

***Interactive comment on “Atmospheric deposition as a source of carbon and nutrients to barren, alpine soils of the Colorado Rocky Mountains” by N. Mladenov et al.***

**Anonymous Referee #2**

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AUTHORS: Mladenov, Williams, Schmidt, Cawley

TITLE: Atmospheric deposition as a source of carbon and nutrients to barren, alpine soils of the Colorado Rocky Mountains

The Mladenov et al. manuscript describes original research on carbon, nitrogen, and phosphorus inputs to and outputs from an alpine catchment at the Niwot Ridge LTER/CZO site in the Rocky Mountains of Colorado, USA. The manuscript presents data that document substantial inputs of C, N, and P via atmospheric deposition, particularly with infrequent yet large-magnitude dry deposition events. The calculated atmospheric inputs are sometimes a remarkably large proportion of the solute yields in

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streams leading to a realization that solute yields from these catchments may be more sensitive to atmospheric deposition and atmospheric pollutants than is commonly conceptualized. This latter point is novel and the strength of the research. The study also includes compositional measures of DOM and this aspect may constitute a novel application of metrics that are now common for surface waters but rarely measured in precipitation. With major revision, the content may constitute an important contribution to Biogeosciences Discussions. The most substantial of revisions should address: 1) validation data that are needed to extrapolate knowledge of spectral properties known for surface waters to precipitation; 2) uncertainty of calculated values that needs to be discussed and quantified; and 3) removal of content related to biogeochemical processes for which no direct measurements or strong evidence is provided. Beyond that, I encourage the authors to thoroughly review the manuscript for wordsmithing, to standardize terminology, add additional details, specify vaguely presented information, define all formulas/acronyms/abbreviations, and remove redundancies. I am providing a marked manuscript with many suggestions and comments for the authors to consider; these comments are not exhaustive – the authors should consider these as guideposts to revise for consistency, relevance, and salience.

Many values are calculated without mention of uncertainty. Uncertainty does need to be acknowledged and substantively discussed, preferably via quantitative analysis. This aspect is especially pertinent to measures of DOC, DON, and DOP that may be very close to the limits of detection without any mention of how non-detects or values below detection limits are used in calculations of atmospheric deposition and stream solute yields. Another example is upscaling of point measurements of precipitation chemistry from a mix of synoptic snow surveys and repeated measures made at a single atmospheric deposition monitoring station. The calculation of dry deposition is another particularly important calculation to bound with uncertainty analysis.

Data are presented on spectral properties of DOM that were measured for surface and precipitation waters. The authors use these data to interpret a variety of compositional

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or functional properties of DOM. For these compositional and functional properties, the authors extrapolate relationships that have been quantified for surface waters to precipitation without providing any evidence that these extrapolations are valid. Validation and discussion of the appropriateness of these extrapolations from surface waters to precipitation is needed.

The deposition and stream solute yield data document patterns and the spectral data are suggestive of some biogeochemical processes, but none of the presented data are direct measurements of biogeochemical transformations. Interpretations based on the patterns and magnitudes of the deposition and yield data are sound and well developed. In contrast, interpretations related to unmeasured biogeochemical processes are highly speculative and there is little basis for delving into topic areas that are not directly supported by the presented data. While the patterns may be suggestive of processes, many of the processes that are mentioned in the discussions cannot be definitively pinpointed with the information that has been presented and many other unmentioned processes may also be relevant. I suggest a focus on documenting and interpreting deposition/yield patterns while avoiding speculation on processes that were not measured or are not definitive without validated spectral data. Removal of speculation and extraneous content that is not well supported would tighten the focus and strengthen the interpretation while leading to a more concise and well-formulated manuscript. The manuscript could easily be trimmed by several pages without compromising the quality of the best described and interpreted research findings.

Also, despite mention in the first sentence of the introduction, the focus of the manuscript is not climate change and the authors should address this topic more specifically or remove if not highly relevant.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/9/C843/2012/bgd-9-C843-2012-supplement.pdf>

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