaluate the diatom flux? Is there an increase in total diatom production or it is just an species substitution?

The text has been updated to be consistent with the updated taxonomy in the figure. Unfortunately, we did not have absolute diatom concentrations, just relative abundances. Therefore it is not possible to calculate changes in diatom flux over time and our interpretation of biological response is restricted to relative changes in certain taxa.

24) 4.0 Discussion. 4.1 Changes in dust fluxes and their geochemical composition. 319-339. Comparison with soils is interesting. However, I wonder to what extent the estimation can be conditioned by the limited sediment sampling points (one per lake). Let's assume that the dust contribution is very high anyway in these two shallow tarns. The last statement in the paragraph is really misleading: "alpine lakes are excellent recorders of changes in dust deposition and that there is very little geochemical influence from bedrock or soils in these watersheds". These two tarns are far from being representative of all or even average alpine lakes. Therefore, I think, it we'll be more faithful to state that "small alpine lakes on non-vegetated watersheds and crystalline bedrocks can be excellent recorders of changes in dust deposition".

This has been revised to read:

'Lastly, the relatively high proportion of dust observed in these lakes suggests that alpine lakes in crystalline catchments with very little soil can be excellent recorders of dust deposition and thus very sensitive to changes in the magnitude and chemical composition of dust deposition.'

25) 340-345. Could you explain how do you discard potential Sr isotope fractionation due to differential rock weathering? I think that your conclusions are correct, but I will see some comment about this alternative hypothesis.

We address the potential of Sr isotope fractionation during weathering in the revised text. We also include a figure of $^{87}Sr/^{86}Sr$ isotope variability with depth in both of the lakes (Supplementary Fig. 2). It is possible that the apparent increase in $^{87}Sr/^{86}Sr$ is due to increased weathering; however, if the apparent enrichment were due to increased weathering we would expect for the elements resulting from this weathering- Sr, Nd, and Sm- to also increase in the surface sediments as well, but they don't. They peak at around 5-6 cm, which is consistent with a peak in dust loading at approximately 80 YBP (Neff et al., 2008). Furthermore, our mixing model, which includes other elements, such as Sm and Nd, that are less sensitive to fractionation during weathering, suggests that weathering from bedrock only contributes a small fraction to lake sediments.

26) 347-350. The dust deposition increase is based in one point per lake evaluation. Provided the lakes are extremely shallow I wonder about the spatial heterogeneity in the deposition process and how it depends on the water budget, for instance. Will changes in snow or rainfall modify the sediment focusing patterns?

This is always a concern when coring a lake of any size. So we followed typical protocols for coring lakes. We cored the deepest part of these lakes and avoided any obvious inflows or outflows. Although these lakes were very shallow, this actually allowed us to see the sediment water interface and avoid areas where sediments were possibly disturbed.

27) 4.2. Implications for biochemistry. 352-375. I think it is totally inappropriate to discuss nutrient limitation based on C:N ratio in sediment organic matter and P from bulk sediment. First, C:N depends not only on autochthonous organic matter but also allochthonous inputs, which in this case they may com from far away with dust. On the other hand, the C:N ratio is further modified in the decomposition process, increasing with time. In addition to this objections, considering stoichiometry of P based on total P of bulk sediments is absolutely inappropriate. We do not know how much of this P has been really bioavailable, and in addition it may suffer a complex cycling depending on the redox conditions and elemental composition. All this stoichiometric discussion should be removed from the paper. And potential dust effect discussed in a more biogeochemical process based way.

The revised discussion focuses much more on the processes affecting the elemental and isotopic composition of these sediments and less on the concept of elemental stoichiometry. However, we do discuss our results in the context of recent work done by Elser et al. (2009) in the mountains of southern Colorado. The revised discussion is much more process based and less dependent on the concept of nutrient stoichiometry which may not apply to sediment.

28) 363. As mentioned before, I do not think that d13C changes probe that productivity increases.

We have broadened the current discussion to include other mechanisms that may have influenced $\Delta^{13}C$ through time, such as differing sources (pelagic vs. benthic algae or lacustrine vs. terrestrial), diagenesis, and changes in primary productivity.

29) 396-397. The latter sentence seems not to be completed.

This sentence has been completed.

30) 402-420. This paragraph is terribly speculative. The authors have not evaluated spatial heterogeneity in sedimentation. Sediment focusing is not the only factor, just think on how many processes can affect sediment distribution in a tarn less than 1 m: changes in water level, redistribution of sediments by water flow during melting, wind action, etc.

It would have been nice to evaluate the spatial heterogeneity of sedimentation within each of these lakes; however, we focused our limited dating resources on multiple lakes in order to evaluate the regional atmospheric signal of changes in dust deposition and their biogeochemical implications. Although there are multiple factors that affect sedimentation rate, the fact that we see a similar response in these two lakes and that sediment accumulation rates scale to watershed size, suggests that these alpine watersheds are extremely sensitive to atmospheric deposition. This has been noted in the revised text and relevant citations have been added. The topic of spatial heterogeneity in sedimentation certainly warrants future study, but it was beyond the scope of our study.

31) 409. Authors use throughout the term "Holocene" to refer to their long term record. However, their data only arrive to about 3000 years ago. This is less than 1/3 of the Holocene. I suggest using "the last millennia" instead.

The term Holocene has been replaced by millennia where appropriate.

32) 421. "Knowledge of modern fluxes, weathering fluxes. . ." could you provide more information on how they were evaluated? In addition to the already stated problem of evaluating and upscaling heterogeneity in snow or dust deposition within the watershed, there is the issue of interannual variability in dust fluxes? If the deposition is mainly episodic there can be huge interannual differences, please discuss this point.

We have provided more information on the inter-annual variability of dust loading in the methods and we have considered this in the revised discussion:

'Although there is considerable inter-annual variability in our estimates of dust loading, this variability is primarily due to ENSO variability (Reheis, 2006) and thus our estimates of dust loading spanning 5 years are probably representative of the mean annual dust loading. Moreover our estimates of total annual dust loading are probably fairly conservative as we only measured discrete events in the snowpack (Lawrence et al., 2010). '

33) 438-451. Concerning P budgets, what role can play P diffusion from sediment to water during low oxygen periods (winter ice and snow cover)? What is the role of water flow? Do these sediments freeze during winter? Is there any wash out of sediments during thawing?

This is a valid point which has been considered in the revised draft (see '4.2 *Implications for Biogeochemistry'). **These lakes have fairly high dissolved oxygen concentrations (see Table 1 added) due to their cold temperatures and shallow depths which promote the diffusion of atmospheric oxygen. Although oxygen levels may be reduced during winter months there is no reason to think that these lakes go anoxic during winter months because biological oxygen demand is very reduced during winter months. Because these lakes have high concentrations of oxygen we suspect that P is tightly bound to iron hydroxides in the sediments (Jensen et al., 1992). Although it is likely that there is some internal loading of P to these lakes from the sediments, we do not think that the amount of internal P-loading has changed over time and thus cannot explain the secular trends of increasing accumulation in the sediments P. Furthermore, these lakes are ice covered in the winter and overlaid by 3 to 5 meters of snow, thus they are fairly well insulated from the atmosphere and don't freeze down to the***

sediment water interface. There was no evidence, such as channels across the sediment or scouring of sediment, that would indicate that sediments had been washed out during thaw.

34) 452-462. Estimating N fluxes without considering wet deposition is extremely risky, some estimation based on potential N values for the area should be compared to the dust N flux.

Thank you for this suggestion, we have included measurements of wet N deposition from a nearby site in our analysis. The revised analysis of total N flux is now in better agreement with our estimates of N accumulation rates in sediments.

35) 463-476. Both species Achnanthidium minutissimum and Staurosirella pinnata are among the most widespread species in remote cold lakes throughout the world, it is difficult to assess the underlying cause of their relative increase. If total diatom valves fluxes were evaluated some direct evidence on productivity changes would have been available. Interestingly, in this part of the discussion you consider changes in lake level, could these changes have modified the local distribution (and thus local accumulation rates) of the sediment deposition, without changes in the average atmospheric dust deposition?

Unfortunately, we do not have absolute diatom counts for both sediment cores, so we cannot use this as independent data on primary productivity. We realize that these are very widespread diatoms that comprise most of the relative taxa in these cores; however, they showed the most variability with depth. This is why we rely heavily on a local study investigating the abundance of these species in response to P and Alkalinity (Sgro et al., 2007). We are currently investigating the interactions between dust and climate and it is extremely difficult to deconvolve these two processes because drought tends to lead to increased dust emissions. Therefore past drought events probably resulted in increased dust deposition.

36) 477-480. You mention droughts during 20th century, is there also evidence of them during the last 3 millennia?

There are several well-documented 'mega-droughts' that are evident in tree ring records from the Western US. However, these 'mega-droughts' occurred between 1000-1200 AD, which is a poorly constrained interval in our sediment cores that really does not show any strong excursion in the accumulation of elements (Neff et al., 2008).

37) 481-512. As I repeatedly mentioned I think is completely inappropriate to discuss nutrient limitation and stoichiometric issues of lake primary production based on C:N ratios from the sediments and bulk sediment total P.

Much of the discussion on stiochiometry has been removed and we simply compare our results with microcosm studies by Elser et al. (2009) in similar 'N-limited' alpine watersheds of southern Colorado.

38) Table 1 legend: The following sentence is ambiguous: "There was no significant difference between sediment and bedrock samples for various watersheds in the San Juan Mountains, therefore these samples have been pooled for all analyses". What did you exactly have done? According to the standard deviation of Sr in bedrock samples, differences in concentration are huge. Is there any relationship between concentration and isotopic composition? How many samples did you measured to be sure SD was stable and thus mean value reliable. Sr/Nd ratios are quite different between the three compartments (5.3 sediments, 8.3 dust, 6.2 bedrock), in this feature, bedrock and sediments were more similar. In summary, you need to explain better the number of samples considered in each comparison, why these samples are representative of the whole watershed and then test statistically that the compartments are different.

This ambiguous statement has been removed from the table legend. Essentially, we checked for significant differences within the compartments (sediments, dust, and bedrock) in terms of their elemental composition and their isotopic composition and found no statistical differences. This is not to say that there aren't differences within these compartments, but we did not have enough samples to statistically distinguish these differences (this has been added to text). Therefore elemental and isotopic ratio data were combined for these various compartments and used in our mixing model. The standard deviation of Sr was incorrectly reported as 225.25 and is actually 25.25. Thus the standard deviation of Sr for bedrock samples is still quite high, but our measurements probably are representative of the respective catchments from which the rocks were sampled. Sample sizes for all compartments have also been added.

39) Table 2 and main text: Note that it is "Akaike" not "Akaiki"

This has been revised in table and text

40) Figure 8. Harmonize legend labels with other figures. Update species names according text.

The figure showing changes in diatom assemblages (Fig. 10) has been revised with consistent species names as appear in text.

41) References: check typo errors and complete references.
Interactive comment on Biogeosciences Discuss., 7, 8723, 2010.

Citations have been checked for inconsistencies

References included in comments to Referee #1:

Ilyashuk, E. A., Koinig, K. A., Heiri, O., Ilyashuk, B. P., and Psenner, R.: Holocene temperature variations at a high-altitude site in the Eastern Alps: a chironomid record from Schwarzsee ob Sölden, Austria, Quaternary Science Reviews, 30, 176-191, 2011.

Jensen, H. S., Kristensen, P., Jeppesen, E., and Skytthe, A.: Iron: phosphorus ratio in surface sediment as an indicator of phosphate release from aerobic sediments in shallow lakes, Hydrobiologia, 235, 731-743, 1992.

Lawrence, C. R., Painter, T. H., Landry, C. C., and Neff, J. C.: Contemporary geochemical composition and flux of aeolian dust to the San Juan Mountains, Colorado, United States, J. Geophys. Res., 115, G03007, doi:10.1029/2009JG001077, 2010.

Neff, J. C., Ballantyne, A. P., Farmer, G. L., Mahowald, N. M., Conroy, J. L., Landry, C. C., Overpeck, J. T., Painter, T. H., Lawrence, C. R., and Reynolds, R. L.: Increasing eolian dust deposition in the western United States linked to humanÂ activity, Nature Geoscience, 1, 189, 2008.

Painter, T. H., Barrett, A. P., Landry, C. C., Neff, J. C., Cassidy, M. P., Lawrence, C. R., McBride, K. E., and Farmer, G. L.: Impact of disturbed desert soils on duration of mountain snow cover, Geophysical Research Letters, 34, L12502, doi:10.1029/2007GL030284, 2007.

Reheis, M. C.: A 16-year record of eolian dust in Southern Nevada and California, USA: Controls on dust generation and accumulation, Journal of Arid Environments, 67, 487-520, 2006.

Sgro, G. V., Poole, J. B., and Johansen, J. R.: Diatom species composition and ecology of the Animas River Watershed, Colorado, USA, Western North American Naturalist, 67, 510-519, 2007.