This paper reports soluble nitrogen and phosphorous concentrations of a Sierra Nevada soil with and without application of artificial rainfall. The data are not very extensive: samples were taken at one point in time from two sites; there was a comparison of forested and non-forested plots in the original design but the forested plots were dropped from the study. It is always useful to make such information available, no matter how limited: progress in understanding of ecosystem nutrient cycling depends on having information from a wide variety of sites.

There are some problems with the presentation and interpretation of these results in the current paper.

The title and text of the paper focus on "colloid-bound" N and P. Operationally, "colloidal" N and P were found by (1) centrifuging to remove the $>0.45\mu m$ particles, (2) analyzing for inorganic nitrate, ammonium, and phosphate, and (3) digesting with persulfate and analyzing for inorganic N and P. The difference between the persulfate digestion and the inorganic is called "colloid bound." As I recall, persulfate digestion following filtering to $<0.45\mu m$ is exactly the method used for determining dissolved organic N and P. I may be out of date on methods; enough background should be provided in the paper to allow a reader to understand how their definition of "colloid-bound" nutrients relates to dissolved (non-particulate) nutrients not measured as inorganic N and P. Differences between using millipore filtration and centrifugation to obtain the $<0.45\mu m$ fraction should also be discussed.

It would be useful to know whether the colloidal N and P are organic or inorganically bound. The authors discuss dispersion of clays and exposure of sesquioxide surface; they seem to assume that inorganic colloids are responsible for this fraction. If they could report the DOC of their solutions, we might be able to judge whether the amounts of N and P reported are too great to be attributed to dissolved organic matter.

The design of the experiment was weak. First, there are only two sites; it is not fair to consider slope a treatment when it is not replicated. In fact, the two sites are blocking for slope, and slope is rightly omitted from the statistical analysis (except that I can't tell whether it was done correctly). Thus the rainfall treatment has replication of 2. Presumably the experiment was limited by the device used to apply the artificial rainfall. Collecting twelve samples from within each block is an unfortunate use of analytical effort; it would have been better to have more sites or even more plots within each site.

Simple effects (means by treatment) should be presented. It is difficult to interpret interactions without these.

Finally, the paper could be much improved in style and presentation. I am returning the original manuscript because I marked areas that need attention, but editorial or writing assistance would be useful overall. It wasn't easy to determine the experimental design. The component square in the grid don't add up to the dimensions of the grid. The term "rainfall simulation" puts emphasis on the simulation, and makes me think of a computer model.

Please don't hesitate to contact me if I can be of any further assistance.

Ruth Yanai
SUNY College of Environmental Science and Forestry
Syracuse NY 13210
phone: 315 470-6955
fax: 315 470-6954
email: rdyanai@mailbox.syr.edu

7,3; 7,13 I don't know the terms "modder" and "hemiorganic." Are they understood by your audience and are they well defined?

8,3-13 These numbers would be more easily found in a table.

8,23-9,5 These numbers, too, would be easier to read in a table.

12,25 This study should be described in the methods section!

13,2-19 These results could be presented in a table.

13,21ff. Sentences of the form "Table n presents..." are usually unnecessary. I prefer to make statements about the findings and include references to tables and figures only parenthetically. Next example on 14,21.

13, 21ff. The nature of the "average" used for streamwater (and other averages reported) should be defined here. Unweighted? If weighted, how weighted?

14,18 The placement of the citation suggests that Laudelout (1989) is cited for having found the fluctuations, not for having proved these fluctuations to be microbial in origin. If so, we need more evidence for the claim that they are microbial!

15 See comments on Figs 2 and 3 in General Comments, above.

16,9 More limited than what?

16,13 Fig 5 only goes to 8 mm/day; you have data up to 15 mm/day? Above that the relationship is not linear? I would have assumed that Fig 5 showed all the data.

16,16 201 does not agree with Fig 5

16,17 I guess this is a language problem; I know what you mean.

16-17 You could put the equations for the regression lines on the graphs and save a lot of text.

17,12-17 These sentences need work.

17,17ff. This sections is not well motivated. The problem is not clearly stated and you don't tell us why this analysis will solve it. Why would a "particularly unusual" set of conditions be interesting to us? Explain up front.

18,16 Give more explicit conclusions to this section!

18,18 I don't consider section headings to be part of the narrative. The sentences in the paper should make sense independent of the section headings.

18,18 Why 1982-82? Maybe the answer would be clear from the table I wish you had.

Fig 8. Why isn't HCO3 shown?

21 Your explanation for the effect of dilution on exchange of monovalent and divalent cations didn't make sense to me. Le Chatelier's principle would explain the release of cations of any valence from exchange surfaces with dilution of the solution. There must be a better way to explain this process.

22,8 What are these "irreversible transformations of the hydrogen-clay"? They aren't shown in the reaction. What, for that matter, is "the hydrogen-clay"? Must be something to do with weathering, i.e. "lattice dissolution by protons."

22,13-17 Not clear. You need to explain what you mean by turnover rate, or maybe the sentence starts too far away from the purpose of the comparison.

22,19-24 Too many ideas for one sentence

22,19 Similarity in composition: not true. What about M^{++}/M^+?

23,19-24 Separate idea, needs a separate sentence (maybe even paragraph) if you want to tell it at all.

24,10 Compartments 1, 2, and 3 would be better identified by name.

24,23 The model assumptions need to be more completely defined. Linear with respect to what, the size of the donor pools? There are assumed to be no other influences on rates? What else? If it's that simple, I'm not sure we need the model. If there's more to it, maybe we need to see the equations.

25 I'm not pointing out all the sections that need revision for clarity and organization.

25,25 Careful with terminology. Your figure says "vegetation" not "canopy." "Soil" might be a better term than "litter."

26,14 Here we encounter the term "top soil." How does it compare to "litter"? Why not "soil"? Need to define terms and consolidate.

27,11 What do you mean by "exaggerated"? Too high?

28,2 You estimate an input rate of 100 kg Ca from a Ca-rich stratum. This seems to me to be a model result, not an independently estimated input parameter.

28,5 What is a "smooth transition of the loss of Ca?" Maybe we need to see model output. Smooth over time?

28,8 Streamflow output is "well-defined?" Maybe you mean that you have confidence in the values.

28,10-16 These data are not well documented.

28,18 No referent in text, only in heading.

28,21 Does this description belong in the methods section?

28,22-23 If they are similar to other ecosystems, show me how. The review could be very interesting. If such a review exists, cite the review. You only compare to one observation (p. 29 l. 2).

28,10 Justify the selection of 40 kg/ha/yr for uptake. Isn't this number constrained by a requirement for mass balance in the model? If it is a model output (like the weathering rate), say so.

28 Show the results of the model. Give more information on the process of model calibration. What were the uncertainties in model parameterization and what effect do they have on your results?

30,2 You conclude that application of Ca could be necessary to maintain fertility. Isn't this model result completely dependent on your assumption of the 100 kg Ca/ha/yr input by weathering? The conditional nature of your conclusion should be explicit.

30,15-22 You argue that the lower rate of Ca export during base flow means that the conclusions of the model are unlikely to be entirely wrong. First, I couldn't follow the argument. Second, this argument is presumable very important and should be presented in the paper, not the conclusions. Finally, "unlikely" to be "entirely wrong" is not very reassuring. Can you find a way to describe the uncertainty?

30,24 The conclusions regarding climate change and the adaptations of species are not supported in the paper.

See comments on figure captions, figures, and tables.

Figure 1. Is there a way to show variation in a compass rose? Error bars? I'm surprised that one-year averages would look so different.

Figures 7. The parts of the figure are not presented in the order in which they are discussed. There are no units on the axis. The graphs should be labeled. Dates or months would be better time-axis labels than day of the year.