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Hydrology and Earth System Sciences Discussions

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Interactive Comment

# *Interactive comment on* "Evaluation of a complementary based model for mapping land surface evapotranspiration" *by* Z. Sun et al.

**Anonymous Referee #2**

Received and published: 18 July 2012

Thank you for the opportunity to review this study.

Like the other reviewer, I would suggest that if the title of the paper aimes to focus on the complementary relationship, then it is important to demonstrate howthe Venturini Equation draws on Granger's complementary relationship in this construction: it does appear on a casual read to primarily be a modification of the Priestly-Taylor Equation.

I think the paper has promise, but it does need to be communicated more clearly. In my reading the authors have set out to: a) do a sensitivity analysis on 2 kinds of assumptions/techniques that Venturini proposed in the original formulation of their model b) do a sensitivity analysis on a new parameterization based on surface temperature to estimate the evaporative fraction in the Venturini equation To move beyond sensitivity



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Interactive Discussion

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analysis, the authors have also proposed to work with ASTER images and data from 2 flux towers to validate and evaluate the proposed techniques at a range of scales. These goals need to be more clearly articulated at the beginning of the study. It is quite difficult to follow the study aims in its present form. It would also be sensible, I think, to include an additional and maybe more standard technique for ET estimation – e.g. the authors could compare their methods with a Penman-Monteith FAO type equation to show how well the Venturini and P-T methods perform compared to what might be considered a state-of-the-art approach?

I have some other detailed queries about the study.

With respect to the first trial of the Venturini equation (hereafter VE), I am confused about the rationale for linearization of the saturation vapor pressure relationship. Considering that a form of the SVP – T curve is explicitly used in the parameterization of Tu, (to estimate $\Delta$'1 and $\Delta$'2), why can the full SVP curve not be used to estimate $\Delta$1 and $\Delta$2 and thus F? Or is this precisely the issue that the authors propose to check?

Secondly, I am a little concerned about the time-period used to evaluate the effects of linearization of the VE. As noted by the authors, the linearization should become more spurious as the differences between Tu, Td and Ts become large. It is not clear to me that by making the comparison only over a 2 day period of time, that the large differences that might "break" the assumption have been incorporated. Why not work with a longer period of time that should incorporate more seasonal variation, more weather variation, and more capacity to test the VE assumptions for a range of conditions? This criticism really applies to the range of tests of the VE quality and the implications of the VE assumptions – it would be good to see the tests run over longer periods of time with a greater diversity of conditions.

Does the finding in section 4.1.4 imply that the iterative technique proposed by Venturini does not converge on a solution, but instead relentlessly pushes Tu towards Ts? This seems highly problematic! Can the reason for this lack of convergence be illuminated



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mathematically?

I am unclear on the message, in fact. My interpretation is that the Venturini Equation, as originally proposed, cannot make accurate predictions because the value of Tu is essentially impossible to determine. However, the new parameterization based on surface temperature alone can avoid the need to specify Tu, making the use of the method more robust; at the expense of some bias being introduced into the specific ET prediction. Is this the point?

Some QA/QC data need to be provided. Can the authors provide details of the footprint of the flux towers, and can they confirm that the necessary conditions of homogeneity within the footprint are met? That is, are all areas within a given footprint planted with the same crop, irrigated with the same volumes and timing, etc? This is important to understand the comparisons being made.

Overall, I suggest that the authors revise the manuscript to make their aims very clear, to target the methods closely towards the aims, and to ensure that all conclusions and results are very clearly expressed. The core of the study seems ok, but it is hard to understand it completely in its current form.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 3029, 2012.