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Interactive comment on "Estimating spatial distribution of daily snow depth with kriging methods: combination of MODIS snow cover area data and ground-based observations" *by* C. L. Huang et al.

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The manuscript tackles an issue of large interest, i.e. the spatial estimation of snow depth within a large area, starting from point site measurements, largely debated within the present literature. However, there are some issue with the manuscript, that in my opinion make it difficult to publish as it is, and require thorough consideration, as follows

1) The authors deal with estimation of snow depth. Worldwide indeed the large amount of available studies do investigate snow water equivalent SWE, i.e. the amount of wa-

ter within snow. This has several reasons, most notably that i) SWE is more useful for water resources assessment, ii) SWE is conservative (unless for the melting season, where however also snow depth goes to zero), while snow depth is not, and indeed the latter changes with compaction, and accumulation of subsequent snowfalls, iii) snow depth does not provide any indication of water availability, or even of snow related risk (e.g. avalanches), unless knowledge is provided of snow mass density rs, iv) snow depth may vary largely locally independently of altitude, exposition, etc. . .so making correlation analysis against topographic variables less sense than for SWE. Accordingly, the authors should deal with SWE, or at least provide indication of rs in the study sites.

2) Snow depth (and density, and hence SWE) estimation always entail an attached accuracy measure. In Kriging this should be taken into account (e.g. Carrol, 1995). Also, rs assessment carries a bearing upon SWE assessment even at measured points, so falling out upon SWE interpolated estimates. This needs to be taken into account duly.

3) The reference section is quite poor. I provide below several recent papers that were overlooked by the authors, some of which I think are relevant for the matter of snow kriging, but many more can be found. I have a feeling that the lack of literature analysis is a possible reason for the improper approach as reported in point 1 and 2 above, and generally for lack of accuracy about kriging procedure in the manuscript

4) In the merit of the manuscript, one can find some unexplained choices, and methods, as below:

4.a) In Section 2, it looks uneven the choice of 46 stations for set up, and only 4 stations for validation.

4.b) In Section 2.1. what do you mean " the coarse-resolution snow depth product were used as auxiliary 25 data to produce cloud-free MODIS snow cover area data". Estimated snow depth at 25 km resolution are likely much coarser than MODIS image, and likely not comparable to point site snow depths. Further, I would expect that

such estimates entail large inaccuracies. Some (multi-scale) data assimilation scheme should probably be used to provide optimal estimation constrained upon ground data, and indication of accuracy.

4.c) This is unclear: “The AMSR-E derived snow depth data and MODIS image fusion are used to remove clouds in northern Xinjiang snow products and to identify areas without snow“. What do you mean ? What are the “northern Xinjiang snow products” ? How do you identify area without snow ? I guess using MODIS images given that AMSR-E are much coarser, and frankly not comparable ? What is the use of AMSR-E ?

4.d) Section 3.1.1. Why do you pursue ordinary Kriging ? In your definition of z as “z at any location can be written as the sum of a deterministic component called the trend, m(x), and a stochastic error component, r(x)” it seems that m(x) is a sort of “average value” possibly depending upon altitude, with r(x) a (yearly, daily ?) fluctuation. In ordinary kriging you apparently Krige a stationary field, i.e. one without (altitude ?) drift. This seems unlikely, because we already know that snow depth (and, better, SWE) depends upon altitude. Why bother doing so ? On top of all this, Kriging in my understanding would require normal distribution (of either z(x) if stationary field, or r(x), e.g. Carrol and Cressie, 1997), also necessary for proper assessment of estimation accuracy. Did you test this here ?

4.e) I do not see the difference between UK, and OCK, you basically introduce altitude drift in both methods. Co-Kriging would be ideally used to assess the dependence of snow depth (better, SWE) against topographic features (altitude, aspect, slope, etc..), however maybe after correlation analysis (e.g. Carrol and Cressie, 1997). Here, you entirely neglect these facets, so losing the potential benefit of co-kriging. The same holds for UCK, not clear how you account twice for altitude, and not for other potentially important variables.

4.f) The kriging weights λ mirror the correlation structure of the snow field, which in

turn depends upon topography (e.g. Bocchiola and Groppelli, 2010), and it is a very important issue for understanding, say, how to implement measurement network to maximize accuracy via correlation. Here you entirely skipped this issue.

4.g) The investigated region is quite large. Are you sure that the correlation structure, and properties of the snow field hold all over the chosen region ? Wouldn’t be necessary a preliminary regional assessment (e.g. Bocchiola, 2010) ?

4.h) The kriging procedure provides by construction an estimation of accuracy (bias, ideally null, variance of error, minimum as per BLUE estimation, see e.g. Carrol and Cressie, 1997). So Bias and RMSE in Eq. (9) and (8) are not necessary. Instead you should verify whether the back estimation error (in cross validation) reflect their theoretical value from kriging. This is to demonstrate the hypothesis, and procedure of kriging is correct, unbiased, and with least variance. Further, you have to demonstrate that kriging in your case is better than other methods, say e.g. linear interpolation or simple averaging (of depth values, or fluctuations). You should test this.

4.i) Not clear why in Section 3.3 you use “virtual stations” ? You use zero snow value for interpolation ? Does this fit with the kriging methodology ? How do you select the number of virtual stations ? The higher the number, the lower the snow depth estimation in snow covered pixels. However, wherever you have no snow cover according to MODIS, you do not need to estimate snow depth ? Also, if you use zero values from MODIS in kriging, you are introducing an estimate with different accuracy than the ground stations one, which brings back to the issue of kriging estimation with accuracy.

4.j) Not clear why you use independent sites in Section 4.3. You already carried cross validation, so this seems not necessary. If this is done to test the procedure on a group of stations not used for set up of the method, which seems reasonable, again 4 stations seem little.

In conclusions, the authors should in my opinion address a number of issues as above for the paper to be properly considered for publication on the journal.

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