We thank anonymous referee #2 for the constructive comments on our paper. Please find below the detailed reply.

Köplin et al.: Relating climate change signals and physiographic catchment properties to clustered hydrological response types, HESSD, 2012.

Anonymous Referee #2

General comment

The article presents an approach for the classification of catchments based on their hydro-climate response types to climate change using a cluster analysis technique. The authors further tried to find a causal relationship between the catchments' hydrological response and their physiographic attributes and climate change signals using a redundancy analysis, which they also used as a test for the robustness of their classification. The methodology is applied to a set of catchments in Switzerland. I find the authors' approach interesting and given their results that can be articulated in a physically meaningful way, I believe that their work deserves publication. I only have a few fairly minor comments, which I believe the authors can address:

Specific comments

Section 3.2: I understand that the authors classified the catchments based only on their hydro-climate responses and here they attempt only to find out if the resulting clusters can be characterised in terms of the physiographic attributes. Is that right? Then why leave out some of the attributes simply because they are correlated with some others? I think leaving out all soil parameters from the attributes, for instance, would lead to losing some important features that potentially explain some important runoff generation process. Why didn't the authors keep all the attributes?

Response: The referee is right, of course, that the soil parameters might explain important runoff generating processes. However, we focus on mean monthly total runoff, here, and do not investigate single runoff components like surface runoff, interflow or base flow, where soil parameters might play a more important role. Therefore, we think it is justified to exclude the soil parameters, here. We partly keep their information, though, by keeping the parameter slope, which functions as a proxy for the soil parameters (with increasing slope, soil depth and available field capacity decrease).

Page 3178, lines 2-5: It is not clear to me how the absence of a clear pattern with respect to dominant aspect and dominant land use is attributable to the parallel plot. How would the authors interpret the result if most of the curves behind each of the visible ones were of the same cluster?

Response: We did not express ourselves clearly, here. What we meant was: One cannot *see* a clear pattern (even if there was one), and only this fact we attributed to the parallel plot visualization. Actually, there are clear patterns for the mentioned two variables, thank you for pointing this out. We changed the respective passage in the text as follows: "*The clusters' modes for the nominally scaled variable dominant aspect also depict a clear pattern. Clusters C1 to C3, which are situated north of the Alpine ridge, are mostly north-exposed. Catchments in C5, which generally drain eastwards, are* *east-exposed (aspect class 2), accordingly. The other clusters are mainly west-exposed. For dominant land use, a correlation of the clusters' modes with the clusters' mean elevation can be observed. In C1 and C2, having a mean elevation of 1000 m a.s.l. and less, the dominant land use is pasture. C3 and C6 are mainly covered with subalpine meadow and are situated between 1500 and 2000 m a.s.l. For clusters situated above 2000 m a.s.l. on average (C4, C5 and C7) the dominant land use is rock.*"

Page 3178, last paragraph and Figure 5: the absolute magnitude of the runoff of C1 is very low with no/weak annual cycle compared to that of the other clusters. The runoff change signals in Figure 5 appear to be in absolute magnitude, which could potentially mask the relative magnitude of the change in C1. I think, it would be better if the authors presented the change signals as relative changes. This would reveal if C1 is really insensitive.

Response: The referee is right that a small absolute change could actually mask a large relative change. We decided to display absolute changes, here, because like this one can easily see that the substantial absolute changes in runoff cannot be attributed to the rather small precipitation changes but to the temperature signal, which is an important and interesting feature. We agree that the relative change is important, too, to decide whether the cluster is really sensitive or not. Therefore we integrated two additional panels in Figure 5 (new line 4 from top, see attached) showing the relative Q-change in per cent. The additional panels complement the previous and show that C1 is really insensitive both with respect to absolute and relative changes. Although the relative change in the summer of the far future is comparable to that of the other clusters, this change is still a small absolute deviance (-30 % but only -10 mm/month). Showing the Q-change in both relative and absolute numbers really helps to decide on the clusters' climate sensitivity. We thank the reviewer for this comment and modified the mentioned paragraph, accordingly.

I think the authors can use better discriminating colour codes for the different clusters in Figures 3-7. For instance, it is a bit difficult to distinguish between C4 and C6.

Response: We adjusted the colours in Figs. 3–7 and are confident they are now easier to discriminate.

Page 3186, lines 10-13: How does the regionalization procedure implemented in the work for the estimation of the runoff mitigate the problem associated with the stationarity problem of the model parameters? Aren't the same parameter sets that are used in the calibration period used under climate change scenarios too?

Response: The referee is right; we used the same parameter sets in the control and in the scenario period, and using seven different instead of one distinct set of parameters does not imply they are not prone to the stationarity problem. It is assumed, however, that the seven parameter sets reflect model parameter uncertainty. There is no formal proof of this assumption, of course, and we modified the text passage to indicate this more clearly:

"*Nevertheless, the calibrated model parameters are the crucial source of uncertainty associated with the hydrological model. As Merz et al. (2011) elaborated, calibrated model parameters are only valid for the period they were calibrated for, and '[…] care needs to be taken when using calibrated parameters for predictions of the future'. They refer to the stationarity problem, here, which is related* *to hydrological model parameters. We assume, however, that the employed regionalization procedure might mitigate the adverse effects of stationary model parameters: the simulated hydrograph is virtually detached from one distinct set of model parameters, or, in other words, the seven different parameter sets applied to regionalize the runoff reflect model parameter uncertainty.*”