Hydrol. Earth Syst. Sci. Discuss., 9, C5347–C5361, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C5347/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "New climate change scenarios reveal uncertain future for Central Asian glaciers" *by* A. F. Lutz et al.

Anonymous Referee #1

Received and published: 22 November 2012

SUMMARY ——-

The paper presents a new methodology for computing glacier evolution at basin scale by using a sub-grid parameterization and volume-area scaling. Future scenarios for glacier evolution in the Amu Darya and Syr Darya basins (Central Asia) and the period 2010-2050 are carried out by forcing the model with both, CMIP4 and CMIP5 scenarios. In the current form, the paper is well organized and well written, but major problems subsist in the presentation of both the methodology and the results. The non-introduction of some variables which are used all of a sudden and the inconsistency in some part of the description makes the reading somewhat frustrating at times. Moreover, the concept of "uncertainty" needs apparently to be re-thought – the state-

ment that assuming a given climatic scenario, the “estimated error in glacier extent” by 2050 is estimated as low as 4.1% appears a bit too optimistic (an euphemism for saying ridiculous, since that is even better as what the total glacierized area is known today. . .). I firmly believe that the ideas presented in this publication are both valid and of real interest, but at the current stage the description of them is certainly not ready for publication. I would encourage the authors to resubmit the paper after major revisions.

GENERAL COMMENTS ————————-

1) Simple things first: (a) If a variable, an abbreviation or an acronym is used, it has to be defined! And this has to happen in the text when it first appears, and not somewhere in the caption of a figure. (b) Before a paper is submitted, it is well worth to verify that all figures are referenced in the text at least once, and that the references are correct. (c) If the text points at a table for some particular value, this table should of course contain it. (d) If a value given in the text is shown in a figure as well, the two should be consistent.

2) The methodology is insufficiently described, and at stages, the description appears contradictory. The most difficult thing to understand is at which scale the methodology finally operates: At a 1km grid-cell scale, or at the scale of basin average? Moreover, some criticism can be made from the conceptual point of view: Updating glacier area at monthly scale based on volume-area scaling and mass balance seems not very suitable, especially for small glaciers, since seasonal signal of the mass balance are then directly transferred to the glacier area.

3) The understanding of “uncertainty” needs to be revisited. Currently it is mostly used for indicating the spread between results when the model is forced with different inputs. This assumes that (a) the model structure is perfectly capable of mimicking reality (which is certainly not the case), (b) the estimated parameter set is absolutely correct and constant in time (which is probably not the case either), and (c) the simplified climate scenarios reflect the future evolution of climate. . . The effort for assessing

HESSD

9, C5347–C5361, 2012

parameter uncertainty presented in Section 4.3. is intriguing, but, apparently, doesn't plays any role when the results are presented.

4) The current "discussion" section is very "prosaic" and of rather low information density. It can definitively be condensed in the current content and maybe expanded with some topics which are currently missing – as the meaning of "uncertainty" for example. . .

5) Some sentences in the "conclusions" need to be removed as they either belong to the introduction or make claims about topics never analyzed in the manuscript. The conclusions should recapitulate the presented work. It is not the right place for speculations.

6) A general stylistic suggestion is to make the formulations of some sentences more "timeless". As an example, the formulation "The latest climate change projections [. . .] generated for the upcoming fifth assessment report [. . .]" in the abstract, will be outdated at latest in one years' time...

SPECIFIC COMMENTS ————————

P 12692, L 13-15. Please revisit the sentence: (a) "estimate changes in glacier extent as a function of glacier size" is a pleonasm and (b) a "glacier mass balance model" per se is not sufficient for "estimate[ing] changes in glacier extent" – for doing this some sort of glacier dynamic model (or a parametrization thereof) is necessary.

P 12693, L 21. Maybe only a detail but I would avoid the wording "melt-water dominated rivers" when referring to Amu- and Syr Dary. Whilst it is well known that this rivers are (strongly) influenced by snow- and icemelt, no study exists so far to my knowledge which actually shows that the meltwater share is larger than 50%. In that sense I would prefer the wording "melt-water influenced rivers", that seems more "cautious" to me.

P 12694, L 10. State a year to which the "present total glacierized area" refers to. It is

HESSD

9, C5347–C5361, 2012

not 2012, is it?

P 12695, L 4-5. Calling a 10-year period a “climatic reference” is not admissible. “Climate” is defined over a period of at least 30 years. Changing the wording would be sufficient in principle, but the statement suggests that the interpretation of the results will be done against this reference! Although there is no argument for not doing so in principle,it is very misleading (and not correct) to “sell” this comparison as an assessment of “climate change”!! Please re-think on the interpretation of the results!

P 12695, L6. Please give more information on the “PERSIANN dataset” (since I’ve never come across that): Who collects this data? Who provides them? What do they contain? Where are they retrievable? Are they of public access?

P 12695, L 6-8. This is confusing: So “PERSIANN” is not a dataset (as claimed in the previous sentence) but a “neural network”? Please be consistent in the naming, and give the information requested above.

P 12696, L 6-9. These sentences are unclear, and further explanations are urgently required. What is the aim of the “sampling”? And what is “sampled” at all? What is happening after the “weighted percentiles according to the inverse number of simulations per scenario” have been computed? And why is the weighting important at all?

P 12696, L 16. Please change the wording: The range stated in the sentence (i.e. 1.3 to 2.4 C) is not an “uncertainty” in the temperature projections, it is only the spread of individual model runs!

P 12696, L 13 - P 12697, L 2. Somewhere you need to state that all the figures presented here refer to annual values! It may be worth giving a short overview on how the changes spread across the year, since in the literature it has been pointed out several times that changes in both temperature and precipitation may differ throughout the individual seasons. In particular it would be interesting in this respect to have a comparison between CMIP3 and CMIP5 projections.

HESSD

9, C5347–C5361, 2012

P 12697, L 5-10. I'm not completely sure but hasn't this updated version of the GLIMS data been included in the Randolph Glacier Inventory RGI (Arendt et al., 2012),? And hasn't the RGI improved upon the Digital Chart of the World (DCW)for the region of interest? I thought so... And by the way: If the DCW was used also by Raup et al. (2007), the figures of glacier area given at P 12694, L 10-12 definitively do not refer to "present"...

P 12697, L 21-22. Well, I wouldn't call 5 glaciers "several" (and also "bench mark" should be removed: that's just what's available; it hasn't been chosen by any particular criteria - beside accessibility ;-)). Please state "five". Moreover, in the text, the mass balance is stated to refer to the period 2001-2010 whereas in Table 2 it refers to 1991-2010. Be consistent (and if you correct the Table to be consistent with the chosen "reference period", recompute the values from the original data!).

P 12698, L 12. I'm not sure if the statement "We repeat the reference period four times" (remove "(Sec. 3.2)", it is distracting at this point) is comprehensible for someone not dealing with "delta-change" methods. Maybe you can think of another formulation?

P 12698, L 18-20. This statement is very important, and should be highlighted previously. Better, however, would be to change the approach: Reducing the information of the CMIP3 and CMIP5 model runs to linear, annual delta change seems very "rude" since it neither exploits the available information adequately nor it is necessary. Unless you have a very strong argument for not doing it (and if so, you should state it) I urge you to follow an approach along the lines of what proposed by Bosshard et al., HESS, 2011, i.e. computing delta-changes on a daily scale. Moreover, it would be similarly easy to avoid the need of linearly interpolating the changes over a given period, since in principle, "deltas" can be computed for any 30-year period. The idea of computing the "deltas" in a running window of 30-years seems then almost obvious, and should therefore be done.

P 12699, L 9. The "distribution of elevation" of what? I guess the elevation distribution

HESSD

9, C5347–C5361, 2012

of the glacierized surfaces within a 1km grid cell? Please state it clearly!

P 12699, L 10. What is F_G?!? I first thought the elevation distribution of the glacier, then the area of the glacier, and eventually, I found that it is meant to be the “fractional glacier cover”. Please introduce this variable adequately.

P 12699, L 11-12. This is very unclear to me. In particular, the statement “assuming that [. . .] the glacier distribution is proportional to the elevation distribution and glaciers occupy the highest (coldest) end of the elevation distribution” seems contradictory to me. In the first part, it sounds like the elevation distribution of the glaciers is simply obtained by multiplying the elevation distribution of the whole grid cell with the according fraction of glacierized area assigned to the grid-cell. In the second however, it seems that the area is just “filled” from top to bottom. As an example, let say the considered 1km-grid-cell is a inclined plane (i.e. the distribution of area with elevation is uniform) and the glacierized fraction is 0.5; would every elevation have half of the area glacier covered and the other half not, or would you assign “totally glacierized area” for the elevations above the mean elevation and “glacier free area” to the elevations below that (or even something else?)? I personally prefer the first option. . .

P 12699, L 13-20. Ok, I probably got the rough idea, but the way it is formulated is not sufficiently clear, and I even believe the stated Eq.1 being not correct. Beside the fact that the chosen symbols in Eq. (1) are prone to create confusion (once you use “F” as cumulative pdf, as often in statistics, and once for defining a fraction. . .), what happens when H_GLAC is outside the range of the considered grid cell? This is going to happen very often (in particular if one considers that F_nˆ-1(etc.) becomes –inf and +inf for F_G equal 0 and 1, respectively), isn’t it? And what is for F_G=0.5? Isn’t what you want in this case H_GLAC=H_AVG? But in the way Eq.1 is formulated, F_G=0.5 would give H_GLAC=H_AVG+0.67*H_SD (since [in R notation] qnorm(1-0.5/2)=qnorm(0.75)=0.67), and that’s probably not what you want. . .

P 12699, L 18-20: I’m not sure to understand this statement either: Does it means, that

HESSD

9, C5347–C5361, 2012

you assume one and the same “hypsometric curve” for all basins? I.e. do every 1 km grid-cell with elevation 4500m have a glacierized area of 45% (that’s what I would say according to Fig. 7)? Probably not, in the sense that you certainly first discern between cells containing glaciers and not according to the glacier outlines, don’t you? If you do so: State it. If you don’t: you should!

P 12699, L 22. Better refer to Fig.4 than to Sect.3.4.

P 12699, L 23 - P 12700, L 2. This is inconsistent with previous definitions: at P12699 L6 you stated that H_GLAC is the “median glacier elevation in a 1 km grid cell”, now you say that T_GLAC is “the representative air temperature for the mean elevation of the glacierized fraction of a 1 km grid cell”, implying that H_GLAC is the mean elevation of the glacierized fraction. . . Choose one definition and stick to it. Moreover, state why Eq.(2) is required! Why is using T_AVG not sufficient? And perhaps more importantly: Why do you not compute T_GLAC directly from the station data, as you say you do with T_AVG (P12695 L11ff)?

P 12700, L 3-11. Sorry, what? What is the averaging over the two basins good for? At this stage, it is just not comprehensible. Please give a hint of what you aim to do before you actually do it – it’s a lot easier to understand then!

P 12700, L 12-14. Please state that, if everything you do is correct, using” \overline{T}_GLAC and \overline{H}_GLAC” or “\overline{T}_AVG and \overline{H}_AVG “ makes no difference (actually you could even use the average of the station measurements and altitudes. . .).

P 12700, L 15-20. Honestly I don’t see the need of introducing the concept of AAR: Why you don’t simply state that accumulation and ablation area are discerned through H0 (accumulation area = glacierized areas abowe H0, and viceversa)? That’s all you do, isn’t it?

P 12701, L 5-7. What do you mean by “composite DDF that includes the relative

HESSD

9, C5347–C5361, 2012

proportions of debris [. . .] covered glaciers"?? Does it mean that you have two distinct DDF for debris-covered and debris-free glaciers, and that the DDF you are actually using is a (weighted?) mean of the two? More information is required! And remove either "relative" or "proportion", these are synonyms.

P 12701, L 8. No! Positive degree days (which I firmly believe you mean with d_m, and if not, do so!!) are not the "NUMBER of days [. . .] with T>0C"!! It is the SUM of degrees for T>0, which is very different!

P 12701, L 15. Does this imply that the precipitation is uniform over the whole area? If not, what do you mean by "monthly precipitation"? The sum over the whole region? Why do you not use the sum of precipitation for altitudes above H_0, since at P12695 L7 you claim that you have a spatially distributed precipitation field?

P 12701, L 18-19. Where are the coefficients in Eq. (8) coming from? In the publication you cite (Bahr et al., 1997) only an exponent is stated (and it is 1.36, not 1.375), but no factor (that what in Eq. (8) reads 0.12) . However, in Bahr et al. (1997), a reference to Macheret et al. (1988), reporting an exponent of 1.379 for some 103 glaciers in the Altai and Tien Shan mountain ranges is given. Maybe you are referring to that value? Since the publication is in Russian and not retrievable to me, still the question remains where "0.12" is coming from? Moreover, according to your coefficients, the units of V and A should be kmˆ3 and kmˆ2 respectively, and not mˆ3 and mˆ2. . .

P 12701, L 19-22. No! Please do not use volume-area scaling at monthly scale! In this way you end up by having shrinking glaciers during summer, and growing glaciers during winter, and that considering area! That is not what you want, do you? Especially for small glaciers this is absolute nonsense! If you stick to VA-scaling, please do it on a year by year basis (more fancy ideas, like removing seasonality from the monthly time series, seem not worth of pursuit to me. . .).

P 12701, L 23. This sentence should be placed somewhere before introducing VA-scaling. And state explicitly that the monthly mass balance is computed by C_m-A_m.

HESSD

9, C5347–C5361, 2012

P 12702, L 2-4. I don't understand this one... Why do you correct the temperature and not DDF? The whole idea behind temperature-index method is to calibrate these coefficients! My guess that your argument is, that DDF was calibrated previously for individual glaciers (that's what you stated at P12701 L8-9), but remember that in general, factors calibrated for a given glacier and a given resolution, are not transferrable neither in space nor in scale. All kind of other uncertainties which you claim are offset by correcting the temperature (L6-10), could be offset by DDF as well (keyword "equifinality"...). And by the way: Why do you call CorT a "parameter" at L 6? Is it a factor used for multiplying the temperature or something like that? Give an explanation how the correction is implemented.

P 12702, L 1. Later in the text it becomes clear (make it clear at this stage already!) that only the average mass balance for the available period is used for calibration. Why? Why do you "waste" the time series (by considering the average only) of records, if they are available?

P 12702, L 10-13. Well, this figure looks not convincing to me at all: The AAR reported in Bahr et al, 1997 (actually a cross reference to Bahr, JG, 1997) refers to the 1980s-1990s, and is about 10% smaller than what your model says! Neither is a 10% difference in AAR a "good match" (actually that is more like a pretty massive difference, considering that you would never expect to find AARs above 60-70% for large regions!), nor would you expect an increase in AAR by the time you are referring to! Please reformulate the sentence!

P 12702, L 19. There is no change in glacier area stated in Table 2! Did you intend to refer to any of the figures?

P 12702, L 20-21. Now I'm really confused: Was your approach not operating at 1km resolution already? That's what I understood at P12697 L17-20, P12699 L10-15, or P12699 L23-P12700-L1! What do you mean then at P12698 L5 when stating "[...] the 5 glacier model (operating on the 1 km scale)"? I urge you to revisit the whole

HESSD

9, C5347–C5361, 2012

"Methods" chapter for improving the clarity!

P 12702, L 23. Again, there is no data referring to 2011-2050 in Table 2 (and please check your references when submitting something the next time. . .).

P 12702, L 26. Concerning "assuming that the glacier distribution is proportional to the distribution of elevation": Similar comment as for P 12699, L 11-12.

P 12703, L 1-4. And what are c1 and c2? Moreover, give a reference for Eq (9), and give a hint for the need of a parameterization (in contrast to the exact solution).

P 12703, L 5-6. Has the ordinate in Fig. 9 been accidentally reversed? F_G is told to be the fractional glacier coverage, which for me means "F_G=1" = "total glacier cover". Why should a grid cell with "mean elevation = 4000m "have F_G=1 for H_GT=3400 and F_G=0 for H_GT=4800? I would expect exactly the opposite. . .

P 12703, L 9-11. How can these values be read out of Fig. 10? According to Fig.10a and 10b, I would say that the three curves look almost identical in both panels, meaning that "P-30%, T+2" is equivalent to "P+0%, T+3", "P+0%, T+2" is equivalent to "P+0%, T+2", and "P+30%, T+2" is equivalent to "P+0%, T+1". It is unclear to me how one can deduce the equivalence between P+20% and T+1. . . And by the way, place "Fig.10" before "panel A and B".

P 12703, L 19+20. Add "Fig.10" before both "panel C" and "Panel D".

P 12703, L 20-24. The aim of this observation is not very clear to me. Is it meant to provide a rationale for testing the sensitivity of DDF? If so, the albedo-argument is probably not the most important one: DDFs vary from glacier to glacier because of local meteorological and topographical conditions more than they vary because of albedo. . .

P 12704, L 6. And what are DDF_CI and DDF_DC?? You never introduced them!

P 12704, L 8.Rempve parenthesis before "." (it doesn't close anything).

P 12704, L 12-15. I'm not convinced that I have understood the general idea: Your

HESSD

9, C5347–C5361, 2012

glacier response model (i.e. the VA-scaling kind of update) is driven by mass balance alone, isn't it? So why do you "use" a glacier mass balance (same page, L 12)? If you force the cumulative mass balance to be the same for different model runs, I very much would expect that at the end of the simulation, you would get the same variations in glacier volume. In any case you should get it if you approach is mass conserving, isn't it? Please explain what causes the model runs to have different trajectories.

P 12704, L 15-19. Please add a Figure showing the results of this experiment!

P 12704, L 19-20. I don't completely agree with this sentence: The analysis carried out in this section gives a hint of the spread model parameter can have when still leading to the same result. Since the result itself is fixed a priori (again according to L12), the experiment doesn't add information on the "uncertainty" of the model simulation. In my opinion, the only way to tell about model uncertainty in this case, would be in a "classical" calibration-validation kind of scheme: Calibrate the model for a particular time period, compute the results for a second time period, and compare them to measurements you believe in. I'm aware that the available measurements are not very suitable (the only things you have are the mass balance series for some sparse glacier), but then, the only "honest" claim you can make is that you do not have the means of assessing the uncertainty. . .

P 12704, L 25 - P 12705, L 4. I'm not sure what Fig. 11 is actually showing: Is each point in the figure the result of only one particular model run, in which a particular delta change in P and T has been assumed? Where does the uncertainty assessment of Sec 4.3 enters the game? Is it considered at all? And what kind of quantlies are used for the delta change? Are they derived by lumping all emission scenarios shown in Fig.2 and 3 (and discerning between CMIP3 and CMIP5)? And please state absolute numbers (and not quantiles) for the delta changes in Fig. 11: It is impossible to reconstruct them from Fig. 2 and 3!

P 12704, L 5-6. Please check the numbers (and do this for the whole manuscript!): Ac-

HESSD

9, C5347–C5361, 2012

cording to Fig.11, I would say that for delta_T=q50 and delta_P=q10 the color states the range 46-48%, whereas in the text you say42%), and for delta_T=q50 and delta_P=q90 the result must be around 40% (and not 37% as in the text). . .

P 12704, L 14-16. Same comment as for P 12696, L 16: This is not an uncertainty range!

P 12705, L 20. What's "AR5"? Of course the "upcoming IPCC report", but you never defined it!

P 12705, L 20-21. Ok, let's try this one: WHEN YOU TALK ABOUT SOMETHING YOU SHOULD DEFINE IT FIRST! Probably the "median case" is delta_T=q50 and delta_P=q50, right? But what is the "dry and warm case"?? Delta_T=75 and delta_P=q25? Delta_T=90 and delta_P=10?Or delta_T=97.5 and delta_p=X? It could just be anything!

P 12705, L 8 - P 12706, L 3. I don't' believe that you believe the stated numbers being realistic: Event glacier area mapped from satellite imagery are commonly assumed to be precise at some 5% only. No-one will ever believe that with a very simplified approach as proposed the glacier extent in 40years time can be predicted with a "estimated error in glacier extent" of 4.1%. Please re-think what "error in glacier extent" should include (uncertainty in present glacier extent, including glacier volume, uncertainty in climate evolution, uncertainty in the data used for model calibration, including meteorological and mass-balance time series , problems of extrapolating calibrated parameters over the whole region, capability of the models of actually mimic reality, in light of the simplifications, assumptions, hypotheses made therein, etc. . .), and discern it very clearly from the concept of "spread in model results".

P 12706, L 14. No. Ice flow models do not necessarily require "detailed knowledge of glacier velocities" – that's what they compute. Such datasets are rather very useful for validation. . .

HESSD

9, C5347–C5361, 2012

P 12706, L 25. Well, in the publication you name, an analytic parameterization is proposed, which could theoretically be used without any “time series of high resolution DEMs“ . . .

P 12707, L 25. Consider citing the recent published work by Huss and Farinotti, JGR, 2012 as well.

P 12706, L5 - P 12708, L13. In light of the rather minor density of information in the whole section, I would suggest to condense it. The text is well written at this stage, and almost reads like a “story” but it could be shortened significantly.

P 12708, L 15-18. Remove the first two statements: They belong to the introduction, and certainly not to the conclusions of the study, which did not covered this topics at all.

P 12708, L 23-27. Somewhere you need to state that these figures refer to the change 2010 to 2050!

P 12709, L 2-4. Why? Give an explanation.

P 12709, L 6. Remove “as well as in terms of downstream water availability” – the study did not address the topic.

STYLISTIC COMMENTS ————————

P 12692, L 15-16. Try to avoid “model [-s / -ing]” 4 times in a single statement.

P 12692, L 19. Remove”the” before and “projections” after “CIMIP3”

P 12692, L 25. Consider “glacier evolution” instead of “glacier extent” (since the wording was already used in the same sentence)

P 12693, L 3-4. Sort the references by year of publication.

P 12693, L 4-5. Consider ", the reason being the lack of. . .“ instead of “. The underlying reason of this ongoing debate is the lack of . . .“

HESSD

9, C5347–C5361, 2012

Interactive Comment

P 12694, L 2. Although the wording is “impressive”, I would remove the expression “geopolitically complex region”: It does not contain any information, as a definition of what “geopolitically complex” means is missing Merge the sentence with the following one, i.e. “The sources of the Amu Darya and Syr Darya rivers are located in the Pamir and Tien Shan mountains respectively . . .”.

P 12694, L 17-19. This statement can be removed.

P 12695, L 24: This sentence confirms that the wording “latest set of simulations” should be avoided: The data you used are one year old. I believe a couple of GCM-runs have been done in the meanwhile ;-)

P 12696, L 2. Insert “,” between “report” and “is also”.

P 12696, L 19. What is “it”? Certainly the 90% and 10% quantiles, as in the previous sentence, but that is plural. . .

P 12703, L 15-16. Consider "contribute substantially to the total ice volume in the basin“ instead of "contain substantial parts of the ice volume of the basin”

P 12704, L 7. “Gaussian deviates”? You probably mean “normally distributed (random) variables”. . .

P 12704, L 5. Remove either “extent” or “retreat” in “glacier extent retreat”.

P 12704, L 11-13. Please revisit you wording and be consistent in the use of adjectives: At P12702 L12 you claimed that a deviation in AAR by 10% is “a well match”, whereas now, a difference in total area by 5% is claimed to be “striking”. . .

P 12708, L 23. This statement (i.e. higher warming = larger range of projections) is not correct as such (e.g. a (hypothetical and absurd) projection of a+20C warming would certainly make all glaciers melt completely. The range of the projection would then be simply 0). Thus remove “thus” ;-)

COMMENTS TO FIGURES ——————-

HESSD

9, C5347–C5361, 2012

Fig. 2. Not sure if it is stated somewhere (but even if so, it may be worth repeating it in the caption): How are this values computed? Are they means of the GCM grid cells covering your region of interest? If so, how many grid-cells does it includes?

Fig. 3. Consider adding a vertical line for better dividing the CMIP4 and 5 scenarios.

Fig.4. It may be worth to state the actual number of glaciers in each bin on top of the individual bars.

Fig. 6. H_SD should be H_AVG+H_SD (or an arrow as for F_G is required between H_AVG and the current position of H_SD).

Fig. 8. This figure is never called in the text! Moreover: Label of left ordinate: "% of 2010", not "to". Label of right ordinate: remove "." after "m".

Fig. 9. See comment for P 12703, L 5-6.

Fig. 10. What do you mean with "change in glacier extent in 2050"? Why 2050? The panel show a time series. . . Moreover, what do you mean by "baseline" properties? And "ceteris paribus" is probably not very common to most of the readers- me included. . .

Fig. 11. Please state absolute numbers (and not quantiles) for the delta changes – it is impossible to reconstruct the values from Fig. 3!

Fig. 12. Please add labels at least to the ordinates of the bottom plots. For the upper plots, consider showing only an enlargement of one specific region. At the current scale of the plot, it is very hard to see something.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 12691, 2012.