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

Anonymous Referee #1

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General comments:

Babel and the 28 Co-authors report from an outstanding and impressive project, the collaboration between soil scientists, geobotanists, ecophysiologists, micrometeorologists, meteorologists and geographers. This is an excellent example for the multidisciplinary nature of Biogeosciences. There is absolutely no doubt that this material will fit very well into the scope of this journal.

The work has generated new data from systems that have been rarely, if anyway, investigated before: the Tibetan grasslands. The effects of disturbance are investigated from the sub-plot to the landscape scale employing both several independent empirical approaches and three different modelling approaches. The consistency between empirical data and modelling is carefully tested on the appropriate scales and critically

discussed. The models are then finally used to estimate the consequences of land use change and pasture degradation on the CO2, water and energy exchange with the atmosphere and consequences for the local circulation. The used methodology is generally at a very high scientific standard and I am deeply impressed when imagining the courage, wit, care and endurance that made it possible to establish such measurements in places like the investigated plots. The Authors claim to have shown with their study that large scale degradation of the alpine grasslands reduces their carbon sink strength and is likely to affect large scale weather and on the long run also climate phenomena.

Writing a multidisciplinary manuscript poses the significant challenge to adjust the level of detail and the amount of information to the integrating scope of the manuscript. Otherwise the messages of the manuscript are very difficult to discern for the readers. The scope of this manuscript seems a too wide, the material too much and new for one single research article. I assume that the Authors had the same feeling and chose therefore to put much of the material into appendices that make up in total 30% of the manuscript's text or amount to 175 % of the results & discussion section's text. For me the many appendices did rather lead to confusion than to clarification, as the results and discussion part became so short and in some cases even meaningless without reading the respective parts in the appendices. The scientific core, i.e. the results and discussion part, which at its present state contains too little discussion and scientific evaluation, has way too little weight in this manuscript. It is obvious that publishing the material in a scientific book or a special journal issue would be more appropriate and efficient instead the too tight and partly incomplete presentation in a single article.

A good solution for this manuscript would probably be splitting it up into two papers, one on water and energy fluxes with the consequences on local circulation and a companion paper on the effects of pasture degradation on carbon budgets.

If the scope of the manuscript should, anyway, stay like it is, it must concentrate much more on the multidisciplinary synthesis around the two main scientific messages. Much of the so far unpublished material does not need to be included in this manuscript, if one carefully evaluates its relevance for the multidisciplinary synthesis. This would lessen the need for long and detailed explanations. My recommendation would be rewriting the manuscript (sections 2-4), including only part of the presented material and keeping the main messages in mind at every stage.

As an example, it remained unclear to me, what the role of the isotope study was for this work. Tracking the carbon in the system has neither been mentioned in the objectives and hypotheses nor in the introduction. Isotope work was not mentioned in the explanation of the overall approach (section 2.5) and the results from this isotope study were not used at all in the summarising discussion/conclusions, i.e. section 4. This suggests that the isotope study is probably not essential for this study. It would rather find its place in a carbon companion paper, which would allow space to discuss the disturbance effects on carbon dynamics.

I value the methodical depth of the manuscript in its single parts and the overall set-up of the experiment very high, but as a reviewer I have to solely judge the quality of the presented manuscript and here I don't suggest publishing it as it is and recommend the authors to either reconsider the publication strategy or revising the existing manuscript carefully along with the above recommendations. I expect that such changes will reward the Authors for their enormous work by strongly increasing the scientific impact of the paper.

Specific and technical comments:

(the line numbers are regarding to 'bg-2014-283-manuscript-version2.pdf')

Abstract

39: 'coupled' is not the right term here, rather use 'investigating . . . together with' 42: 'coupled' is not the right term here, rather use, e.g., 'combine'. 48 -50: The first sentence states that the sum of evapotranspiration (ET) remains unaffected, the following

assumes likely triggered by enhanced evaporation. It is not obvious that, if ET stays constant but E becomes larger at the costs of T in the degraded systems how this explains the earlier onset of convection (Include the aspect of 'timing' in the sentence).

1. Introduction

The introduction is well written and gives a very nice overview over the different aspects of land degradation at the very little investigated ecosystems looked at from different disciplines. 56: impact->impacts. There are several types. 75: For what is it important that this definition was later used by Zhou et al. (2005)? 82: Please mention for what this is relevant. 102: The sentence on radiation does not fit here, because 1. radiation is a driver - not a parameter and 2. it is not affected by degradation. 104-106: Please reformulate this as a main hypothesis. 'We expect that . . .

2. Methods

117.5 I was missing an introduction to the general project approach, i.e. what was measured, why, where and how are the measurements logically interlinked. Later I saw that this was attempted in section 2.5. For me it would have been more logical to move section 2.5 to here.

2.1

Table 1: 30°47'âĂšN, 90°60'âĂšE ->30°47âĂš N, 90°60âĂšE (60' = 1°, so why not 91° 0'?) The location for these coordinates is in or very close to Lake Nam Co (at least gmap would suggest this). If this was correct this would put some important constraints on the eddy covariance (EC) data evaluation, which was not mentioned in the manuscript. 2.2 150: Please add a sentence why this classification was only done at Kema and not at the other sites and how this mapping is related to the objectives of this study (I saw later that this was done in 2.5). 169 '(('->'(' 179 (Cyperaceae) family already mentioned in line 137

2.3.1

186: Please add 'the' before 'Nam Co site' and before 'Kema site' and all other instances 189: I suggest harmonising the way you refer to the type and the supplier in Table A1. 208-215: This section describes that the flux data have been corrected using the non-closure of the energy balance. The way this was done is new and needs more explicit description in order to enable others to reproduce it and compare their approaches. It's a bit confusing that, as it stands now, attributing the lack of energy balance closure mostly to the sensible heat flux was based on an assumption (208), later some references are given, but the assumption is not further tested, supported or justified. Please comment and clarify accordingly in the text. 210: 'postulated from model studies' but there is only one reference. 214: The abbreviation 'Bo' (Bowen ratio?) was not defined. It is also important to know when partitioning the missing heat flux into latent and sensible heat, whether the corrected or the uncorrected Bowen ratio was used. Please clarify this, maybe best by including the equation which was used in the manuscript or refer explicitly to a published one. 243: 'long-term chamber' is probably the wrong term; rather use 'automated chamber'. 253-254: Why should the differences compensate? Was this assumption tested?

2.3.4

In general it became not clear to me what the essential contribution of the 13C labelling to the overall objectives of the study was. 275: chased -> traced?

2.5

312-361: I suggest moving this to the beginning of the section, because it makes it easier to understand the other sub-section. Table 3: Please include the periods of observation in the table. 343: Please change to 'For the investigation of the impact of surface degradation on the atmosphere . . .' or even specify 'local circulation in the atmosphere' (atmospheric impact sounds at first glance as if the atmosphere would cause an impact) Please explain how the 13C labelling and the laboratory experiments contributed to this study. There is no reference to them in in the relevant parts of the

manuscript (see comments above).

3. Results and discussion

General comment to this section: I found the structure of first 3 sub-sections very confusing. The headings don't help much. In 3.1 the models are being compared with EC measurements. The heading does not tell this; it would rather be a general heading for 3.1-3.3. In 3.1 includes both water and carbon and thus the two models used in this study. In 3.2 EC – micro lysimeter and SEWAB were compared, but EC results are virtually missing in the text. In 3.3 SVAT-CN is compared with chambers independently from the results from 3.1, where it was compared with EC measurements. For the reader it is very difficult to recall the comparisons for the different sites and the three different methods. A more systematic presentation, e.g., a matrix with correlation and regression parameters and average daily sums or even the graphs, would help much.

3.1

In general: I miss a short comment on how much the above mentioned EBC correction changed the water flux values that were otherwise obtained with the eddy covariance method. 365: Change heading to 'Comparison of modelled fluxes with results from eddy covariance flux measurements' 371: Fig. D1 should be part of this section, where it was referenced (regression parameters) 376: Please explain, which medians are meant. I assume medians from an ensemble diurnal cycle over the entire measurement period. 374-382: Please refer to figures, where applicable. 3.2 392: Here you only compare the SEWAB simulations and results from microlysimeters. Why didn't you include eddy covariance data as mentioned in the introduction to this paragraph? Or are the results form EC only represented in the world 'all' (line 392)? Why does Figure 3 not contain the respective data from EC? 392-393: 'all approaches showed no clear differences' : Please justify this statement with a quantitative statistical analysis and use 'significant' instead of the qualitative term 'clear'. 393: Please add 'the model results suggest that' - before 'even for dense . . .' to make clear that this is a model result as well as the partitioning is. Do you have any empirical evidence that this partitioning is realistic? 395: Instead of 'decreasing' I suggest writing 'is lower', that fits better for a comparison.

3.3

General comment: The way the parameters are tuned to the chamber data is not state of the art. (see comments to C2, below). 408: What is the reason for the different behaviour on IM between the two periods (only 3 weeks' time difference). Was there a shift in phenology? Why was this not considered in the model parameterisation? 409: The Figure looks nice, but regression plots for GPP, Reco and NEE would make it easier to compare the model simulations and data. 411: 'yield'->'yielded' 422-423: Please check the format of the unit 425: Reco->R_sub_eco

3.4

General comment: Please explain how the results of this investigation are used the context of the project.

What is the relevance of the montane Kobresia pasture for this work? None of the other parts og the manuscript refer to a montane pasture.

451: Please define 'allocation period' 447-449 This is an interesting approach and result but not much discussed. What does it say regarding the modification of the carbon cycle of the Tibetan highlands through pasture degradation?

3.5

485: I had difficulties to compare the timing of convection between V25 and V75. If you rearrange the panels such that v25 is on top of v75 with the dry and wet cases on the left and right this would be easier. 494: I could not find Figure B1 495: says: ' Evapotranspiration decreases from SIM to SBS in this model degradation experiment . . .' the abstract says ' Pasture degradation leads to a shift from transpiration to evaporation while the total sum of evapotranspiration remains unaffected' please explain this

contradiction.

4. Conclusions

General comment: This section is a bit too long for conclusions. It is rather a summarising section on the discussion, which is very useful. It would be a good idea to make it a sub-section under discussion and let it end with a paragraph or two on conclusions. This would give you more space for general discussions, which are definitely lacking, and more exemplification, e.g. of the statements that are now presented as a bullet point list (see next comment). 519: 'plot size' -> 'plot scale' 533: This bullet point list lacks explanation. Which are the goals that we could achieve from additional research? How is this need derived from this study? What does ' Investigation of the processes along elevation gradients, with special reference to functional dependences' really mean? Which processes? Which functions? What should have a functional dependency to what? The terms 'function' and 'ecological function' have been used several times in the manuscript leaving it to the reader, what exactly was meant.

Appendices

In general, I would rather avoid having that long appendices. I see this as an indication that the mass of results and the need for explanation exceeds the 'capacity' of a single research paper. Additionally it creates some redundancy between some of the appendices and the results (discussion) part. 617: '2005))'-> '2005)' 685: 'physiologically based'-> 'physiology based' 673- 691 and 716-734 (appendix C2): These sections are very interesting. In order to establish a complete physiological parameter set the gas-exchange of Kobresia plants was investigated in a greenhouse in Göttingen. It is not mentioned whether these plants were taken from the site or raised from seeds from the site. Interestingly, using the parameters from the greenhouse measurements, the model underestimated the fluxes in chambers at the site. This is on its own an interesting result, showing how little representative physiological parameters taken in artificial environments can be for the field situation.

To adjust the model to the measurements, three model parameters were manually multiplied by a factor of 1.6. This doesn't seem to be the most elegant method to calibrate a model. There are several, more objective and powerful parameter estimation methods, e.g., multifactorial non-linear regression, Monte Carlo simulation based methods, Bayesian calibration. These will also yield the parameter uncertainty, an aspect that was so far neglected in this study. What was the reason for the choice of one single factor for all three of the parameters? I don't recommend the in depth discussion of this material in this manuscript, but it shows that the too tight presentation of the material leaves many questions open.

Table C2: the reference given in the caption that explains the model, is not generally available. Please use another one or describe the model. What are the uncertainty ranges for the model parameters? Please check the units; in other versions of the model the scaling parameters are dimensionless. 765-767: Please reformulate in a way that the correlation showed that the simulation (not the correlation) was realistic.

Interactive comment on Biogeosciences Discuss., 11, 8861, 2014.