

***Interactive comment on* “Estimation of
Evapotranspiration and Other Soil Water Budget
Components in an Irrigated Agricultural Field of a
Desert Oasis, Using Soil Moisture Measurements”
by Zhongkai Li et al.**

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Short Comments

I read the paper by Li et al. with great interest as we are doing similar research activities, i.e., the inversion of the soil water balance equation to obtain estimates of rainfall (e.g., *Brocca et al., 2014*) and irrigation (*Brocca et al., 2018; Zaussinger et al., 2018*) through soil moisture observations (no need to cite them in the paper, it's just for in-

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formation). In our research, we mainly use satellite observations and therefore we are testing the capabilities to estimate rainfall and irrigation through satellite soil moisture products, assumed not accurate.

In the paper by Li et al. it is mostly assumed that in situ soil moisture observations are perfect and through the inversion of the soil water balance equation accurate estimates of water fluxes, i.e., irrigation, evapotranspiration and drainage, are obtained. While I mostly agree that in situ soil moisture data can be used for obtaining water fluxes, i.e., the soil water budget components, I also believe that uncertainties in the inversion approach are present.

The main issue I found is exactly related to the uncertainties in the inversion procedure. For instance, the six soil moisture probes show significant differences in the amount of irrigation for different probes; irrigation at NT1 is nearly double (1-8 times) of NT3. I do not understand how it might be possible as I believe that the amount of irrigation applied in the field is the same for all probes. Am I wrong? Indeed, the irrigation estimates strongly depend on the assumption behind equation (2) and, specifically, in the estimation of S_{max} . Why is the comparison with in situ observations of irrigation not done? I believe it is needed definitely.

Anyhow, I congrats the paper for the very interesting paper.

References (not exhaustive)

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