Responses to Referee 2

Authors appreciate you very much for your constructive comments on our manuscript. We have tried to respond to all your comments point by point and to revise our manuscript accordingly. We hope our responses clarify all questions and concerns. All revisions were colored in red on the revised manuscript.

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 how the 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.
Response: As you and Referee 1 suggested, the title was changed to "Evaluation of a Priestley-Taylor model for mapping land surface evapotranspiration". The derivation of the Venturini equation from Granger's complementary relationship (Granger, 1989) was added in section 2.1.

I think the paper has promise, but it does need to be communicated more clearly.
Response: Several revisions along with structure updates were technically conducted in the context to highlight the study purpose and content, including:

1) Scientific questions, research motivation, research purposes, and main content were highlighted in the "Introduction" section.
2) A brief introduction of content in each section was added at the beginning of "Methodology", "Study site and data", and "Results and discussions", respectively.
3) The purpose of evaluation on the Venturini equation was divided into four sub-purposes which were highlighted in section 2.2, and results were also respectively presented in section 4.1.

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…
Response: Figures 8-12 detailedly tested the 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….
Response: Table 2 as well as it related discussion was added for a sensitivity analysis of the improved Venturini equation. Percent changes of F and ET resulted from ±10% changes of $(T_s)_{max}$, T_a, T_s, and F, respectively, were calculated.

…To move beyond sensitivity 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…
Response: This point regarding the ASTER-based case study was added at the end of the "Introduction" section.

…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?

Response: Authors believe that eddy covariance fluxes are the most straightforward to evaluate / validate model estimates.

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 Δ'1 and Δ'2), why can the full SVP curve not be used to estimate Δ1 and Δ2 and thus F? Or is this precisely the issue that the authors propose to check?

Response: As presented in this paper and Venturini et al. (2008), T_u is the temperature corresponding to the surface actual vapor pressure on the SVP curve (see Fig.1). If T_u or e_s are obtained from field measurements, ET estimates from the Venturini equation is rational. Results in this study also dispelled your concern about the rationale for linearization of the saturation vapor pressure. Evaluation strategies of ES1, ES2, and ES2', and their results were presented in details in the manuscript.

Because e_s is hard to obtain from remote sensing, Venturini et al. (2008) proposed a proxy of T_u (see Eq. 6b in this paper). However, this "virtual" T_u also cannot be obtained from remote sensing, so Venturini et al. (2008) parameterized T_u through linearizing the SVP curve (see Eq. 6b in this paper). Authors proved that the proposed parameterization of T_u is questionable, and proposed a new straightforward parameterization of F to fix this problem. This is the motivation and research purpose of this paper.

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.

Response: There are two reasons for the data selection on two days. With a purpose to ensure that the humidity at the observational height of 1 m represents the surface vapor pressure (e_s) of crop, data should be collected when crop height is 1 m. Only two days were selected for evaluation when maize height was 1 m. This is the first reason. The second reason is that desired results have been obtained from two-day evaluation data. Based on two-day data, following results were obtained: if T_u or e_s are obtained from field measurements, the Venturini equation is rational; the lack of convergence for T_u calculation is pointed out.

A long-term dataset (half-an-hour measurements from 21/9/2006 to 21/11/2006) was used for the validation of the improved Venturini equation.

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 mathematically?

Response: Yes, the referee also found the potential problem of the Venturini equation, the lack of convergence. As seen from Fig. 2, any value T_u on the SVP curve between T_s and T_d could yield Eqs. 6a and 6b.

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?

Response: The referee got the point of this paper. This is the motivation and research purpose of this paper. Authors highlighted this point in this context.

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.

Response: A description of the footprint of the flux tower was added in the context. The eddy covariance sensors were installed at a height of 6 m at the center of an open homogenous cropland. The distances to the edges of the cropland in all directions are greater than 600 m. This ensures most of flux measurements from the experimental cropland (Schmid, 2002). As for extreme occasions (e.g., strong wind), the remaining few percent of flux may come from an area beyond the edge of the experimental cropland. Crop type and field managements (e.g., seeding, irrigating, fertilizing, and harvest) are the same for the experimental cropland.

Instruments on the flux tower and automated meteorological station were carefully calibrated before installation and are maintained by a professional company once a year after installation. The routine maintenance of instruments as well as long footprints ensures the quality of flux measurements.

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.

Response: As suggested by the referee, the content and structure of this paper was improved for good communication with readers. Detailed responses were addressed above.

Reference:

Schmid, H.P. (2002). Footprint modeling for vegetation atmosphere exchange studies: a review and perspective. Agricultural and Forest Meteorology, 113, 159-183.

Venturini, V., Islam, S., & Rodriguez, L. (2008). Estimation of evaporative fraction and evapotranspiration from MODIS products using a complementary based model. Remote Sensing of Environment, 112, 132-141.