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

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We thank Reviewer #2 for his comments, which have been of help to us in improving the manuscript.

General comments

We believe that the reviewer misunderstood our intention of making some general conclusions based on our experimental investigations. We will check the manuscript carefully to find out which statements could have led to this misunderstanding. For clarification: The experimental measurements with chambers and micro-lysimeters were done

below the micro-gamma-scale (Orlanski, 1975). An upscaling to the micro-beta(alpha)-scale was done with chambers and micro-lysimeters within the footprint of the eddy covariance measurements (Reth et al., 2005; Aubinet et al., 2012; Leclerc and Foken, 2014), which is also the typical scale of the applied models SEWAB and SVAT-CN. Therefore, it was possible to use parameters found on this (ecosystem) scale and there was no need to apply "highly parameterized" models with so-called effective parameters. Up to this point, our study is a process study and is scale consistent.

Reviewer #2 appears to have two points of criticism:

1. The reviewer assumes an inadmissible generalization of the results with the ATHAM model in Chapters 2.4 and 3.5. We are convinced that processes relevant on the micro-scale can be assumed to be also relevant on a larger scale if the land cover is uniform on this larger scale. Because the soil-vegetation-atmosphere-transfer scheme of ATHAM is in good agreement with SEWAB (Gerken et al., 2012) we believe that the use of the process-oriented model ATHAM is justified. In this process study we found a time-shift between evaporation and transpiration in the case of degraded Kobresia pastures, which we feel to be a relevant mechanism for the effect of degraded soil-vegetation on the formation of clouds and their impact on radiation. Certainly this mechanism needs further elaboration, as we propose in the conclusions.

2. The link to "climate" is referred to the paper by Miehe et al. (2014), who found evidence of increasing degradation of the Kobresia pastures from biological/biogeographical long-term observations. The link between our results and this publication was made in the discussion. It is not a topic of this current paper.

Specific comments (first paragraph)

Concerning the scale problem, see above. The problem of the appendices was already discussed in our answer to Reviewer #1. In accordance with Reviewer #1, we have modified Appendix D and included a Table in Chapter 3.1. In addition, we will delete Appendix A and move both tables into Chapter 2. However, we do not understand the second part of this paragraph. All model parameters were separated according to the different degrees of degradation in Table C1. Some parameters are internal model parameters given in the literature, with the references given in Appendix C. Table C1 shows (e.g. in footnotes) which parameters were measured (e.g. albedo, LAI, and plant cover). The meteorological measurements were used as input data for the models as described in Sect. 2.5. We believe that Tables A1 and C1 are well linked. Some more details about the parameters used are given in our answer to Reviewer #1 (Biogeoscience Discuss. 11, 2014, C4129-C4130) and in the references added there (Wohlfahrt et al., 1998; Falge et al., 2003).

Specific comments (second paragraph)

Page 8869, line 14-15: We believe that the TK3 program is well-known in the eddy covariance community (Aubinet et al., 2012). We have more than 300 software downloads and we have more than 100 users. We will add a recent comparison paper (Fratini and Mauder, 2014), but will not quote the numerous experiments where the TK3 program has been used (e.g the complete calculation of large experiments with TK3 by Mauder et al., 2006; Metzger et al., 2006; Mauder et al., 2007; Eigenmann et al., 2010; Mauder et al., 2013). We will change the sentence “and successfully applied in numerous international field campaigns” into “calculated fluxes match up-to-date micrometeorological standards (Foken et al., 2012; Rebmann et al., 2012).”

Page 8870 line 20-25: We will add the experiment documentation (Biermann et al., 2013, available free online) as an additional reference, where all details, including pictures, are given. One of the figures is added to this answer.

Page 8871, line 18-22: see also Reviewer #1 (Biogeoscience Discuss. 11, 2014, C4122): This sentence was deleted, because coherent structures were not tested as in the paper by Riederer et al. (2014), but this should not be relevant. We added “at day time”. Quantitative results are given in this reference.

Specific comments (third paragraph)

The model simulations were performed with the well established atmospheric model ATHAM, extended by a soil-vegetation module tested against observations and the more specific soil-vegetation model SEWAB for the observation site (Gerken et al. 2012). ATHAM explicitly treats clouds, convection and precipitation and their interaction with radiation (see Appendix B3). It is, therefore, a suitable tool for process oriented analysis, and is by no means "highly parameterized“. As mentioned in the text (see Table C1), observed albedo is similar for intact and degraded root mats, hence albedo effect can be neglected and only evapo-transpiration effects are relevant. Since the large-scale forcing of the model is identical for all four simulations, the simulation results differ solely due to the imposed vegetation and moisture initialization. While there are potentially complex feedbacks in the surface cover and soil moisture that are not addressed in this study, there is a clear effect that links soil moisture/vegetation cover and convection, as indeed both dry and wet cases differ. The effect on convection, cloud density and precipitation is obvious from Figure 8. Several aspects were already published (Gerken et al., 2013; Gerken et al., 2014b). A more complex investigation of all the atmospheric processes involved would be beyond the scope of the current overview paper. A manuscript dealing with these issues is about to be submitted (Gerken et al., 2014a).

Specific comments (fourth paragraph)

Page 8878, line 10: done

Page 8879, line 6-14: Responding to Reviewer #1 (Biogeoscience Discuss. 11, 2014, C4126), we have added the allocation period, which is the time up to a steady state situation of ^{13}C fixing, in our case 15 days (total trace period was 64 days in Kema and 27 days in Xinghai). Therefore, within the short growing season on the Tibetan Plateau, only one experiment per season (with repetitions; error bars are given in Figure 7) is possible. The combination of the relative carbon fixing with the labeling with eddy-covariance measurements (for the case of a nearly steady-state daily carbon uptake, line 10-11) offers an absolute carbon fixing estimate. We will make some small changes in the text to make the procedure of the experiment clearer.

Specific comments (fifth paragraph)

As we already noted in our answer to Reviewer # 1 (Biogeoscience Discuss. 11, 2014, C4127-C4128), we will make some significant changes in the conclusion chapter and we will add some words and some further references to make our ideas, as given in the bullet points, better understandable. Furthermore, we will also make some changes to create a better link between the introduction and the conclusion.

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Fig. 1. Micro-Lysimeter installed at Kema site (Photo by S. Willinghöfer, 2012)