Response to the reviewers' comments (2) on HESS-2009-180

Dear Dr. Hrachowitz,

Your detailed comments and suggestion are appreciated. Below are our responses. Look forward to your further suggestion.

Sincerely,

Huade Guan
On behalf of the co-authors.

Specific comments

1) Although the paper is in general well written, I would nevertheless encourage the authors to have it proof read by a native speaker as there seem to be several grammar and typing mistakes.

Discussion and actions
Thanks for your reminder. My co-authors are going through the revised manuscript carefully.

2) P.5853, L.9-10: rephrase "…breaking down…"

Discussion and actions
This is removed.

3) P.5854, L.1-2 and L.14-16: Maybe try to combine the two sentences into one as they somehow seem to say the same thing.

Discussion and actions
The two sentences are different in that the first one gives overall description, and the second one give detailed function of the bulk chloride deposition. In the revised version, we add another sentence saying that over a short distance, linear decay can be used to approximate BCD.

4) P.5854, L.27: Applicability of kriging not only depends on an appropriate sampling density but also on a sufficiently large sample size.

Discussion and actions
We agree. The sampling size is added to the statement.

5) P.5855, L.17-19: While the results suggest that the mentioned variables control chloride deposition, it might be worth toning the sentence down a bit, especially in the light of the relatively small sample size and associated relatively high p-values.

Discussion and actions
We agree. The part is re-written. The text is now read as "The results indicate that terrain slope and aspect (slope orientation), associated with prevailing wind direction, may influence BCD in the coastal area, but in a manner that is contrary to our starting hypothesis. The elevation does not significantly affect BCD. "

6) P.5856, L.7-8: Is this the long term mean annual precipitation or the annual precipitation during the observation periods? Perhaps include a reference.

Discussion and actions
It is long term mean annual precipitation. The reference is added now.

7) P.5856, L.10 and Fig.2: are these instantaneous measurements of wind direction or are these the dominant wind directions over e.g. 12 hours? Please specify!

Discussion and actions
The wind directions were measured at two time points daily, 9:00am and 3:00pm local time. It is stated in the caption of Fig. 2 and Fig. 9 now.

8) P.5856, L.14 and elsewhere in the manuscript (e.g. P.5862, L.26): I am not sure if the chloride concentration measured from bulk precipitation samples is really the "bulk precipitation chloride concentration". Would we not expect at least a proportion of dry deposition to end up in the rain samplers as well? I would thus suggest calling it "bulk chloride concentration" or even "bulk total chloride concentration" instead.

Discussion and actions
Thanks for pointing this out. The term "bulk chloride concentration" is now used throughout the paper. It means chloride concentration in the totalized rain samples which includes wet deposition during the event and dry deposition in the prevent dry interval.

9) P.5856, L.15-17: to increase sample size, the authors chose to include samples from two different observation periods. This is, in general for their purpose, not too problematic. However, I think it would be good to include an estimate for interannual variability in chloride deposition. I am aware that these data are obviously not available for the region of interest. Are there any estimates of interannual variability for regions not too far from the project region in South Australia available in literature?

Discussion and actions
We agree with you about issue of the inter-annual variability. A sampling period of 2 to 3 years is common to some other similar sampling work that have been done in Australia (Keywood et al., 1997, and Blackburn and McLeod,1983). We could not find a good dataset to access long-term inter-annual variability. Some other mapping works in Europe that we cited in the paper have similar sampling length.

10) P.5856, L.18-19: The authors mention that some chloride concentrations were obtained from multiple month cumulative rainfall samples. Except for the oil layer that does to a certain extent reduce evaporation, did the authors correlate the rainfall totals of their multiple month samples to higher resolution rainfall totals from nearby precipitation gauges to make sure that the evaporation losses are not significant?

Discussion and actions
No, we have not done this. The oil layer applied in the collectors follows a well tested procedure (Friedman et al., 1992), primarily for testing rain isotope composition. For chloride, as we measured chloride deposition, even if there is some evaporation (very unlikely), it would not affect our results. However, we agree with you, if possible, the test should be done to avoid some unaware inappropriate operation, if evaporation is important issue.

Friedman, I., Smith, G.I., Gleason, J.D., Warden, A. and Harris, J.M.: Stable isotope composition of waters in southeastern California. 1. Modern precipitation. J. Geophys. Res.-Atmos., 97(D5), 5795-5812, 1992.

11) P.5856, L.23-24: Please state the method, precision and possibly detection limits for the chloride analysis.

Discussion and actions
We agree. The method and uncertainty is added.

12) P.5856, L.26: Although not the focus of this paper, are nevertheless some crude estimates of dry deposition in the area available from literature? It would be good for illustrative reasons for the reader.

Discussion and actions
Apparently, we don't have dry deposition data yet. But we set up the first one in our area. Hopefully, we will be able to report the results soon.

13) P.5856, L.26: Was the wind speed mentioned here used in any of the subsequent analysis? If not, it can be removed. In any way, it would be interesting to see how wind speed might affect the distribution patterns of chloride.

Discussion and actions
Thanks for pointing this out. We actually didn't use the wind speed data. It is removed now.

14) P.5857, L.5-11: Although I appreciate detailed descriptions of methods, I think the Pearson product-moment correlation coefficient should be well known and the description could be shortened or left out.

Discussion and actions
We agree. This part is made concise.

15) P.5857, L.19: Should r not rather be rxy(z)?

Discussion and actions
It is revised as suggested.

16) P.5858, L.7: Should maybe read "geographic and orographic effects", as X and Y are not orographic effects per se?

Discussion and actions
We agree. It is rephrased as suggested.

17) P.5859, L.6: Not entirely clear how the de-trended residual map was produced. Should maybe read "…a de-trended residual map by kriging".

Discussion and actions
It is exactly right. Thanks. It is revised as suggested.

18) P.5859, L.12: General comment for kriging: 17 points seem relatively few for the generation of a variogram (cf. n>50, Burrough and McDonnell, 1998). It might be worth acknowledging this fact and highlighting that the limited sample size is likely to cause uncertainties in the kriging procedure.

Discussion and actions

We agree. The small sample size problem is now stated earlier in the introduction, following your previous suggestion.

19) P.5859, L.21: This reads a bit too definitive – consider toning it down a bit, e.g. "…both wet and dry chloride deposition in the study area tend to come from westerly direction."

Discussion and actions
We agree. It is revised as suggested.

20) P.5859, L.23-26: Sounds a bit speculative – maybe leave out.

Discussion and actions
It is revised as suggested.

21) P.5860, L.1-3: I am not convinced by excluding sites 16 and 17 which seems quite an arbitrary decision even if it is speculated that some short range effects might dominate at these sites.

Discussion and actions
The two sites are removed, based on the analysis of wind direction. At these two sites, local chloride source is from south, adding to the region chloride source from the west. The high measure chloride deposition at these two sites is unlikely due to the region source, as it far down wind from the west. It must be due to some local source. The wind direction data (Figure 2) show that dominant wind in this area is northerly. Thus, it is unlikely that this local chloride source affect BCD in the whole area. If they are included, the correlation and regression results will be disturbed by this local short-range chloride source.

22) P.5860, L. 17: maybe rephrase to "…show a highly significant ($p<0.01$?) relationship"

Discussion and actions
This part is rephrased.

23) P.5860, L.20: should read "…with a significance level of $p=0.04$"

Discussion and actions
Thanks. It is rephrased and incorporated with Goovaerts's comment on statistical testing.

24) P.5861, L.3: should read "…significant factors…"

Discussion and actions

In the correlation analysis, terrain aspect and slope is one term. We rephrase this sentence as "Terrain aspect and slope are the second significant factor, next to the coastal distance, for BCD in the study area, as indicated by the partial correlation between D and $\beta \sin \alpha$."

25) P.5861, L. 17-18: Sentence seems a bit awkward.

Discussion and actions
It is removed

26) P.5861, L.24-27: Sentence not entirely clear, please rephrase.

Discussion and actions
We agree. And this sentence and the rest of paragraph are rephrased. See response to your next comment.

27) P.5862, L.21-23: Not entirely clear which MAE the authors refer to here and what the difference is to the one mentioned at P.5861, L.27. Please rephrase sentence and provide a more clear explanation.

Discussion and actions
The paragraph now reads as follow. Note that the order of Figure 5 and Figure 6 is switched.

"After regression is performed, it can be used to construct the BCD regression map and ASOADeK map. To examine the mapping performance, cross validation was performed in comparison to the direct ordinary kriging. One example of cross-validation semivariogram model for direct kriging is shown in Fig. 5a. It is similar among 17 sets of cross-validation data. The semivariagram models for cross-validation residual sets are not shown, as they are different among the 15 sets. In comparison to direct kriging, both regression and ASOADeK estimates give a smaller mean absolute error (MAE), calculated from all cross-validation sets, and higher correlation coefficient between the estimates and observations (Fig. 6a). The MAE value of regression cross validation is 0.80 g/m^2, about 20% of average observation values over the first 15 locations in Table 1, and the MAE value of ASOADeK cross validation is 0.84 g/m^2, about 21% of the observation average. ASOADeK cross validation results slightly degrades in comparison to that of the regression, probably because the chloride network density is too low. The residual kriging is nevertheless applied because sites 16 and 17 were not included in the regression."

28) P.5862, L.29: Please indicate how precipitation was regionalized and what the approximate uncertainty is.

Discussion and actions
The precipitation mapping is previously published in Guan et al., 2009, which is cited in the paper. However, the uncertainty was not provided previously. The precipitation uncertainty is now calculated from monthly precipitation map, it on average throughout the study area, the annual precipitation mapping uncertainty is 2% at 90% confidence level. This uncertainty number is now added to the text.

29) P.5863, L.23-25: Has this effect been observed previously? If so, please provide references.

Discussion and actions
We are not aware that this work has been done. Recently, we have done additional sampling, and will report the results shortly.

30) P.5864, L.3: Maybe more useful to provide p-value instead of r.

Discussion and actions
Considering Goovaerts' comment of the problem of spatial dependence, we decide not to use p values in the discussion.

31) P.5853, L.2: Regarding the application of chloride as an environmental tracer it might be worth including some more recent references, e.g.
Hrachowitz M, Soulsby C, Tetzlaff D, Dawson JJC, Malcolm IA. 2009b. Regionalization of transit time estimates in montane catchments by integrating landscape controls, Water Resources Research 45, W05421, doi:10.1029/2008WR007496.
Shaw, S.B., Harpold, A.A., Taylor, J.C., Walter, T.M., 2008. Investigating a high resolution, stream chloride time series from the Biscuit Brook catchment, Catskills, NY. Journal of Hydrology 348:245-256.
Dunn, S.M., Bacon, J.M., 2008. Assessing the value of Cl- and δO18 data in modeling the hydrological behaviour of a small upland catchment in north-east Scotland. Hydrology Research 39:337-358.

Discussion and actions
Thanks for providing the references. Two of them are now cited.

32) Table 1: It would be useful for the reader to include elevation, slope, aspect, distance to coast and precipitation for the individual observation points

Discussion and actions
This is a good idea. They are now in Table 1.

33) Table 2: To make the correlation matrix easier readable, please mark significant correlations, e.g. with asterisk and indicate significance level in caption

Discussion and actions
We agree. It is done.

34) Figure 1: It might be worth including an outline of Australia with the approximate location of the project region. Furthermore, I think it would be good to use different colour schemes for elevation and precipitation (possible both on a graded scale, so that the map can also be easily read in black and white print outs). Also, please include the Site numbers or IDs for the chloride observation sites and highlight the 4 wind observation sites used in Figure 2.

Discussion and actions
Thanks for the suggestion. It is done.

35) Figure 3: Please include Site numbers or IDs and the p-value

Discussion and actions
We agree. It is done.

36) Figure 7: Although the individual figures are quite small, would it be possible to include the chloride observation sites? Maybe in at least on figure, e.g. in the uncertainty plot (7d).

Discussion and actions
Thanks. It is done.