Response to the anonymous reviewer 3# (RC3)

We would like to thank the reviewer 3# for his accurate and frank review and used their precious suggestions to improve the paper. We tried to answer to all the comments made and we are ready to prepare and submit a new version of the manuscript. The point by point answers are written in blue.

General comments

Arguments of this paper need to be stated more clearly. Sub-sections 3.1, 3.2 and 3.3 should be moved to the Section 2. Conclusion section repeats the results to significant extent, while responding to the arguments very weakly. The novel contributions of this work should be presented explicitly. Readers may feel confused by finding out the focusing points: the improvement of methodology or new scientific findings?

Response: Thanks for the nice suggestion, according to which we reorganized the proposed questions in our work, and make it more focused on the improvement and implementation of the methodology. The novel contributions of this work also have been further clarified according the reviewer's suggestion. The reviewer also suggests us to move the Sub-sections 3.1, 3.2 and 3.3 to the Section 2. We spend much time to think about it, and finally decide to keep them at the original places, because the information presented in sub-sections 3.1, 3.2 and 3.3 are the results from our laboratory experiments and field observations, rather than a simply background introduction.

Specific comments

1) Line 1-2: Estimation of Evapotranspiration and Other Soil Water Budget Components in an Irrigated Agricultural Field of a Desert Oasis, Using Soil Moisture Measurements, **Comment:** (1) evapotranspiration is one of the soil water balance components. Is it necessary to let it stand out here? (2) what are the key issues to be addressed in this paper. A clear definition to the problem is needed.

Response: (1) Yes, we do think it is necessary to let evapotranspiration (ET) stand out, because ET is the most important one among all the soil water balance components (SWBCs), and the one the related researchers are most interested in, because of its direct relevance to the crop yield, and the fact that maximizing crop yield is the major objective of agricultural irrigation strategies (Kang *et al.*, 2002; Liu *et al.*, 2002; Zhang *et al.*, 2004). (2) The key issue we concerned in this paper is the potentials of soil moisture measurements in determining ET and other SWBCs in the croplands of desert oases environments.

2) Line 14-15: water cycle is principally driven by irrigation (I), drainage (D), and evapotranspiration (ET) in desert oasis settings, **Comment:** Water cycle is primarily driven by evaporation demand under influence of irrigation. Soil water percolation may occur when too much water applied to the root zone. Anyway, it is not proper to say that water cycle is driven by irrigation and drainage.

Response: Thanks for the nice suggestion, we have changed "water cycle" as "hydrological process of farmland", and cited this comment in the revision.

3) Line 24-25: through a data-driven method that combined both the soil water balance method and

the inverse Richards function. **Comments:** (1) It is not very common to say 'Richards function'. Instead, Richards equation is the most popular description. (2) data-driven? According to the manuscript, it is a soil-moisture data-based method. This method is not uncommon.

Response: (1) Thanks for the useful information, and the description of "Richards function" has been replaced with the more popular one ("Richards equation") in the revision; (2) As the reviewer suggested, "soil moisture data-based method" was adopted in the revision to replace the "data-driven method". We agree that the idea of "soil moisture data-based method" is not uncommon in literatures, because soil moisture measurements were used to estimate the infiltration by numerical solutions as early as 1950s (Gardner and Mayhugh, 1958; Hanks and Bowers, 1962). However, ET estimates with the inverse methods are recent developments, i.e., Zuo *et al.* (2002), Ross (2003) and Guderle and Hildebrandt (2015). Indeed, according to our knowledge and based on the literature search, only very few researches applied this method especially in arid environments and coarse-texture soils due to the limited availability of highly resolved soil moisture measurements, so that here we would argue this method is still novel, and it deserves more attention in future researches on agricultural water management. Our work investigated for the first time the performance of using soil moisture measurements to determine ET and other *SWBCs* in the croplands of desert oases. The estimated results of the *SWBCs* will provide a great potential for optimizing irrigation strategies, thus moving toward sustainable water resources management in water-limited environment.

4) Line 31-32: "suggesting that the irrigation amounts had limited influence on the accumulated ET throughout the growing season", **Comment:** Regarding this study, enough water was applied to each treatment and caused significant percolation, indicating that crops grew under non-water stress condition. However, it cannot be concluded generally that irrigation amount had limited influence on the accumulated ET. Otherwise, this may mislead both understanding and practice.

Response: Thanks for pointing this out. We have reorganized this statement in the coming revision as "suggesting that the superfluous irrigation amounts had limited influence on the accumulated ET throughout the growing season because of the poor water-holding capacity of the sandy soil".

5) Line 45: Traditional irrigation, **Comment:** What is the traditional irrigation? It should be defined specifically because it is different from place to place around the world.

Response: The traditional irrigation in this work was defined as flood irrigation, and it has been further clarified in the revision.

6) Line 58-50: In desert oasis settings, the water cycle is principally driven by irrigation (I), drainage (D), and evapotranspiration (ET). None of these drivers is easily measured in practice. **Comment:** It is not proper to call all these components drivers of water cycle.

Response: This sentence has been re-worded as follows: "In desert oasis settings, the hydrological process of cropland is principally driven by irrigation (*I*), drainage (*D*), and evapotranspiration (*ET*)".

7) Line 65-66: "its estimation is only possible through the application of mathematical models, and is commonly calculated by relying on reference ET(ET0) or potential ET (PET)", **Comment:** "only possible"? You might have not said it.

Response: This sentence has been re-worded as follows: "its estimation in field scale is usually through the application of mathematical models, and is commonly calculated by relying on reference ET

(ET_0) or potential ET (PET)".

8) Line 79-80: . . .oasis. So far, however, no works have been published on testing the potential of using a soil moisture database as a data-driving method in this region. **Comment:** As prerequisite condition, it should not be locally limited. Otherwise, the value of the research could be discounted.

Response: This part will be re-organized in the coming revision to solve the concern.

9) Line 161: With no water shortage, **Comment:** It is better to phrase it as under non-water stress (condition).

Response: rephrase as the reviewer suggested.

10) Line 164: The potential ET during that day. **Comment:** How is the potential ET calculated here? Reference ET, potential ET, maximum ET are different concepts.

Response: Potential ET here was calculated through Penman-Monteith combination equation using hourly environmental data during the period from 1 April to 30 September (Fig. 3). This information has been mentioned in section 2.3.3 of the earlier version of manuscript, and further clarified in this revision.

11) Line 199-200: The upper boundary of the calculation was set as the atmospheric boundary condition, and the calculation involved actual precipitation, irrigation, and potential evapotranspiration rates for the crop cover. **Comment:** (1) how is the film mulching effects considered for the upper boundary condition? (2) how is the bare soil evaporation estimated as the upper boundary condition? (3) how is the upper boundary condition defined for the inter-cropping treatment? And the alternative mulching strips?

Response: (1) the film mulching effects on the upper boundary condition were modeled as proportionally damped such that $E_{p,a} = \beta \times E_p$, where β is the area percentage without plastic film mulching in each experimental plot, and E_p is potential ET estimated with the Penman-Monteith method. This issue has been clarified in the revision. **(2)** Basically, the bare soil evaporation (E_a) can be estimated via equation 6, which was provided in section 2.3.2. However, to be convenient in our coding, a simplified method proposed by Porporato *et al.* (2002) was employed to do this calculation, i.e., the evaporation was assumed to linearly increases with soil moisture (θ) from 0 at the hygroscopic point (θ_h), to $E_{p,a}$ at the field capacity (θ_{fc}). For values of θ exceeding θ_{fc}, evapotranspiration is decoupled from soil moisture and remains constant at $E_{p,a}$. We have added this information in the revision to clarify this point. **(3)** As already been mentioned in our response to question (1), we defined the upper boundary of alternative mulching strips according the ratio of plastic film mulching (i.e., 40%) and the potential ET estimated with the Penman-Monteith. However, we did not set specific upper boundaries for inter-cropping treatments, because the difference in surface soil evaporation between mono- and inter-cropping treatments could be relatively small when comparing with the transpiration in a growing season. We clarified this point in our revision and some potential uncertainties caused by this simplification also were include in this revision.

12) Line 226: In Table 4, **Comment:** Table 2?

Response: Sorry for the typo, it should be Table 2. We have corrected it in the revision.

13) Line 237: The profile averaged values of saturated drainage velocity (K_s) were 119, ˘ 129.36, 286.04, 189.42, 207.92, and 216.14 cm day^{-1} at. . . **Comment:** It is not necessary to list these values in the text because they are already given in the table.

Response: The part has been reworded as suggested.

14) Line 269: . . .irrigation crop demand… **Comment:** Irrigation demand, crop demand are meaningful concepts in crop water requirement studies. What does the irrigation crop demand mean?

Response: Sorry for the misleading wording. It has been changed as "irrigation volume" in this revision.

15) Line 292: . . ., a slow-down or even a very light increase. . .**Comment:** A slow-down decrease or even light increase?

Response: Yes, it should be "A slow-down decrease or even light increase". Thanks for point it out.

16) Line 293-295: We checked all the soil moisture time series of NT1-NT6 during the entire growing season period (Fig.5), and no constant water content throughout the entire soil profile was detected in any of those selected plots, suggesting that our previous hypothesis that no steady-state flow took place during any irrigation events was supported. **Comment:** What is the purpose of this sentence? For any frequently irrigated soil profile, it is hard to reach a steady flow state.

Response: This sentence was used to prove that our previous hypothesis that no steady-state flow took place during any irrigation events was correct. We agree that it is a little bit redundant because it is hard to reach a steady flow state for any frequently irrigated soil profile. Following the reviewer's suggestion, it has been removed in the revision to solve the concern.

17) Line 298: . . .and strong potential evaporation may have hampered any effective infiltration from those precipitation events. **Comment:** It is the insufficient precipitation that attributes to the negligible infiltration rather than the strong evaporative demand.

Response: Thanks for pointing this out. We had corrected it in the revision.

18) Line 310: . . .and increased gradually as LAI became greater with crop development, . . . **Comment:** LAI has never been mentioned previously in the paper although it is very important information supporting discussions in the later sections.

Response: Since we don't have detailed information on LAI in this paper, we change the word "LAI" as "vegetation coverage" in the revision.

19) Line 315-316: The relative facility with which an excess of water in the soil was produced caused an important deep percolation, which became greater as it progressed further up the irrigation gradient. **Comment:** This sentence should be rephrased. It is confusing.

Response: It has been rephrased as: "The excess of water in the soil produced an important deep percolation, which became greater as the increasing of the irrigation quota."

20) Line 340: . . ., the soil moisture data-driven method. . .**Comment:** The soil moisture data-based method, might be a better description to this work.

Response: Corrected it in the revision as suggested.

21) Line 341: . . .the best. . . **Comment:** "the best" among which and which?

Response: "The best" has been replaced with "the better" here in the revision, and thus the revised sentence will be "Compared with the methods used in the literatures listed in Table 4, the soil moisture data-driven method used in this paper is more reliable because it produced the better fit between the numerical solution and the measured values of soil moisture content, even with vertical flow accounted for Guderle and Hildebrandt (2015)."

22) Line 344-345: . . ., which in turn suggested that both cropping systems and agronomic manipulation had limited influence on the accumulated ET during the growing season, . . .**Comment:** This is correct when preconditioned only.

Response: preconditions have been included here to solve the concern, and the statement in the revision has been reworded as "which in turn suggested that for the unmulched alfalfa and mulched maize, both cropping systems and agronomic manipulation had limited influence on the accumulated ET during the growing season".

23) Line 365: 4.2 Other estimated SWBCs. **Comment:** Does it mean the other SWBCs in this study or the SWBCs given by other people in the literatures?

Response: We mean the other SWBCs given in this study. It has been reworded as "the other SWBCs in this study".

24) Line 401: 4.3 Long-term effects on soil water budgets. **Comment:** Does this manuscript involve any long-term issues, either the parameters or the water balance budgets?

Response: Yes, this manuscript does involve some related issues of long-term management, i.e., the plots were designed to do long-term agronomic manipulation experiments (~10 years). Although the calculation was not based on long-term measurements, the long-term effects of agronomic manipulation on the soil hydrophysical properties and thus in turn on the soil water budget balances were analyzed. To solve the concern, this sentence has been changed as "section 4.3 Long-term effects on soil hydrophysical properties".

References:

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