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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Development of a 10 year (2001–2010) 0.1 dataset of land-surface energy balance for mainland China" *by* X. Chen et al.

Anonymous Referee #1

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In this manuscript, the authors applied an energy balance model, SEBS, which was developed by the authors group before, to evaluate whole China's terrestrial surface energy balances in 0.1-degree spatial resolution by making the maximum use of satellite data sets. The results show that the estimated fluxes are well represented in China. Comparisons with the eddy covariance measurements and other data sets show that the energy and radiation fluxes by the proposed approach attained one of the best performances among the data sets.

Generally, the global surface energy flux data sets, including reanalysis data, do not have enough spatial and temporal resolution when looking at the national-level fluxes. The surface flux data sets from reanalysis data sets still contain large uncertainty. Therefore, this reviewer agrees with the authors that it is necessary to produce spatially

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and temporal higher resolution surface flux data sets.

My major concerns are below: 1. From the current manuscript, it is not easy to find the novelty of this study. I understand that energy and radiation fluxes estimation across China in such a high spatial resolution is new. But I feel this may not be enough because the suits of equations used in this approach were developed in the past studies (Su et al., 2002) and there are other energy flux estimation studies with satellite data sets as is cited in this manuscript. It may be necessary to make an introduction to let readers know where is the novelty of this study.

2. Discussion of this paper is not organized well. Some of sentences are just the rewords of Introduction. Based on the validation results, I would like to see more general characteristics of the data sets. When and where the produced data is likely to fail or to deteriorate the accuracy? And why? What's the bottleneck? Data or flux modellings? How could it be improved in future study?

Specific comments

3. The authors use the term "turbulent heat flux". However, radiations like SWD, LWD are not considered turbulent heat flux. Rephrase it.

4. Page 14472, line 16: "turbulent flux and evapotranspiration" sounds like a little weird. Latent heat flux is also one of the turbulent flux, so I would recommend using latent heat flux instead of evapotranspiration.

5. Page 14486, Lines 3 – 7: I'm not sure that this comparison is meaningful and fair. The regions of interests are different and some of data are global estimation.

6. Table 3: please add the explanation of "MB" in the caption. "Mean bias"

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 14471, 2014.