

## ***Interactive comment on “Pasture degradation modifies the water and carbon cycles of the Tibetan highlands” by W. Babel et al.***

**Anonymous Referee #2**

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General Comments:

This paper seeks to address the environmental impacts associated with changing pastoral conditions in Tibet through the combined use of several data sets and models. Due to some complications associated with permitting the experiments, the data sets are, unfortunately, rather limited. Similarly, the models used for interpreting these data are highly parameterized and present challenges for representing the pastoral conditions of interest. The manuscript ultimately concludes with some broad statements about linking simulated weather patterns (and, by extension, climate regime changes) to these changing pastoral conditions, and that scaling from the plot scale to the ecosystem-landscape scale is useful for studying processes. The authors end by recommending that a number of additional, follow-on, investigations should be con-

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ducted wherein more data is collected to further test their conclusions.

While I fully recognize the authors' desire to extract as much science as possible from their field campaigns, which were no doubt very cumbersome, I am hesitant to recommend that this manuscript be published in its current state. I disagree with the conclusions that have been drawn primarily because the models do not provide a convincing story of attribution (ie, the simulated weather patterns may be driven by something completely different than pastoral vegetation changes). A better link between the data and the representativeness of the models must be better established before their results can really be interpreted in quantitative way. Furthermore, the generous use of appendices detracts from the readability of the manuscript and serves to obfuscate the reader. However, as this manuscript does highlight results from a field campaign that has been widely reported and is of broad interest to the ecological community, I do think it is suitable for publication in Biogeosciences. However, because these results require further elucidation, I recommend that this paper be accepted only after the aforementioned concerns have been addressed.

Specific Comments:

This paper seeks to inform how changes in the cover of *Kobresia pygmaea* pastures on the Tibetan plateau scale up to the ecosystem/landscape level and make inferences about the potential impacts of these changes. To do this, plot-scale observations are combined with ecosystem-scale observations which are then interpreted through modelling. While this is a sound approach, it is heavily influenced by a significant number of assumptions that are made. Because many of the details regarding these assumptions are confined to the appendices, it is challenging for the reader to gain a thorough understanding of the analyses. I recommend re-working the structure of the paper to include the important details of these data sets and models in the body of the text (Section 2) and reduce/remove the appendices. I can appreciate that appendices were utilized in an attempt to conform to the manuscript preparation guidelines, but I think it is manifesting more of a hindrance in this case (for example, Appendix A is a 1 sentence reference

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to two tables which could easily be incorporated into Section 2). Specifically, I think it would be useful to elaborate on the three model parameters that represent the degradation (missing vegetation, different soil properties, and available energy changes), and explain how the various observations inform these parameterizations. For example, please explicitly describe how the observations from the instruments at the Kema Site (Table A1) translate to the model parameters in Table C1.

In addition to the aforementioned changes to Section 2, I think it would be useful to add some additional references and figures. On page 8869, line 14-15, there is mention of a software package that has been “successfully applied in numerous international field campaigns”; please add appropriate references. On page 8870, line 20-25, a description of how soil monoliths are used for assessing hydrological measurements is given; please add a figure (perhaps a cited figure) to help depict this. On page 8871, line 18-22, soil chamber measurements are described as “besides differences. . . the comparison was satisfactory”; please provide quantitative results so the reader can better interpret the meaning of these measurements.

I am hopeful that the results presented in Section 3 will be more obvious to the reader after adding the detail from the appendix to Section 2. Section 3.5 is still troubling as it seems to assume (rather than prove) that changes in precipitation are being driven directly by changes in surface land cover. For this to be properly attributed, I think that a more thorough, process-based, model investigation must be done in which radiation and convection are both considered. Specifically, I think it would be useful to elaborate on the net-radiation measurements that were taken at these sites to better justify the claim that the “albedo effect can be excluded”. This connection between the surface vegetation and precipitation is one of the primary conclusions of the paper and it must be supported more strongly if it is to remain.

On page 8878, line 10, please change Reco to Reco, for consistency. On page 8879, line 6-14, the discussion of how the 13-C labelling is coupled to the eddy covariance methods is unsatisfactory; please elaborate and provide more results.

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The conclusions of the paper should be better linked to the introduction. For example, there is some discussion about the relationship between ground-cover degradation and carbon cycling in the conclusion that could be previewed in the introduction. Since the “additional research” that is proposed includes a reference to utilizing remote sensing data to interpret the simulations of cloud cover/precipitation, I think it would behoove the authors to add this analysis to this paper to support the aforementioned claims.

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