Hydrol. Earth Syst. Sci. Discuss., 10, C1948–C1956, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C1948/2013/

Hydrology and Earth System Sciences Discussions
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Interactive comment on "Inverse modeling of hydrologic parameters using surface flux and runoff observations in the Community Land Model" *by* Y. Sun et al.

Anonymous Referee #1

Received and published: 24 May 2013

Inverse modeling of hydrologic parameters using surface flux and runoff observations in the Community Land Model

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Overview

1. The authors explore a method for the retrieval of hydrological parameters of a land surface scheme, using observations of runoff and latent heat flux observations. They use a Monte Carlo Markov chain method to retrieve parameters of CLM4, and explore several alternative scenarios within this framework. They also use Bayesian model

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Interactive Discussion
Discussion Paper

averaging to combine the results from several of the scenarios.

2. I think that the topic area is of considerable interest, since land surface schemes are moving towards increasingly complex representations of subgrid hydrological processes, for which reliable parameter estimates are currently difficult to obtain. Inversion methods are a potentially useful approach to obtaining parameter estimates.

3. I was not aware of the previous work by the authors showing that LH flux was sensitive to some of the hydrological parameters in the CLM4 model, and I was intrigued because it seems a little surprising that parameters which are intended to control runoff generation are also having a significant impact on latent heat flux at the monthly time scale. It is plausible that changes in parameters which affect runoff generation can also lead to consequential changes in soil moisture, which in turn affect latent heat fluxes. However, the relatively weak physical basis for the hydrological parameters means that some of them have extremely wide prior distributions and very complex joint distributions with other parameters, so perhaps any reliable information on the water cycle might be helpful in narrowing those very wide priors.

4. I would like to see some discussion on whether the SIMTOP concepts used in CLM4 are meaningful at these specific sites; what are the dominant pathways for runoff generation in Walnut River? TOPMODEL-style concepts are not very relevant in some physical settings, for example where deep groundwater flows comprise a substantial component of the water balance, or where parts of the catchment are disconnected from the river system for extended periods of time, or where infiltration excess is the dominant runoff generation mechanism.

5. I found the authors' discussion of the posterior distributions rather subjective, and I reached quite different conclusions to the authors. To me, it seemed that the posterior distributions were very sensitive to small changes in the reference acceptance probability, and the sensitivity seemed random at times. If the method is working correctly, I don't understand why the distributions of all the parameters do not change gradually

HESSD

10, C1948–C1956, 2013

Interactive Comment

as the p_ra value is varied from 1.0 to 0.95 to 0.90. My observation is that as p_ra is varied gradually, the posterior distributions jump around randomly sometimes, and this leads me to doubt the reliability of the results; I would like to be reassured that the method is in fact working correctly.

Main points

6. 5079L10 “It is also important for an inverse approach to be capable of quantifying and evaluating the prediction uncertainty” Please briefly explain why this is important.

7. 5080L1-11 This paragraph seems to address uncertainty in parameters, and in measurements of the model output variables, but it does not mention uncertainty in the forcing data, or in the model structure, both of which should be addressed.

8. 5082L1-6 What is the uncertainty in the forcing data which you derived from NLDAS? This is relevant because errors in forcing would affect the inversion process.

9. 5083L8 “However, simulations of heat ïňĆux and runoïňĂ using the calibrated parameters show only small improvements compared to simulations using the default parameter values.” This seems like a result, and belongs later in the paper. In any case, it deserves more discussion. Why do you think PEST was unable to find better parameter sets than the default? What PEST options/features did you use? Is PEST a less efficient optimiser than MCMC? What if you had used a different optimiser with the same objective function? Is the least squares objective function really very different to the log-likelihood function?

10. 5083L8 The discussion on the PEST application is too brief to be useful to readers. I suggest you either expand it or remove it altogether (since PEST is not central to the paper).

11. 5084L6 “assumptions that εi jεi j are normally distributed with variance σ2i j, and the distributions are independent” Do you have any information to support these assumptions? Did you make any transformations of the outputs to ensure these assumptions

HESSD

10, C1948–C1956, 2013

Interactive Comment

were approximately satisfied? Would your study have reached different conclusions if your assumptions were incorrect?

12. 5084L6 What assumptions did you make about the variances for runoff and LH flux?

13. 5087L14 “the US-ARM site and one MOPEX basin (07147800), which are located in close proximity with similar climate and land surface conditions.” Since the basin has an area of over 4800 sq km, is it meaningful to say that these two are close together? And is this 4800 sq km basin really that homogeneous in climate and land cover? You stated earlier that the US-ARM site is in croplands, but the basin is only 22% croplands.

14. 5088L10 “Posterior distributions with diïňĂerent reference acceptance probabilities generally are consistent, except for fdrai, Qdm and Sy when the rejection rate is very low with a reference acceptance probability pra of 0.5” In my view, the posterior distributions are NOT generally consistent across the various values of p_ra. To meet my criterion for consistency, I would expect the distributions to substantially overlap. For example, for f_over, the distributions for p_ra=1.0 and p_ra=0.95 hardly overlap at all. And for Ks, the higher P_ra values lead to posterior distributions lying mainly between -1 and 0, but the posterior distribution for p_ra=0.5 lies mainly between -2 and -1. I think the degree of consistency needs to be quantified if the authors wish to pursue this point.

15. Figure 1: I was surprised that the posterior distributions did not change in a more systematic manner as p_ra varied, and so it is not clear to me whether the sampler has converged. What were the stopping criteria?

16. 5089L6 “which might be due to errors in the observed heat ïňĆuxes, errors in the CLM forcing data, and/or under-representation of the complicated physical processes using the current parameterization schemes.” It would greatly aid the reader if the authors could provide uncertainty estimates for the heat fluxes (could the true mean LH flux for January really be 10 W/m2 lower than the measured value?) and in the

CLM forcing data. It would also be helpful for the authors to point out any features of the land surface processes at this site which they consider are not well parameterized in CLM4.

17. 5089L3 “However the estimates with reference acceptance probability of 0.5 noticeably deviate from other inversion estimates” Whether the differences between simulations are considered large or not must depend to some extent on the uncertainties in those predictions. How large are the uncertainties (due to parameter uncertainty) in the simulations of LH flux for each p_ra?

18. 5089L12 “They show consistent patterns for diïňĂerent reference acceptance probabilities, except for the parameter b.” Again, in my view, the posterior distributions are NOT generally consistent across the various values of p_ra. For f_over, the p_ra distribution does not overlap with the others, the distributions of C_s occupy most of the feasible space, and there seem to be two distinct Ks distributions. The authors must be using other criteria to decide on consistency; these criteria need to be made explicit in the paper.

19. Figure 6: To understand the relatively poorer model performance at US-ARM for LH flux, it would be helpful to have some basic information about the comparative climates at the two flux sites. Why does the measured LH flux at US-ARM have a seasonal peak in April (and similar values in May-June, while the US-MOz has a clear peak in June? Is this an effect of moisture limitation at US-ARM, or plant development/harvesting, or something else?

20. Figure 6: It seems that simulations of LH flux using some of the posterior parameter distributions (especially p_ra=0.9) are worse than the default set of parameters. This is especially so in winter, when none of the posterior distributions are better than the default, and most are worse. This should be commented on, since it is at variance with the authors’ later claim that “Inversion results at the ïňĆux tower and MOPEX sites using monthly and daily surface ïňĆux and runoïňĂ observations show that the MCMC-

HESSD
10, C1948–C1956, 2013

Interactive Comment

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Discussion Paper

Bayesian inversion approach eïňĂectively and reliably improves the simulation of CLM under diïňĂerent climates and environmental conditions". The use of the adjective "reliably" does not seem justified.

21. 5091L6 "It is interesting to see that fmax is identically estimated by inversions with diïňĂerent reference acceptance probabilities. When the rejection standards are relaxed, the bounds of posterior distributions of most parameters become wider, and multi-modal patterns occur" It would be helpful if the authors could explore the reasons why the distribution of fmax might be insensitive to p_ra, especially when most other parameters are more sensitive.

22. Figure 7: These results conform more with my expectations (compared to Figures 1, 3, and 5, which did not). The posterior distributions using p_ra=0.5 (for Cs, f_over, f_drai, Q_dm, Sy, Psi_s) tend to be quite distinct from those obtained using other p_ra values (1, .95, .9). I would conclude from this that, using the authors methods, inversion of several hydrological parameters from runoff data can be achieved, but inversion of hydrological parameters from LH data cannot be reliably achieved.

23. Figure 7: Do the authors agree that the results in Figure 7 are more in line with their expectations than those of Figures 1, 3, and 5? If yes, what do they think are the implications of that result?

24. 5091L13 "larger variability than observations is noted from July to October" the modelled variability seems to arise from a high modelled streamflow in August. Why is that? Was there a single very large rainfall event one August for which the model runoff greatly exceeds the measured runoff?

25. 5091L15 "Among the four sets of simulations based on inversion, more stringent sample rejection criterion results in a better match between the simulated responses with observations." This is as expected, and is good to see. Did this also happen for the LH simulations? If not, why not?

26. 5092L2 “simulated LH and runoïňĂ are most sensitive to three subsurface parameters.” Which three parameters? Is this sensitivity result reflected in the present study - were their posterior distributions narrower than those of other parameters?

27. 5092L20 “Using posterior estimates of the reduced parameter set can signiïňĄcantly improve the latent heat ïňĆux simulations compared to the results using the full-set of parameters, especially from October to December, and from January to May” Given these improvements, Figures 9 and 10 would be more interesting if they contained results from US-ARM, rather than US-MOz,. Did the authors choose US-MOz for some other reason?

28. 5095L1 “Inverse modeling using heat ïňĆux at US-ARM and runoïňĂ at the MOPEX basin, which is located close to US-ARM, provides an opportunity to assess the impacts of data type on inverse modeling.” I think it is more than just data type that differs between these two model assessments! There are substantial differences in spatial scale, and hence in the dominant land surface processes and the spatial heterogeneity thereof, between the two cases.

29. 5095L14 “model inversion leads to more signiïňĄcant improvements in runoïňĂ (Fig. 8) than heat ïňĆux (Fig. 2) compared to simulations that use the default parameter values.” This might also be caused by having rather poor default estimates of the parameters which control hydrological processes, and rather better default estimates of the parameters that control LH fluxes.

30. 5095L25 “may require structural changes in the hydrologic parameterizations combined with parameter calibration to improve model skill.” It could also be a problem with the forcing data for the MOPEX catchment. Does the NLDAS precipitation data agree with the MOPEX precipitation data (which is based on a relatively large number of rain gauges)?

31. 5097 I would have liked to see some discussion on the potential benefits of (i) using other observations (such as soil moisture), which more tightly link the water and en-

ergy components of the CLM model (ii) doing inversions which simultaneously consider runoff, LH flux (and soil moisture).

32. 5098L25 “The improvement is more signiïňĄcant for runoïňĂ than heat ïňĆux because the calibrated parameters are more directly related to runoïňĂ processes.” I do not agree. I think the main reason that the improvement is larger for runoff is that the default parameters produce very poor simulations of runoff.

Minor points

33. 5078L13 Unclear meaning: “the predictive intervals of the calibrated parameters become narrower”

34. 5079L08 “However, as the conditions are usually violated in practice, some regularization is generally needed to introduce mild assumptions on the solution and prevent parametric over-ïňĄtting.” Which of the 3 conditions is usually violated? All of them?

35. 5080L1 “the input and output uncertainties” By input do you mean the external forcing data, or the parameters or both?

36. 5081L26 “covered by 6 % C3 grass, 22 % C4 grass, and 20 % croplands” What about the other 52%?

37. 5083L16 “In practice, it is critical to evaluate and quantify the uncertainty associated with parameter estimation; therefore, we should consider stochastic inversion/calibration approaches (e.g. Bayesian inference) and describe the input/output uncertainties in a probabilistic manner.” This text belongs more in an introduction

38. 5084L20 the symbol n is not defined

39. 5085L2 the symbols p_ra and p are not defined

40. 5085L6 the symbol q is not defined

41. 5092L1 “Our global sensitivity analyses across 13 ïňĆux towers and 20 MOPEX

basins," Are you citing earlier work? If yes, please reference it.

42. 5092L8 "a reduced set of parameters" How did you choose the values of the fixed parameters?

43. Figure 15B: there is too much temporal information compressed into this graph – the authors need to find an alternative presentation. For example, just show a single year of the validation, or present the daily data in summary form (e.g. flow duration curves).

44. 5097L19 "We found that RMSEs are reduced more for monthly data than for daily data" This was not clear to me. Which reduction are you referring to? What causes it?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 5077, 2013.