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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.

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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

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Interactive Discussion

Discussion Paper

still contain large uncertainty. Therefore, this reviewer agrees with the authors that it is necessary to produce spatially and temporal higher resolution surface flux data sets.

RESPONSE: We thank the reviewer for these appreciated comments. We fully agree with them. We have added more detailed discussions as the reviewers has pointed out. To give the readers a fully understanding of our work, the following paragraph was added in the new manuscript, '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, partly due to the deficiency in their land surface process models that simulate land surface temperature by solving soil thermal transport equations (Chen et al., 1996) and usually result in a large error in LST simulation (Chen et al., 2011;Wang et al., 2014) if the model is not properly calibrated by measurements (Hogue et al., 2005). So the hypothesis tested in this paper is if it is possible to overcome the complex process in the soil by using satellite observed land surface temperature directly to calculate the land surface fluxes at continental scale? This study has demonstrated a benchmark on how to use satellite to derive a land surface flux dataset for a continental area on a personal laptop which is absolutely not feasible for the land surface process modeler to do in such a time and resource economic way.' on page 25.

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.

RESPONSE: Actually, part of the innovative points have been explained in introduction, model development, input dataset preparation, and dealing with a large heterogeneous

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data. We agree that the basic equations were developed in the past studies. However the past studies do not give solutions on how to upscale the model to a continental area. The meteorological forcing data and satellite product used in our work are also from other studies. But the problems is why we chose these dataset but not others? and how to combine several sources of dataset and use them in our study? Is the dataset applicable or not? All these issues need to be tackled before the model run. These experience and accumulated knowledge have never been reported in other papers. Thus firstly, we have innovated ways of using the model. Secondly, and certainly, the flux product is also one novelty of this work (on page 14491, line 24-26). As scientists have pointed out a spatially and temporally estimate of surface energy fluxes is urgently need by hydrological and meteorological studies due to that 'all the available flux datasets are based on model simulations, which have deficiencies for studying changes in water-cycle and land–air interactions in China'. As you have seen in our response to your previous comment that we also added another paragraph to make the second novelty to be clearer to the reader. Thirdly, there are so many challenges in the beginning of the work, such as: difficulties in producing an accurate estimate of water and energy spatial distribution at a continental scale with remote sensing method. Remote sensing approaches to estimate surface heat and water fluxes have been largely used on regional scales, but there is rarely satellite-derived data which could be used for land-atmosphere interaction studies for continental area (on page 14474, line19-24). But here, we have made the first step by using satellite data to make this reference dataset for China's continental land area. Besides, most remotely-sensed fluxes and evapotranspiration product have null values in urban, water, snow, barren and desert areas, such as the studies of Mu et al., 2007, Wang et al., 2007 and Jiménez et al., 2009 (on page 14475, line 1-18). Here we have overcome the shortages of their dataset and produced a spatially continuous distributions of land-surface energy fluxes and evapotranspiration. The sentence was added to make this advancement more clearly for the readers to understand the importance of our work: ' We have overcome the shortages of previous remotely-sensed evapotranspiration products which have

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null values in barren and desert areas.'. in the 'conclusion and discussion'. Finally, the critical challenge in using turbulent flux parameterization to remote sensing data is how to transfer from regional to continental and global scales (on page 14475, line 19-21). We have developed several steps to tackle the complexities met with the method when combining different spatial and temporal sampling input variables (on page 14480, line 13-30, page 14481, line 1-19). We also found a solution how to produce roughness length distribution for a continental area (On page 14475, line 25-29). Usually, the surface roughness length is given a fixed value in numerical models, here we developed a method to produce a dynamic variation of surface roughness length for the Chinese landmass which is closer to the reality. This novelty is notified by adding the paragraph in the 'discussion and conclusions': 'We also found a solution on how to produce a dynamic surface roughness length due to variations in the canopy height, which is closer to the reality, for a continental area. Usually, the surface roughness length is given a fixed value in numerical models.'

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?

RESPONSE: Thank you for pointing out these important issues for modellings. We agree that the assumptions and model imperfection are issues of importance. From the validation results, it shows that the sensible heat fluxes over high canopy is underestimated, this is due to the roughness sublayer over the high canopy is not considered in the model. So we added this sentence in the discussion part 'Additionally, the sensible heat flux over forest is underestimated by present turbulent flux parameterization method in SEBS which does not take the roughness sublayer over high canopy (Bosveld, 1999) into consideration.'. The bottleneck should be the availability of accurate remote sensing data, we have discussed partly on page 14492, line 14-19.

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To clarify the problem, we would like to add the sentences in the new manuscript to discuss it more and how the dataset may fail, 'The accuracy of turbulent heat fluxes is largely dependent on the remotely sensed land surface temperature. Here we have made an assumption that the averaged Aqua and Terra sensors sensed LST in each month can represent the monthly average LST. Terra satellite sensor passes twice a day (at about 10:30am, and 22:30pm local time), also the Aqua satellite passes twice a day (at about 01:30am, and 13:30pm local time). So MODIS have four samples each day. The samples may not be enough for calculating the monthly LST, also due to the cloud noise. Besides, the time period of MODIS datasets is not longer than 15 years which may limit application of our dataset in climate analysis.' following 'It is challenging to . . ..from half-hourly through to monthly.'

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

RESPONSE: Here we use 'turbulent heat flux' to represent sensible and latent heat. 'turbulent heat flux' was used two times in our paper. The first one is 'In our study we set out to estimate turbulent heat fluxes simulated with energy balance' and aerodynamic parameterization formulas that are based on a revised model of the surface energy balance system (SEBS)'. As this work is to calculate sensible and latent heat fluxes with SWD, LWD etc. which is produced by other works. So we think this is correct. The second usage is in the sentence 'Potential effects of changes in turbulent heat fluxes on the monsoon over East Asia (Lee et al., 2011)'. Lee et al. have analyzed the changes of sensible and latent heat impacts on the East Asia, so the sentence is also right.

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.

RESPONSE: We understand that the reviewer to pick 'turbulent flux and evapotran-

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spiration' out from the background. Turbulent flux includes 'sensible and latent heat fluxes'. So we do not suggest to use 'turbulent flux and latent heat flux'. Here we use 'turbulent flux' and 'evapotranspiration' to relate two community, 'turbulent flux' refers to the land-air study, and evapotranspiration refers to water cycle and hydrological study. The accurate downward long-wave radiation datasets are needed for both area when using the surface energy balance method. So we prefer not to change this sentence.

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.

RESPONSE: Vinukollu et al. (2011b) could be the first and only one SEBS application in global fluxes and evapotranspiration efforts. We also contacted the authors to share their dataset with us, unluckily, due to disk physical problems, they can't share the dataset with us which make it impossible to do more detailed comparative analysis. Our paper also addresses how to produce a continental turbulent flux and evapotranspiration dataset with the model, but with an improved one. Due to there are so many common basis, we think the comparisons are useful. We agree that the forcing dataset are different. But, we and Vinukollu et al. have the same purposeâĂŤhow to get more accurate global or continental heat fluxes and evapotranspiration. The lower RMSE could be due to the model improvement and more accurate forcing dataset used in our study. So we have added the sentence 'The difference could be due to the model improvement and more accurate meteorological forcing dataset used in our study.' to discuss the difference in RMSE values. This literature comparison is important for our conclusion that 'more accurate ....datasets are needed to be able to accurately estimate turbulent fluxes and evapotranspiration when using the surface energy balance model.'

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

RESPONSE: We have added 'MB is mean of observation minus model simulation.' in the caption of new attached manuscript.

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Reference: Bosveld, F. C.: Exchange processes between a coniferous forest and the atmosphere, Ph.D, Wageningen University, 181 pp., 1999. Chen, F., Mitchell, K., Schaake, J., Xue, Y., Pan, H.-L., Koren, V., Duan, Q. Y., Ek, M., and Betts, A.: Modeling of land surface evaporation by four schemes and comparison with FIFE observations, Journal of Geophysical Research: Atmospheres, 101, 7251-7268, 10.1029/95jd02165, 1996. Chen, Y., Yang, K., He, J., Qin, J., Shi, J., Du, J., and He, Q.: Improving land surface temperature modeling for dry land of China, J. Geophys. Res., 116, D20104, 10.1029/2011jd015921, 2011. Hogue, T. S., Bastidas, L., Gupta, H., Sorooshian, S., Mitchell, K., and Emmerich, W.: Evaluation and Transferability of the Noah Land Surface Model in Semiarid Environments, Journal of Hydrometeorology, 6, 68-84, 10.1175/jhm-402.1, 2005. Wang, A., Barlage, M., Zeng, X., and Draper, C. S.: Comparison of land skin temperature from a land model, remote sensing, and in-situ measurement, Journal of Geophysical Research: Atmospheres, 2013JD021026, 10.1002/2013jd021026, 2014.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C7051/2014/acpd-14-C7051-2014-supplement.pdf

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