

Interactive comment on “Regional soil erosion assessment based on sample survey and geostatistics” by Shuiqing Yin et al.

Anonymous Referee #2

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I would disagree with the first review that this is not a valuable paper. The manuscript addresses an interesting issue and if the authors would improve it significantly then it can be worth publishing. However, there are a number of issues that the authors should address and correct.

The authors present in the introduction a number of methodologies for assessing soil erosion: a) fractional scoring b) plot measurements c) field-based approach d) Modelling (RUSLE). Then they analyse more in detail the application of RUSLE as 3 different options: 1) sample survey 2) raster multiplication 3) sample survey and geostatistics. The authors have followed the third option.

Find below the most important remarks and issues that authors should address in their revision: First remark: I would appreciate if the authors have compared their

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results with the second option. This would give much more advanced knowledge in the manuscript. You mentioned that you have available K-factor, R-factor maps at 250m resolution plus a land use map at 100000 scale. So, it would have been excellent to compare your results with an estimated Soil loss by water erosion (simply multiplying the above mentioned high resolution grids).

Second: As the 1st reviewer said (and I agree) , the authors have presented an interpolation method which takes into account 5 different group of parameters. It is logical (and obvious that the IV and V would perform much better than the I. In a recent research (to be online soon), we identified cover management factor as the most sensitive for estimating soil loss by water erosion. The manuscript could be even more worthy if the authors have compared their findings with alternative methods (plot measurements, expert knowledge, field-based approach).

Third: The findings regarding the forests are much too high. Erosion of $> 3 \text{ t ha}^{-1}$ in forest is not at all acceptable. Even there can be very steep slopes, the forestland experience erosion of much less than 1 t ha^{-1} annually. Their comparison with the findings of Guo (2015) and the findings in Europe (2015) show that erosion in forests is much less. The same applies for grasslands.

Please consider also a comparison of your findings with the paper of Wang et al (2016) "Assessment of soil erosion change and its relationships with land use/cover change in China from the end of the 1980s to 2010"

Fourth: authors should explain and justify the selection of their statistical model BPST and not the selection of Cubist or GPR or regression krigining? Moreover, In your geo-statistical model , the topography is ignored. Why?

Fifth: The filed survey (section 2.1) indicates that the sampling of erosion points was not so dense. Please give some levels of uncertainty taking into account that you sampled on PSU every 25 km 2 even less. Moreover, you mentioned that "PSU points were surveyed" : you don't describe how you estimate the R, K, LS, B, E, T factors

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in each point? Did you sample and analyse the soil for estimating K-factor? Did you install a high temporal resolution rainfall station for measuring R-factor? Etc . Maybe this is somehow written in section 2.3 but it is not clear as you don't provide detailed information on how the R-factor, K-factor was calculated.

In the same way that you criticize the non-availability of all input layers when multiplying the grids (factors), somebody can criticize your methodology that non all information (K-factor, R-factor, ect) is available at point level. How you respond to this?

More specific comments related to text: - P2L25-26: Rephrase the sentence. - P2L3: The reference should be Panagos et al, 2016a. The paper of Panagos et al, 2015a in your reference list does not feature in the literature. Please check carefully your literature. The same in P2L6 (it should be Panagos et al 2015; Panagos et al., 2016a). - P3L16-17: You cannot put in the same importance the papers of Bosco (2015) and Panagos (2015) regarding the European soil erosion assessments. The second one is much more advanced with new knowledge. For more info about the model evolution in Europe, please consider the paper of Borrelli et al (2016), Land Use policy. - P3L23-25: some references to NRI methodology and the outcome results are missing here. I would also appreciate some applications and datasets derived (With references) derived from this methodology. - P4L6-9: The European assessments was commented by 2 papers (you have put the 3 comments here) but you ignore the response of Panagos et al (2016a, 2016b) to those 3 critiques. - - Section 2.2 and elsewhere: R-factor, K-factor and land use map. Please give some citations and source of this dada. - You may be familiar with CSLE but for some non-Chinese, it would be better to write 2 lines about the biological and engineering factor. - P7L11-13: Your method has many similarities with the estimation of C-factor in Europe based on vegetation density and land use (Panagos et al, 2015 Land use policy). - P7 Equations 2 and 3 : they seem to be very similar. Which is the difference, please explain. - Model I has no sense as it is obvious to be so poor!!! - Section 2.4.2: you start without any introduction about BPST model. Please add an introductory paragraph - P11L13-15: it is obvious that

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model I and II will have no extreme erosion levels (as the land use is ignored). The smoothening effect is obvious! - Conclusions: P14L5-6: it is better somewhere in the introduction.. In general the conclusions should highlight the important findings of this study. - The way forward: The authors should conclude about the usefulness of their methodology. How this can be used? How it can be complementary to the traditional multiplication of grids.

Fig1: The Dark green image Towner-ship cannot be 10 x 40km? Fig2: The land uses are 5 and not 3. Fig 5: It is difficult to see the distinction between 40-80 and 80 categories in the first bar. Fig. 7: Scale bar and overview map is missing . As a non-Chinese, I don't know where this province is located.

Minor comments - P5L7: please replace with “erodibility” - “soil species” is not a mature term - 3.1 : Four or Five models?

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