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## ***Interactive comment on “Relationships between substrate, surface characteristics, and vegetation in an initial ecosystem” by P. Biber et al.***

### **Anonymous Referee #1**

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General comments. The most prominent studies of primary succession take advantage of dunes, volcanoes, and glacial forelands. Only a handful of these have managed to simultaneously study geomorphic, biological, and ecosystem processes from the initiation of a barren landscape, and I would argue that none have done so in a coordinated and integrated way. Moreover, these natural disturbances often leave extremely complex landscapes that are difficult to understand thoroughly. Many other studies have focused on anthropogenic primary successional sites such as mine spoils or mine reclamation. While generally encompassing much smaller spatial scales, many important insights have derived from these study systems. Such systems have some advantages too in that they are often in more convenient locations, sometimes more uniform, and sometimes more replicable. There too, however, few if any studies have simultaneously studied geomorphic, hydrological, biological, and ecosystem processes

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in an integrated way. The present study of the initial ecosystem at Chicken Creek is impressive in that it does integrate detailed study of these diverse processes from the time of initiation of a controlled, relatively homogenous ecosystem. Because the abiotic system is constructed, its initial structure is extremely well understood. It is also unusual among such reconstructions in that biological colonization is allowed to occur unassisted. Another key feature is that it comprises a complete sub watershed (catchment) and includes an impermeable lower layer, features that will allow thorough understanding of hydrogeologic processes. This manuscript is the latest in what is already an impressive body of work focused on this site, even though the site and research programme is still in its infancy.

In the authors' words, this system allows "Tracing the development of .. young ecosystems and observing how new relationships and feedbacks emerge with increasing complexity..." I concur. In addition, it may provide numerous insights with more theoretical perspective or that unite previously separated disciplinary views of ecology and geology. The group's recent American Naturalist paper is a good example of this.

The current manuscript helps to achieve the integrated view of geological and ecological processes. There are no ground-shaking findings, but the work provides a very thorough understanding of the transition from a system dominated completely by hydrological and geological features and processes to a system that is much more influenced by vegetation, and in doing so elucidates a number of interesting relationships. For example, the non-linear effects of total cover, spatial location, relief energy, and time on different vegetation categories are particularly well documented. Hence, the paper will certainly be of interest to ecologists interested in primary succession and land reclamation.

Specific Comments. Below, I make a few comments that I hope can improve the manuscript. For the most part I really enjoyed reading the manuscript, but there are a number of issues that should be addressed. These are almost all issues of omitted information that make the presented results difficult to interpret and thus less valuable.

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1. There are some methods I did not evaluate critically because they are either new to me (terrestrial laser scan) or that I did not have the time to evaluate because I would need to review the technique (GAM). Other than this, I found the field and statistical techniques to be appropriate generally well described.

2. In an ordinary printed journal article, I would recommend that the authors could save space by omitting much of the presentation of statistical models, as these are standard and could be incorporated by references. However, if space is available, I think it is worth including.

3. The authors are carefully to honestly state that they conducted preliminary analyses to choose the best variables. That honesty is to be encouraged, but I to be clear when such preliminary analyses are done that the true p values are inflated relative to the ones presented. I think a statement to that effect should be included

4. What is soil skeletal content? 4739: 1. Is it a description of physical structure, or is actual content of biological skeletal material? I searched for this on the web, and after 20-30 minutes could not find a clear definition. Use of the term primarily occurs in work from Germany, Switzerland, Czech republic, but I could not find a definition. Most ecologists reading this paper will not know the term, so please define it. I was especially interested to understand why the presence of Fabaceae would be related to skeletal content.

5. 4733-4744: what are the explanatory variables for H1? Similarly, explanatory variables are not provided for H2. Thus the description of the analyses here is unclear.

6. Figs. 2-4. It is standard to show the data points along with the fitted line, that is not done here. In the absence of that is very difficult to gauge how well supported the results are. I recognize that CIs are provided in the figures, but these obscure data structure. I recognize also that with a mixed effects model the raw data do not always provide an actual visualization of the actual relationships, but perhaps this can be improved by graphing data corrected for the random effects. I also found the figures

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took more time to understand than they should have Y labels do not indicate the actual dependent variable. Also valuable would be to back-transform the axes so that we can see the actual proportion cover. If it were actually biological skeletal content then it would suggest an important role of P limitation for primary successional colonists. In this event, see the recent paper by Lambers et al. 2012 Annals of Botany.

7. A major omission from this manuscript is the complete lack of summary data of the variables. This makes interpretation of the results very difficult and the entire work becomes very abstract. In my opinion these must be included for this manuscript to be publishable. For vegetation should include species list with % covers (maybe at end point) and categories (ann, grass, etc).

8. 4746 effect of organic C weakens with time, but I thought there was initially no organic C. Is organic C highest near upper site edge?

9. 4746: So is rill formation driven by organic carbon or by relief energy?

10. Figs. 2-3 – why only show effects of distance, why not show effects of other key explanatory variables?

11. 4751 5-10: I am having trouble forming a mental image – perhaps including a photograph or two would help visualize how local conditions cause rills perpendicular to the main slope and the nature of the gullies channeling runoff?

12. related to point 7 the authors should provide a species list that includes plant family and classifications for purposes of this manuscript. It seems quite strange to me to include *Rubus* in the same category as tree-form woody plants. The authors single out Fabaceae but do not make clear whether these are the only N-fixing plants or organisms in the system. I realize the authors provide this information in other publications, but it is important enough for interpreting the results in this paper that the information needs to be provided here.

4749: p(woody plants) decreases as total plant cover increases

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13. No information is provided about the surrounding plant communities, sources of carbon input and propagules, local climate, local atmospheric N deposition, etc. I recognize that many of these attributes are described in other publications, but they should nevertheless be briefly described here before referring to those publications. This is especially true if any of these descriptions are in books that are hard for most readers to access.

14. Abstract: the abstract does not need to include the names of the specific statistical techniques or even that your hypotheses were confirmed. The abstract would much improved by including some specific results that support the statement “transition from a geo-hydro towards a bio-geo-hydro system, where pure geomorphology or substrate feedbacks are changing into vegetation-substrate feedback processes”.

Technical corrections. Here are some minor suggestions to improve language use. There Discussion section had quite a few English phrasing issues that I did not take time to recommend corrections for. unproductivity not an english word, change to low productivity or something similar.

4735 10: influences back – change to just influences or feeds back to influence or in turn influences.

4735 20: are the starting conditions at point zero known exactly

is it really called Chicken Creek, or is it something like Huehnchen Bach? Should German language place names be translated?

4737 5: across the catchment area

4740: 5 change despite to Although or While and move roughly to 4740: 9: typo: laser  
4741: 17 - replace further on with thereafter or subsequently 4742: 23 replace with  
The way that ..transformed 4743 20 the question of whether the initial... or H1: Do  
the initial... 4744 5: what does “most useful” actually mean? It shouldn’t just mean  
“gave the best fit or lowest p-value” but instead should mean “best satisfied model

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assumptions". 4744: Nitrogen fixing or Dinitrogen fixing, not nitrogen collecting. 4745: overarching important . . . - incorrect phrasing.

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