Hydrol. Earth Syst. Sci. Discuss., 9, C119–C122, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C119/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Combining surface reanalysis and remote sensing data for monitoring evapotranspiration" *by* M. Marshall et al.

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Received and published: 22 February 2012

This study evaluated two models for actual evapotranspiration at 8 eddy flux sites in Africa. The authors aimed to improve performance of the PT-JPL model by replacing some components with the Noah LSM. The authors used GLDAS forcing data for the models. It should be commended that the focus of this study is on a region and data that are understudied. This is a very complex study integrating two very different types of models, inconsistent QAQC eddy flux data, reanalysis forcing data, sensitivity analyses, and model modifications. However, like a complex model, each of these components opens up vulnerabilities to errors in the analysis, which propagate through so as to make the end result difficult to interpret and containing large uncertainty. The paper is very long and long-winded, revealing disconnect between the strong, but eventually

off-place foundation in the beginning, the ambitious Methods, the clearly unexpected results, and the strong Discussion that does not link with the rest of the paper. The take-home points are riddled with confusion and uncertainty. The primary question of interest to the reader, "Why do the models agree or disagree with the observations?" is not clearly answered, likely because it cannot be answered given the analysis, other than the typical hand-waving explanations of "complexity". Much of the model-obs differences are due to both the quality of the forcing data and the energy balance, but neither of those are rectified, and the conclusions are worded generally and specifically in the structure of the physics of the models.

It is not exactly clear why the main methodological work–replacing PT-JPL functions with Noah–even takes place. The argument to swap the interception evaporation and soil evaporation components out of PT-JPL because RH is not readily available or accurate globally, but Noah relies on Precipitation, which is also not readily available or accurate globally, furthermore, Noah introduces a whole suite of new uncertainties and degrees of freedom, such as as the soil properties–texture, field capacity, wilting point–and canopy water holding capacity. It is understood that the interception evaporation parameterization in PT-JPL is simple, but a more logical replacement/modification would be with the GLEAM interception evaporation approach, which is one of the more intriguing parameterizations for interception evaporation.

In summary, the paper presents important African data and a complex modeling framework, but small holes in multiple parts of the complexity add up so as to render the conclusions uninterpretable.

- P1549L19&21: What about precipitation? - P1550L3: Needs a ref. - P1550L6: What about Jimenez et al 2011 and Mueller et al 2011? - P1550 full paragraph: This is a bit muddled. There are delineations within the non-LSM approaches. The authors need to differentiate empirical approaches (e.g., linear eqn with NDVI) from physics based approaches. From the physics-based approaches, the authors need to differentiate between the indirect approaches (i.e., those that calculate H, then take LE from the residual of the energy balance; e.g., SEBS, ALEXI) and direct approaches (i.e., those that target LE directly from Penman-Monteith or Priestley-Taylor; e.g., PM-MOD16, PT-JPL). - P1551: Non-LSMs and LSMs alike can provide near real-time AET–just depends on the forcing data and model set-up. I think what the authors are thinking of here are operational LSMs, which should be differentiated from most other research LSMs. - P1552L3: Fisher & Tu recently renamed this model to PT-JPL. Search and replace throughout for both reference to the "Berkeley" model and the "Fisher" model. There is large inconsistency with the naming of the Noah model as well. - P1552L3: Why integrate it with a LSM? - P1552L9: These components are not actually highly empirical power functions. They are based on the physics of PT for PET, then reduced to AET using simplified physics-associated functions, e.g., drying function based on RH. I think what the authors are looking for here is the drawback is that they rely on RH, which is not readily available. They could say that the simplified nature is a drawback too, though others would argue the simplicity is a strength. - P1552L11: This is an assumption that improvements could be made by linking it with another model. Could just as easily be degradation if the other model is not good. - P1553L7: The PT-JPL model is based on Priestley and Taylor (1972) for potential evapotranspiration (PET), then introduces ecophysiological constraint functions to reduce PET to AET. - P1554L3: Is G readily available? - P1555L8: Midday values are used not because they show better results, but because of the theoretical justification. In some cases they show worse results. - P1557L3: Why 2000-2008? - P1557L8: "decadal" - P1558: Energy balance not corrected. This could be the major source of error in the model predictions. - P1559: Show time series of NDVI and EVI for each site. - P1559: P vs. soil E. Not a good comparison.

- Table 1. Two comments here. First, is the use of EVI substituting for the original Fisher et al. 2008 SAVI. EVI does not equal SAVI. It needs to be clear somewhere how the swap took place, and what kind of normalization was needed and implemented. Second, the exponent of 10 in fwet is a little different than original. It shouldn't matter too much, but curious to see what effect it did have. - Table 5. It is not clear what the

usefulness of this table is. - Fig 2. Confused as to what the PT-JPL and Noah aspects are in the figure. It would be good to show the forcing data skill at all the sites. - Fig 3. This fig is not very intuitive. For instance, no change with p and q, so why include them in the further ensembles? - It is unclear whether or not the models were forced with in situ met data or with GLDAS data. The models should be forced with in situ to assess their actual performance, otherwise the models are only so good as the forcing data, and it is difficult to interpret why variation exists. - It is not stated why the models behave when and where they behave against the data, so not much is gained in terms of the science from this paper.

Sorry Mike for a somewhat harsh review! There's some good stuff here, but it still has a way to go.

Josh Fisher

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1547, 2012.