

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

**W. Babel et al.**

thomas.foken@uni-bayreuth.de

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First of all, we would like to thank Reviewer #1 for his very constructive comments and many – even very small – hints to towards improving the manuscript. We are also grateful that he acknowledged the interdisciplinary work of researchers with different scientific backgrounds. He also recognised the problem we faced in formulating a single paper with material for a special issue. Our intention was to bring different aspects of the land use degradation on the Tibetan plateau together and to follow the pathway up to final conclusions for weather and climate. Because all the authors have worked for a long time on the Plateau and have published several papers, and all experimental documentations are cited and are available online, we found that a single paper is possible. We only had problems with the inclusion of all modeling approaches, be-

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cause eddy-covariance measurements were planned for all experimental phases; the non-scientific reason was mentioned in lines 313-325 (BGD, p. 8874, line 2-25). We therefore moved all model adaptations to the section describing the conditions of the Plateau, and model validations into the Appendix. We believe that a splitting of the paper into two separate papers would destroy the story ranging from the degradation of the meadows and the change of the carbon pathways – investigated with isotope analysis – up to a different ratio between evaporation and transpiration with consequences for convection and even climate. Furthermore, two papers would need a lot of cross-references and the papers could be self-plagiates. We therefore decided not to split the manuscript. Nevertheless, the careful review showed us where information is missing and where the thread of the story is difficult to find. Because the reviewer also would accept a single manuscript, we would like to follow his suggestions carefully.

Because the review is based on the submitted manuscript and its line numbering, we also added the pages and lines of the printed discussion paper in order to make our answers transparent to everybody.

General comments (page C3442 of the review):

The paper is based on more than ten Master- and PhD-theses, and not all material is published yet. Therefore significant details must be included in the manuscript, but we have carefully checked the manuscript to ensure that the reader sees the messages of the paper. In particular, the model validations and the modifications of some parameters are important because the application of models which work well under Central European conditions to the Tibetan Plateau, with its different land uses, is not trivial. On the other hand, we have encountered reluctance on the part of journals to publish these studies as single papers.

We agree to modify some parts of the manuscript. The most relevant change will be to move parts of Chapter 2.5 into the introduction.

We apologize that the reason we included the isotope study with some main results

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into this paper was not clear. The allocation of carbon in roots and soil is very specific for Kobresia meadows and not comparable with montane meadows. Therefore only the turf layer is very stable and gives the Tibetan Plateau its specific character. Our labeling studies were the first at high altitudes.

We will make small recommended changes with no further mention in this reply.

Abstract:

Line 48-50 (p. 8863, line 15-18): We have modified the sentence to make the meaning clear: “Pasture degradation leads to a shift from transpiration to evaporation, while a change in the sum of evapotranspiration over a longer period cannot be confirmed. The results show an earlier onset of convection and cloud generation, likely triggered by a shift in evapotranspiration timing when dominated by evaporation. Consequently, precipitation starts earlier and clouds decrease the incoming solar radiation.”

1. Introduction:

Line 75 (p. 8864, line 18): The paper by Zhou et al. (2005) is in English while the paper by Liu et al. is in Chinese; we will include this in the text and not only in the list of references.

Line 82(p. 8864, line 24-25): We agree that this is not relevant for the paper, but it describes the dimension of the problem.

Line 102(p. 8865, line 18): we have changed “parameters” to “factors” (line 93, p. 8865, line 8).

Line 104-106 (p. 8865, line 20) we will reformulate accordingly.

2. Methods:

A simple removal of Chapter 2.5 to the beginning of Chapter 2 or even to Chapter 1 is not possible because Chapter 2.5 is based on many details given in Chapters 2.1 to 2.4. We have therefore only moved the first paragraph of Chapter 2.5 (lines 313-323,

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p. 8874, line 2-25) to the introduction and also the last paragraph (line 352-361, p. 8875, line 15-25), and we will add a further paragraph to the introduction with a short explanation of the research concept.

## 2.1 Study site:

Thanks for detecting this mistake. The correct location is: 30°46′N, 90°58′E. {with seconds: 30°46′22″N, 90°57′47″E, but we can leave them out to be consistent with the other locations}. It is correct that the station is near the Nam Co, but the nearest water body is 300m away and is either not in the footprint area of the EC measurements, or is not significant. A detailed footprint analysis has been carried out by Zhou et al. (2011). We will add a remark in the manuscript.

Line 151 ff (p. 8867, line 18ff): We will change the paragraph to make it clear that the classification is more general but the distribution is necessary, e.g. for the eddy-covariance footprint.

## 2.3.1 Micrometeorological measurements:

Line 208-215 (p. 8870, line 2-9): We agree that the energy balance closure by Charu-chittipan et al. (2014) is new, but the paper is freely available. We will add to the reference the number of the equation and the relevant figure. Furthermore, we will replace line 114 (see also comment in Chapter 3.1) with “for the measured range of Bowen ratios from 0.12 (5% quantile) to 3.3 (95% quantile) 37 % to 2 % of the available energy was moved to the latent heat flux. For Kema 2010 this is equal to an addition of 5 Wm<sup>-2</sup> missing energy to the latent heat flux on average.”

## 2.3.3 Soil gas exchange measurements:

Line 253-254 (p. 8871, line 19-21) was deleted, because coherent structures were not tested as was done in the paper by Riederer et al. (2014), but this should not be relevant. We added “at day time”.

## 2.3.4 13C labeling:

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“Chase” is a normal term in labeling studies. Nevertheless, we see that not every reader might know that, therefore we change the formulation to: “. . .was traced. . .”

2.5 Experimental and modeling concept:

See Introduction; reference to the 13C measurements is now given in the introduction.

3. Results:

We rename this chapter as “Results”, see Chapter 4

We believe that the confusion comes from the administrative problems mentioned in Chapter 2.5 lines 313 -325 (p. 8874, line 2-25). Therefore we will include an additional sentence at the beginning: “Because of the administrative problems mentioned in the introduction, we used separate experiments in 2009 (Nam Co) and 2010 (Kema) to validate models against eddy-covariance data (Chapter 3.1). These models were compared in 2012 against micro-lysimeters (Chapter 3.2) and against chambers (Chapter 3.3). Because in the scope of this paper the models are only tools used to replace the (not possible) eddy-covariance measurements, the model description and adaptation were moved to an appendix. The specific results are given in Chapters 3.4-3.6”

The new headlines will be: 3.1 Comparison of eddy-covariance flux measurements with modelled fluxes 3.2 Class-specific comparison of evapotranspiration with micro-lysimeter measurements and SEWAB simulations 3.3 Class-specific comparison of carbon fluxes with chamber measurements and SVAT-CN simulations

3.1 Comparison of eddy-covariance flux measurements with modelled fluxes:

A short comment about energy balance closure is now given in Chapter 2.3.1. Line 376 (p. 8876, line 13; also 746, p. 8890, line 13)

Your assumption is correct; the medians are hourly medians from an ensemble diurnal cycle over the entire period. We will clarify this in the text.

We will add the following Table (see enclosed pdf-file) to this Chapter and delete these

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data in Appendix D. We think that we now have enough information about the models in the main text.

3.2 Class-specific comparison of evapotranspiration with micro-lysimeter measurements and SEWAB simulations:

Line 384 (p. 8876, line 21) : we have deleted EC in the headline and reformulated the headline, see above.

We will not directly compare EC measurements with the micro-lysimeter, because two additional steps would then have to be explained in more detail: 1) the EC measurements have to be gapfilled with the simulations in order to be consistent with the integrative lysimeter measurements over several periods. 2) Because of its footprint, the EC measurements cannot be related to the degradation classes. We see that this attempt has not been carried out very clearly and we correct the relevant text passages in 3.2 and additionally delete in 3.1 (p.8876, lines7-9): “Therefore, the simulations are well suited to filling the gaps in the eddy-covariance measurements for comparison of evapotranspiration with micro-lysimeter measurements”

Regarding EC data see 2.5 line 313-325 (p. 8874, line 2-25).

392-393 (p. 8877, line 4-5): We will reformulate section 3.2 in order to eliminate the EC measurements (see reformulation below). Furthermore, we include a statistical analysis of the differences between BS and IM-lysimeter (n=4) using the Wilcoxon rank sum test. For the simulations, no comparable uncertainty can be given (see our answer to the questions raised for appendix C2), but we can test whether these values were within the confidence interval of the lysimeter measurements ( $1.96 \cdot$  standard error of the mean).

393 (p. 8877, line 5): will be reformulated

We cannot support this model result through empirical evidence as we did not measure transpiration and there is, to our knowledge, no available study of the measurement of

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the partitioning of evaporation and transpiration on the Tibetan Plateau. We therefore cannot quantitatively prove the partitioning found by the model. On the other hand, it is quite consistent with our understanding that the relationship transpiration / evaporation must decrease with declining plant cover. Furthermore, the fact that the total evapotranspiration does not really change from IM to BS is supported by the micro-lysimeter measurements. Therefore we regard our qualitative statement made in this study as reliable.

Reformulation of section 3.2: "3.2 Class-specific comparison of evapotranspiration with micro-lysimeter and SEWAB simulations

Daily evapotranspiration (ET) of the *Kobresia pygmaea* ecosystem was about 2mmd<sup>-1</sup> during dry periods and increased to 6mmd<sup>-1</sup> after sufficient precipitation (not shown). This was confirmed with small weighable micro-lysimeters giving a direct measure of ET from small soil columns over several days and SEWAB simulations. For a 33 day period at Kema 2010, ET for both micro-lysimeter and simulations varied around 1.9mmd<sup>-1</sup>, reflecting drier conditions, while in 2012 the micro-lysimeter showed a maximum ET of 2.7mmd<sup>-1</sup> at BS, and the simulations 3.5mmd<sup>-1</sup> at IM (Figure 3). In both periods, the lysimeter measurements do not differ significantly between IM and BS (two-sided Wilcoxon rank sum test, n=4). The model results support this finding in general, as they are within the 95% confidence interval (1.96 times the standard error) of the lysimeter measurements in three cases; however they differ significantly from the lysimeter measurements for IM in 2012. The model results suggest that even for dense vegetation cover (IM), a considerable part of ET stems from evaporation. At DM and BS, transpiration of the small aboveground part of *Kobresia* is lower, but it is compensated by evaporation. Therefore, the water balance is mainly driven by physical factors, i.e. atmospheric evaporative demand and soil water content."

3.3 Class-specific comparison of carbon fluxes with chamber measurements and SVAT-CN simulations:

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General comment: Please see our statement regarding the comment for C2 408 (p. 8877, line 18): The differences in weather conditions were small between P1 and P4 and they do not contribute to the mismatch between chamber and model simulations as the model was forced with measured standard meteorological data. The important point is that the measurements for P1 and P4 were not done on exactly the same plot. An above-ground biomass survey from inside the chamber rings (and other, similar, plots) after the measurement period showed differences in the vegetation development for the whole period (P1, NEE chamber: 3.1g and P4, NEE chamber: 4.5g). Furthermore, the difference may be related to an increase in LAI during the measurement period, but this cannot be confirmed by measurements as we have LAI measurements only from the biomass survey. Thus we decided to work with a constant, average LAI and to adjust the simulations of IM to both periods, which unavoidably leads to an over-estimation of NEE for P1 and an underestimation of P4, but reflects the net ecosystem exchange for average vegetation conditions. We will explain the reasons for the difference in the text.

409 (p. 8877, line 19): We include the regression results for NEE and Reco in a summary table (see our answer to comments on 3.1) and we will provide the regression plots in a supplement.

### 3.4 Distribution of the assimilated carbon in Kobresia pastures and the soil:

General comment: see above

447-449 (p. 8879, line 6-8]: Allocation period is the time up to a steady state situation of  $^{13}\text{C}$  fixing, in our case 15 days (total trace period was 64 days in Kema and 27 days in Xinghai). We will make this more clear.

### 3.5 Influence of plant cover on convection and precipitation:

485 (p. 8879, line 15): We agree that rearranging the figure in accordance with the reviewer's suggestions will give a better impression of convective timing, and we will

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change the figure accordingly.

### 3.6 Simulation of different degradation states:

494 (p. 8881, line 1-2): This appendix had been removed, but this Figure reference had been forgotten. We delete the sentence: “The related mean diurnal cycles are given in Appendix B, Fig. B1.”

495 (p. 8881, line 3ff): Indeed we missed explaining our thoughts conclusively here. For a hypothetical transition from IM to BS, the model results suggest a decrease in mean ET, while the day-to-day variation increases (Figure 8). Compared to the overall large day-to-day variation, we regard the decrease in mean values as not significant. As well, the lysimeter measurements do not show any differences in mean ET between IM and BS (Figure 3). The large day-to-day variation of BS is a consequence of a missing turf layer, which would be able to store water over a longer time, the reduced stomatal control on transpiration, and missing connection to deeper soil layers via *Kobresia* roots. Therefore water, if available, is immediately evaporated over bare soil but such spots dry out earlier at the surface, leading to this larger variation. We rewrite as follows: “Evapotranspiration decreases from SIM to SBS in this model degradation experiment (Figure 9b), but this reduction is small compared to the overall day-to-day variability and is not supported by the lysimeter measurements (Figure 3). Therefore a change in mean ET due to degradation cannot be confirmed in this study. The day-to-day variability, however, increases from SIM to SBS. This is connected to a larger variability of simulated soil moisture in the uppermost layer, as the turf layer retains more water due to its higher field capacity and lower soil hydraulic conductivity, and the roots can extract water for transpiration from lower soil layers as well.”

Furthermore, we make a change in the abstract (p.8863, l.15-16), see above.

### 4. Discussions and conclusions:

We will rewrite the headline as “Discussion and Conclusions” and delete the first para-

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graph. We believe that the remaining three paragraphs are too short for a separate discussion chapter, because some discussions were already done during the presentation of the results.

We have included the bullet points to show which research is necessary to complete our research. This was impossible within the very limited measuring periods and data access, but it may be interesting especially for our Chinese colleagues. We will add some more words and some references to make our ideas better understandable.

Appendices:

As mentioned above, the appendices are necessary for describing the model performances on the Tibetan Plateau, which replace the planned eddy-covariance measurements. To read the paper the appendices are not necessary, but give the expert a lot of additional information. We did, however, shorten Appendix D and included a table in the result chapter.

673- 691 (p. 8885 line 22 to p. 8886 line 10) and 716-734 (p.8887, line 11 to p. 8888, line 3): Twenty intact soil-vegetation monoliths were sampled randomly at the “Kobresia pygmaea Research Station Kema” site (described in main text under 2.1) near the small village Kema. The sampling location is in the centre of the main distribution of *Kobresia pygmaea* on the Tibetan Plateau, and the sampled monoliths were dominated by *Kobresia pygmaea*. The main properties of the soil are described in the main text under 2.2 (Intact Root Mat).

The sampling took place in mid September 2012. The sampled monoliths had a diameter of 15 cm and a length of 20cm, and were inside Plexiglas tubes (like the microlysimeters described in the main text under 2.3.2). The above ground biomass was cut, and the samples packed in aluminium boxes for transfer to Germany. From October to December 2012 the monoliths were placed outside the greenhouses of the Experimental Botanical Garden in Göttingen. After this the samples were kept in a climate chamber for 101 days to conduct experiments with a diurnal regime for light

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(stepwise in five levels: 0, 210, 430, 680 and 970  $\mu\text{mol m}^{-2} \text{s}^{-1}$  PAR, with a total irradiation period of 13.5 hours), temperature (between 6 and 16.5 °C) and relative humidity (between 85 and 45%). The samples underwent different wetting regimes/ irrigation levels (medium irrigation: 2.1 mm d<sup>-1</sup>; intense irrigation: 2.4 mm d<sup>-1</sup>). At the beginning of May 2013 the above ground biomass was harvested, and root samples were taken. Afterwards the monoliths were placed outside to let the leaves regrow. One week before the gas exchange measurements started, the monoliths were transferred back to a climate chamber with a constant temperature setting of 15 °C, and a total irradiation period of 14 hours. For the gas exchange measurements performed in mid June 2013, one of the intense irrigated samples was chosen for the derivation of an individual leaf gas exchange parameter set for use in the model adaptations described in appendix C2. The diurnal and wetting regime in the climate chambers was adapted to mimic hydro-meteorological conditions at the field site on the Tibetan Plateau.

We will add in the text: "...soil monolithes including turf and plants directly from Kema, regrowth/recovery of the plants in Göttingen..."

Review p. C3448, first paragraph: Model adaptation strategy: We agree that there are many methods available for optimising the parameter space to yield a best fit for any target variable. But in our opinion, this would be very complicated for our case, and does not necessarily yield useful results. Our model investigation tries to discover differences between changing land surface conditions, and therefore we have to believe that the physics implemented is working in a realistic manner. Consequently our strategy is to estimate as many parameters as possible from the field or with laboratory experiments. Different land cover types should then be simulated with the same parameter values except the determinative ones. For such an approach, a large bias is likely to occur, as laboratory plants do not really behave the same as those outside, and some parameters have to be regarded as "effective" parameters. We believe that a single factor, modifying a set of related parameters is a good way to remove bias and keep the physics realistic. Our results show that the SVAT-CN model simulated NEE

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with acceptable accuracy in 2010 and 2012, while the daily values of GEE and Reco were comparable with the measurements, too. We will make an additional remark in Chapter 2.5.

Review p. C3448, second paragraph: Table C2: The model in its current form was successfully applied for forest sites in Falge et al. (2003). For the 2003 study, the equations with originally unitless scaling parameters were converted to equations using scaling parameters with units. This was done to enable a better comparability with parameters generally used and shared in the project EUROFLUX. The model and the units of the parameters applied in the current study correspond to those used in Falge et al. (2003). We also give this reference in the title of the table. The equations are also published in Wohlfahrt et al. (1998).

765-767 (p. 8891, line 8-10). We will reformulate accordingly.

References:

Biermann, T., Babel, W., Ma, W., Chen, X., Thiem, E., Ma, Y., and Foken, T.: Turbulent flux observations and modelling over a shallow lake and a wet grassland in the Nam Co basin, Tibetan Plateau, *Theor. Appl. Climat.*, 116, 301-316, 2014.

Charuchittipan, D., Babel, W., Mauder, M., Leps, J.-P., and Foken, T.: Extension of the averaging time of the eddy-covariance measurement and its effect on the energy balance closure *Boundary-Layer Meteorol.*, online first, 10.1007/s10546-014-9922-6, 2014.

Falge, E., Tenhunen, J., Aubinet, M., Bernhofer, C., Clement, R., Granier, A., Kowalski, A., Moors, E., Pilegaard, K., Rannik, Ü., and Rebmann, C.: A model-based study of carbon fluxes at ten European forest sites, in: *Fluxes of Carbon, Water and Energy of European Forests*, Ecological Studies Series 163, edited by: Valentini, R., Springer, Berlin, Heidelberg, 151-177, 2003.

Riederer, M., Serafimovich, A., and Foken, T.: Eddy covariance – chamber flux dif-

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Wohlfahrt, G., Bahn, M., Horak, I., Tappeiner, U., and Cernusca, A.: A nitrogen sensitive model of leaf carbon dioxide and water vapour gas exchange: application to 13 key species from differently managed mountain grassland ecosystems, Ecol. Mod., 113, 179-199, [http://dx.doi.org/10.1016/S0304-3800\(98\)00143-4](http://dx.doi.org/10.1016/S0304-3800(98)00143-4), 1998.

Zhou, D., Eigenmann, R., Babel, W., Foken, T., and Ma, Y.: Study of near-ground free convection conditions at Nam Co station on the Tibetan Plateau, Theor. Appl. Climat., 105, 217-228, 2011.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C4119/2014/bgd-11-C4119-2014-supplement.pdf>

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

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Table: Comparison of the models SEWAB and SVAT-CN against Eddy-covariance and chamber measurements

	comparison	Class	Variable	Unit	r <sup>2</sup>	slope	offset	n
Nam Co 2009	EC vs. SEWAB	AS	30-min ET	mm d <sup>-1</sup>	0.74	1.10	-0.50	572 <sup>*)</sup>
	EC vs. SVAT-CN	AS	median NEE <sup>*)</sup>	gCm <sup>-2</sup> d <sup>-1</sup>	0.90	1.15	-0.15	24
Kema 2010	EC vs. SEWAB	Refic	30-min ET	mm d <sup>-1</sup>	0.72	1.03	-0.28	577
	EC vs. SVAT-CN	Refic	median NEE <sup>*)</sup>	gCm <sup>-2</sup> d <sup>-1</sup>	0.81	0.99	-0.02	24
Kema 2012	Chamber vs. SVAT-CN	IM (P1+4)	30-min NEE	gCm <sup>-2</sup> d <sup>-1</sup>	0.86	0.80	-0.89	537
		DM	30-min NEE	gCm <sup>-2</sup> d <sup>-1</sup>	0.74	0.85	0.24	363
		BS	30-min NEE	gCm <sup>-2</sup> d <sup>-1</sup>	0.48	1.77	-0.38	195

<sup>\*)</sup> Already published by Biermann et al. (2014), offset recalculated in mm d<sup>-1</sup>

<sup>\*)</sup> Hourly medians from an ensemble diurnal cycle over the entire period

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Fig. 1. New Table