Authors’ Response to Referee Comments by T. Russo

General Comments:

Overall this is an interesting paper on an important topic. The data collection methods seem thorough, and use current technology to quantify previously difficult fluxes. The water balance model is quite simple and in some ways not explained thoroughly. The paper could benefit greatly by omitting much of Sections 2 and 5, improving the description of the methods, especially the calculations, and ensuring that the Discussion and Conclusion actually focus on the results of this paper, rather than reviewing other literature.

Response:

Thank you for the generally positive comments. I appreciate your suggestions and would like to revise and modified the paper accordingly.

For the model explanation, the Section 3.3 (Methodology) has been revised to describe the model more clearly. To improve the structure of the manuscript, Section 2 has been greatly simplified, and the Section 5 (Discussion) and Section 6 (Conclusion) have been modified to focus on the measurements and results of the study. The quantitative results obtained from field experiments have been thoroughly discussed in Section 5.1.3 (Balanced development stage).

While we acknowledge that our field experiments were specifically carried out in a cotton field under mulched drip irrigation condition, our results can indeed be QUALITATIVELY extended to other crop fields under different irrigation methods. With these extensions, we can discuss the interactions between social and hydrological systems in this hyper-arid inland oasis, which is also the purpose of this special issue ‘Predictions under change: water, earth, and biota in the anthropocene’. Such broad perspective also can help us gain deep insight into the multifaceted effects of irrigation method conversion and achieve a sound policy for sustainable water management.

Specific Comments:

1. The history of the TRB is interesting, but not needed to support the paper conclusions.

Response:

According to the comments, we have shortened the history of TRB. Still the history is mentioned in the Introduction and Discussion parts since the anthropogenic effects on water resources are important for oases development. As we'd like to discuss the groundwater dynamics results from a socio-hydrological perspective, the TRB history is necessary, which can help us not only understand the current situation of TRB, but also predict the future when the irrigation method has been changed.

2. Statement that water saving irrigation mitigates soil salinization is arguable. I can't find the paper (Ma et al, 2010) in English. If this was a conclusion of that paper, then it should be introduced as a hypothesis, or at least stated with respect to areas with shallow water tables only.

Response:

We agree with the referee that soil salinization trend is still unclear under water-saving irrigation condition. In general, water-saving irrigation can mitigate soil salinization when groundwater table is shallow (Dou et al., 2011; Rajak et al., 2006). However, salinization also can be caused by deficient leaching water under water-saving irrigation (Chen et al., 2010).

In TRB, groundwater table in most irrigated croplands had risen to less than 1 m below the surface due to long-term flood irrigation in the late 1990s and soil salinization was severe because of the intense phreatic evaporation. Therefore, the application of water-saving irrigation indeed mitigates the soil salinization in TRB (Ma et al., 2010; Wang et al., 2011). To ensure the statement clear, we have revised the statement. Now it reads: "Recently, water-saving irrigation has been popularized within the TRB to enhance the irrigation efficiency and mitigate soil salinization in the irrigated farmlands where the groundwater table *is quite shallow (Dou et al., 2011; Ma et al., 2010; Wang et al., 2011)". In addition, more relevant papers have been cited here for reference.*

3. Section 2 can be shortened to include only the relevant material for the project.

Response:

Thank you for the suggestion. Section 2 has been shortened from 1059 words to 744 words now.

4. Do you calibrate between the two SWC methods, hydra sensors and gravimetric method?

Response:

Thank you for the interesting question. We did not calibrate anything, but we did compare the soil water content results measured by different methods (see Fig. A). The figure shows that the SWC results by soil sensors and gravimetric method agree well. Moreover, in this study, we only considered the change of soil water storage (ΔS *) during water balance analysis. Although there will be some systemic errors between different methods, the SWC change for each method is relatively consistent and reliable.*

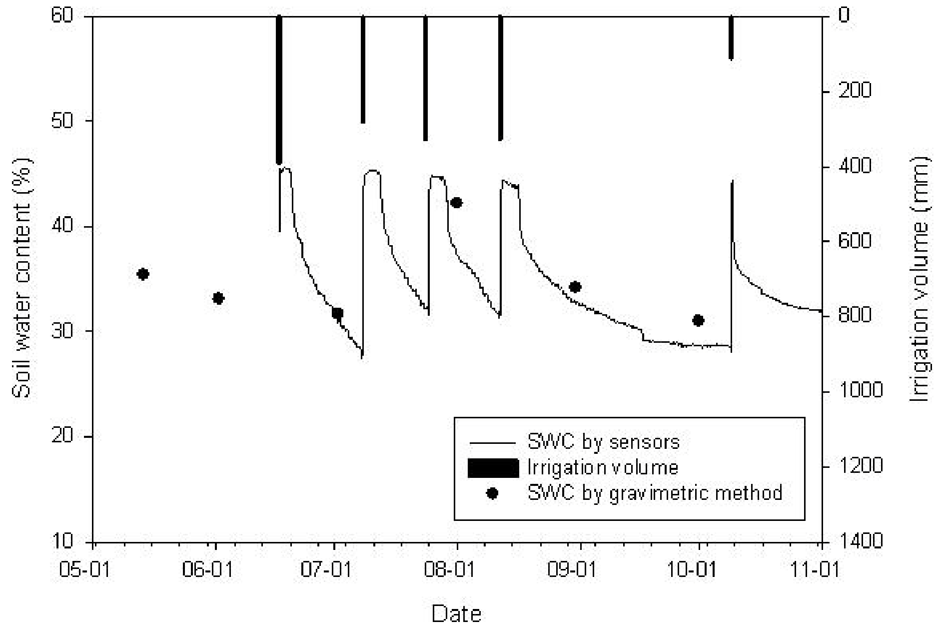

Fig.A Soil water content measured by different methods in 2011

5. Lateral flow is ignored in Eq 1 because it's negligible in the control volume, however it's included in Eq 2. Is LF needed to close the water balance in this case? Please explain why it is needed here and not before.

Response:

We have modified Section 3.3 (Methodology) to make it clearer. Actually, Eq. 1 focuses on the water balance of soil column which is above the groundwater table. The lateral flow is trivial in the unsaturated zone. However, Eq. 2 focuses on the groundwater balance, and the lateral flow is significant below the groundwater table.

6. Please explicitly define ΔS, ΔS_D, and S_D and make sure their use is consistent. When you discuss in section 4 changes in the soil water, does this refer to ΔS or ΔS_D?

Response:

Thanks for the suggestions. The manuscript is revised to make sure ΔS, ΔS_D, *and* S_D *explicitly defined in Section 3.3. In Section 4, the expressions of* *“changes in soil water” have been revised to be more specific to avoid misunderstanding.*

7. $(\theta_{sat} - \theta^{'})\Delta Z_{wt}$ is the change in water storage associated with the change in water table, and the description of ΔS_D makes it sound like the change in water storage between the water table (the bottom of the control volume) and the upper boundary of water table variation (where the water table was?). These appear to be the same. Please clarify the text to differentiate between these two, and confirm that they account for the full mass balance without counting anything twice.

Response:

Sorry for the confusion. Actually, the control volume in this study refers to the soil column stretching from ground surface to 90 cm soil depth. Therefore, ΔS_D is the soil water storage change in the zone between 90 cm (bottom of control volume) and upper boundary of groundwater table variation, and $(\theta_{sat} - \theta^{'})\Delta z_{wt}$ is the soil water storage change associated with a falling or rising groundwater table.

The calculation is correct and the results shown in the paper are precise. We have revised the expressions to avoid misunderstanding.

8. Clarify Figure 2 to illustrate what areas ΔS and ΔS_D apply to. This will also be clearer when you define them in the text (previous comment).

Response:

Revised according to suggestion.

9. p1790, ln 21. How did you measure the porosity? Did you also determine θ_{sat} from any saturated SWC measurements?

Response:

Three undisturbed soil columns were collected in the experimental field using the special containers, and gravimetric method was adopted to measure the saturated SWC by a drying oven. θ_{sat} *was determined as 0.42.*

The results of hydra sensors verified the value of θ_{sat}*. SWC measured by hydra sensors was 0.424 within 50 cm soil depth during the first three days of spring flush in 2013. The surface soil in this period was regarded as saturated soil due to the large amount irrigation volume and remarkable water ponding. Therefore, it is reasonable to assign a value of 0.42 to* θ_{sat} *in this study.*

10. Section 5. The discussion on human-water systems, including the review of water use in the area seems like an appendix to the paper, rather than an integrated part. It should either be omitted or shortened significantly and justified by integrating with the results of the paper.

Response:

Thank you for the suggestion and we have modified the Section 5 to make it more concise. Although, the authors would like to keep the broad discussion and literature review, as explained in the response to general comments. Specifically, in this paper, the exchange flux and groundwater dynamics have been studied based on the field experiments under water-saving irrigation condition. The results have been discussed in the sub-section "5.1.3 Balanced development stage", the key part of Section 5. However, in order to well understand the effects of irrigation method conversion on human-water system, the other two stages of human-water system development have also been discussed here. This broad view may help us to evaluate the effects of water-saving irrigation and predict the future of oases.

11. The paper would benefit from a limitations section in the discussion.

Response:

Thanks for the suggestion. We have reorganized and shortened the

Discussion Section.

12. The Conclusion section should emphasize the findings and conclusions drawn explicitly from this paper, rather than summarize the motivation for the study.

Response:

Thank you for the suggestion. The Conclusion Section has been revised to focus on the findings and results drawn from this paper.

Technical Corrections:

1. Several language issues p1781 ln 22, (and elsewhere in text) "mainstream" should be"main stream" or "primary channel"

Response:

Thank you for the suggestion. The term "mainstream" has been revised to "main stream".

2. overuse of the word "serious" and "seriously"

Response:

Thank you for the comment. Unnecessary "serious" and "seriously" have been replaced or deleted.

3. p1781, line 27, start new paragraph with "Large-scale irrigation..."

Response:

Done according to suggestion.

4. p1783 ln14, no "in general"

Response:

Done according to suggestion.

5. p1784 ln 4, "conveyed" should be "conveying" or "routing"

Response:

Done according to suggestion.

6. Section 4.2. Please revise and clarify the first sentence.

Response:

This sentence has been rewritten as "The seasonal groundwater dynamics are analyzed in this section using the Eq. (2)".

7. Table 1 needs more explanation. Should 2012 and 2013 listed be the same year?

Please also list the year for the bottom two rows.

Response:

There are some typesetting problems in Table 1. First two rows are the data in 2012, and bottom two rows are the data in 2013. It will be corrected in the new version.

8. Figure 1. Can't read the lat/lon values in the top two maps, too small.

Response:

Thank you for the comment. Figure 1 has been splitted into two, also that all the characters can be read in the larger figures.

9. Figure 3. Hard to distinguish between two grays. It also might be more intuitive to flip the y-axis for exchange flux to show negative flux going up.

Response:

Revised according to suggestion.

10. Overall could benefit from an English language review, I did not edit for language throughout the manuscript.

Response:

Thanks for the edition. We have reorganized the manuscript, corrected *language mistakes and modified the expressions.*

Reference:

Chen, W., Hou, Z., Wu, L., Liang, Y., and Wei, C.: Evaluating salinity distribution in soil irrigated with saline water in arid regions of northwest China, Agric. Water Manage., 97, 2001-2008, 2010.

Dou, C., Kang, Y., Wan, S., and Hu, W.: Soil salinity changes under cropping with Lycium Barbarum L. and irrigation with saline-sodic water, Pedosphere, 21, 539-548, 2011.

Ma, Y., He, J., Hong, M., and Zhao, J.: Analysis of development process and tendency of drip irrigation under film technology in Xinjiang, Water saving irrigation, 12, 87-89, 2010 (In Chinese with English abstract).

Rajak, D., Manjunatha, M. V., Rajkumar, G. R., Hebbara, M., and Minhas, P. S.: Comparative effects of drip and furrow irrigation on the yield and water productivity of cotton (Gossypium hirsutum L.) in a saline and waterlogged vertisol, Agric. Water Manage., 83, 30-36, 2006.

Wang, R., Kang, Y., Wan, S., Hu, W., Liu, S., and Liu, S.: Salt distribution and the growth of cotton under different drip irrigation regimes in a saline area, Agric. Water Manage., 100, 58-69, 2011.

Authors' Response to Comments by Anonymous Referee #2

Specific Comments:

1. I found the topic interesting and important however in my point of view the paper is not well structured. The research question is not clear enough and was not answered properly. The authors started from a very broad and general problem of sustainable groundwater level to a very specific field-scale study and again tried to generalize it by introducing a longer time series of groundwater level record for the Tarim River Basin. But how the field-scale observation and modelling is transferred to a longer period of general

groundwater behavior remains unclear to me as many factors like deep wells and other irrigation methods are simply neglected.

Response:

Thank you for the comments. We agree with the referee that the other irrigation methods and water extraction from the deep wells are important for us to understand the effects of irrigation on human-water system in oasis. Since the experiments were mainly implemented in the cotton field under mulched drip irrigation, information and data about other irrigation methods were mainly collected from the relevant literatures. The results from experiments and information from literatures were both discussed in the Section 5 (Discussion).

While we acknowledge that our field experiments were specifically carried out in a cotton field under mulched drip irrigation condition, our results can indeed be QUALITATIVELY extended to other crop fields under different irrigation methods. With these extensions, we can discuss the interactions between social and hydrological systems in this hyper-arid inland oasis, which is also the purpose of this special issue 'Predictions under change: water, earth, and biota in the anthropocene'. Such broad perspective also can help us gain deep insight into the multifaceted effects of irrigation method conversion and achieve a sound policy for sustainable water management.

We have also reorganized this manuscript and made it more concise. Section 2 (Description of Tarim River and Kaidu-Kongqi River Basins) has been greatly simplified. Meanwhile, the Section 5 (Discussion) and Section 6 (Conclusion) have been modified to focus on the measurements and results of the study.

2. The conclusion is too general, this conclusion and behavior of groundwater dynamics can be anticipated even without any calculation. I am wondering what is the novelty of the result and this work.

Response:

We generalize our conclusion based on the field experiment and qualitative *extension. We agree that the qualitative extension about behavior of groundwater dynamics can be anticipated without any calculation. However, we place the results in the context of socio-hydrology and discuss the interactive aspects between human and water such as irrigation paradox. Moreover, the quantitative analysis implemented in this study can help us gain more insights on the general conclusion.*

3. The presentation of model is not enough and clear. First of all, I ask the authors to clearly distinguish between fluxes and states. Fluxes and states cannot be summed or subtracted without considering time steps. I would suggest to change the labels into single letters with appropriate subscripts (e.g. I_{S}). I also suggest the authors to conceptualize the soil column and each layer clearly by explaining the states and fluxes one by one and their interactions. A flux can be positive or negative but this should be clearly explained. In the abstract there are positive and negative values which are reported, I suggest to remove them as you mentioned the upward or downward directions. The exchange flux (EF) is introduced in introduction but to my point of view it is too generic to be mentioned in this way as almost all the fluxes in a hydrologic systems can be considered as exchange fluxes.

Response:

Thanks for the suggestions. We have modified Section 3.3 (Methodology) to make the descriptions of model clearer. The fluxes and states have been distinguished, i.e., I, P, ET, EF and LF are fluxes, and the changes of SWC are states. Positive represents inflows and negative represents outflows of the control volume. That is to say, at the upper boundary of control volume, I and P are positive and ET is negative. At the bottom boundary, positive EF represents upward flux and negative EF represents downward flux. If we have mentioned the inflow or outflow directions, the plus or minus signs will be ignored. More descriptions of exchange flux have been added to the introduction to make the definitions clearer.

4. I would suggest the authors to make one figure with different panels with equal axis out of figures 3-6. This way they make it much easier for the reader to compare the fluxes, groundwater fluctuations and rain during different periods.

Response:

Thanks for the suggestions. Figure 3-6 have been merged into one figure with different panels. For the same period for different years, the equal Y-axis has been adopted. However, since the values of EFs are quite different for different periods, the Y-axis is not identical for all the periods.

5. I am not personally agree with the argument that any work must include uncertainty analysis. However for this study as the fluxes and states are estimated it would be interesting to see how the final finding, which in my point of view is not a surprise, will be affected.

Response:

Thanks for the suggestions. The error analysis has been carried out and the results are shown in a separate section (Section 4.5).

Authors' Response to Comments by Anonymous Referee #3

General Comments:

The authors present an interesting case study of groundwater dynamics for the Tarim River Basin of Western China. The article is well written. The major concerns I have are that the article does not present some of the raw water balance data and reports only mean estimates of water balance terms with no uncertainty. It is difficult to trust conclusions drawn by the study without properly estimating uncertainty with the mass balance method used in the manuscript. With an inclusion of some of the raw data and basic uncertainty analysis I feel the article would be suitable for publication.

Response:

Thank you for the comments. The raw data have been presented in Section 4.1. Also, the error analysis has been carried out and the results are shown in a separate section (Section 4.5).

Specific Comments:

1. P1779 L22. An uncertainty of exchange flux should be reported with the mean.

Response:

The error analysis has been carried out and the results are shown in a separate section (Section 4.5).

2. P1786 L17. I am not sure what a phi20 evaporation pan is. Please explain more or provide reference.

Response:

Φ20 evaporation pan is the circular evaporation pan with the diameter of 20 cm. It is widely used in Asia to determine the quantity of evaporation at a given location (Liu et al., 2009; Xu et al., 2006). More explanations about Φ20 evaporation pan have been added to the Section 3.1.

3. P1786 L19. Please provide some detail about the soils? Type, %sand, %silt, %clay, bulk density, porosity, soil hydraulic parameters, etc. Difficult to assess rate of fluxes through soils without a qualitative or quantitative description.

Response:

Thank you for the suggestion. Soil information has been presented in Section 3.1. The major soil type in experimental field is silt loam, and the sand, silt and clay contents are 32.8%, 62.4%, and 4.8%, respectively. The soil porosity is 0.42 which was directly determined in the laboratory using the known volume *of undisturbed soil columns collected in the experimental field.*

4. P1787 L22. So what is the energy balance closure then, 10%? Please provide a graph documenting seasonal changes in LE, H, RN, G. Could also include monthly estimates of average diurnal cycle of energy balance terms. Hard to gain insight about how system works without seeing some basic data.

Response:

The figure of energy closure has been presented in this paper (Fig. 5). Figures of seasonal changes in LE, H, R_n and G have also been shown in Section 4.1 (Fig. 4).

5. P1788 L5-10. Were the high changes in pore water conductivity due to brackish irrigation water accounted for in the estimates of volumetric water content using TDR methods? Please also present some of the raw data and report both the mean and uncertainty of the changes in water content with depth. Soil moisture is highly variable in space, how representative are the two profiles you instrumented to the larger study area? Difficult to trust EF value without first justifying changes in soil water content represent the entire field instead of 1 point in the 3.48 ha field.

Response:

We agree with the referee that the soil moisture is highly variable in space. We compared the soil water content results measured by different methods (Fig. A). The figure shows that the SWC results by soil sensors and gravimetric method agree well. Moreover, in this study, we only considered the change of soil water storage (ΔS) during water balance analysis. Therefore, although there will be huge spatial heterogeneity in SWC, the SWC change at each measured location is relatively consistent and reliable. Moreover, the error analysis including SWC uncertainties has also been shown in Section 4.5.

In this study, the fresh water from the canals was used for irrigation rather than brackish water from the groundwater wells. Therefore, the effects of brackish *water have not been considered.*

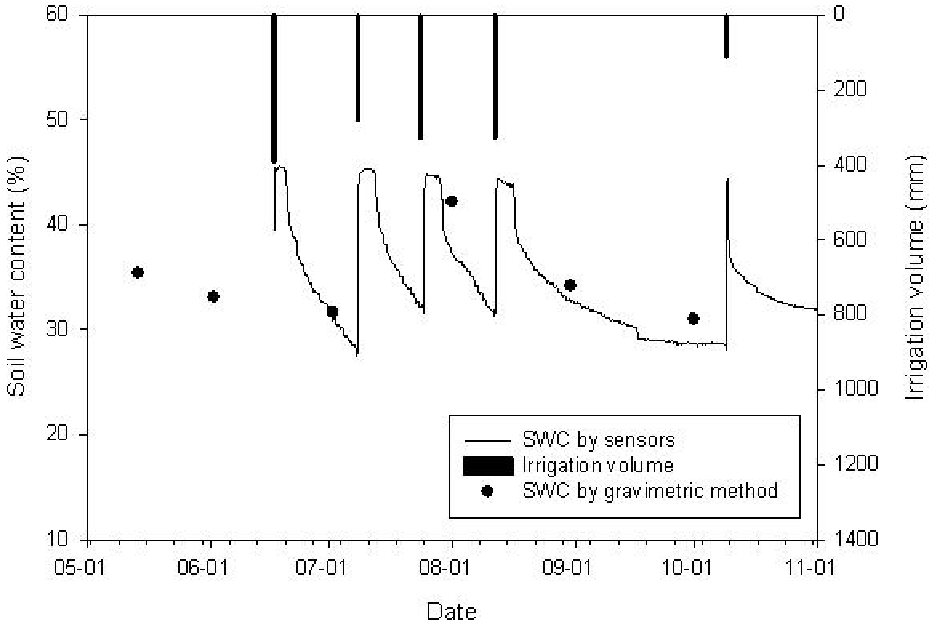

Fig.A Soil water content measured by different methods in 2011

6. P1790 L21. Porosity is not reported in manuscript, please provide with more description about the soil types.

Response:

Thanks for the suggestion. Soil information has been added to the Section 3.1. The major soil type in experimental field is silt loam, and the contents of sand, silt and clay are 32.8%, 62.4%, and 4.8%, respectively. The soil porosity is 0.42 which was directly determined in the laboratory using the known volume of undisturbed soil columns collected in the experimental field.

7. P1793 L10. “soil water storage”.

Response:

Revised according to suggestions.

8. P1799 L9. “which was common after previous flood irrigation events”.

Response:

Revised according to suggestions.

9. P1800 L18. “salinization is problematic”.

Response:

Revised according to suggestions.

10. Table 2. Please provide estimates of uncertainty as well.

Response:

The error analysis has been carried out and the results are shown in a separate section (Section 4.5).

Reference

Liu, C., and Zeng, Y.: Changes of pan evaporation in the recent 40 years in the Yellow River Basin, Water international, 29(4), 510-516, 2004.

Xu, C., Gong, L., Jiang, T., Chen, D., and Singh, V. P.: Analysis of spatial distribution and temporal trend of reference evapotranspiration and pan evaporation in Changjiang (Yangtze River) catchment, Journal of Hydrology, 327(1), 81-93, 2006.

Authors’ Response to Comments by Anonymous Referee #4

General Comments:

This paper investigated the water balance of an arid inland basin, where knowledge about the interactions between water and salt balance is very important for sustainable socio-economic, agricultural, ecological and water resources managements. Measurements by contemporary advanced eddy co-variance techniques was used in this paper, which facilitate the researchers to close water balance and help us to derive more reliable knowledge about this kind of important ecosystem. Overall, the topic is important and interesting. However, this paper is subjected to major revision for publication. I am reporting below two general comments and some specific remarks, which I hope are useful.

Response:

Thank you for the comments.

1. Quantitative analysis of salt balance is needed.

This paper only presented one essential cycle, i.e. water balance, for the sustainable water management in an arid inland basin. Without quantitative results of the other critical cycle, i.e. salt balance, and coupling between two cycles given me a strong perception that, at current stage, novelty of this paper for sustainable water management in the Tarim River was very limited and the discussion digressed from data and results. So, major revision is expected.

Response:

We agree with the referee that soil salt balance is critical for the sustainable development of an arid inland basin. We did investigate soil salt condition just like water balance we presented in this manuscript. Some relationships between salinization trend and exchange flux have been explored (Fig. A). The results show that the salinization has been mitigated when the upward exchange fluxes are significantly reduced. Since the water balance and groundwater dynamics under water-saving irrigation is the major concern of this paper, and the relationships of soil water and salt need a lot of analysis and discussions, the salt balance is excluded in this paper. We plan to discuss the coupling of two critical balances (water and salt) in a separate paper in future.

We have also reorganized this manuscript and made it more concise. Section 2 (Description of Tarim River and Kaidu-Kongqi River Basins) has been greatly simplified. Meanwhile, the Section 5 (Discussion) and Section 6 (Conclusion) have been modified to focus on the measurements and results of the study.

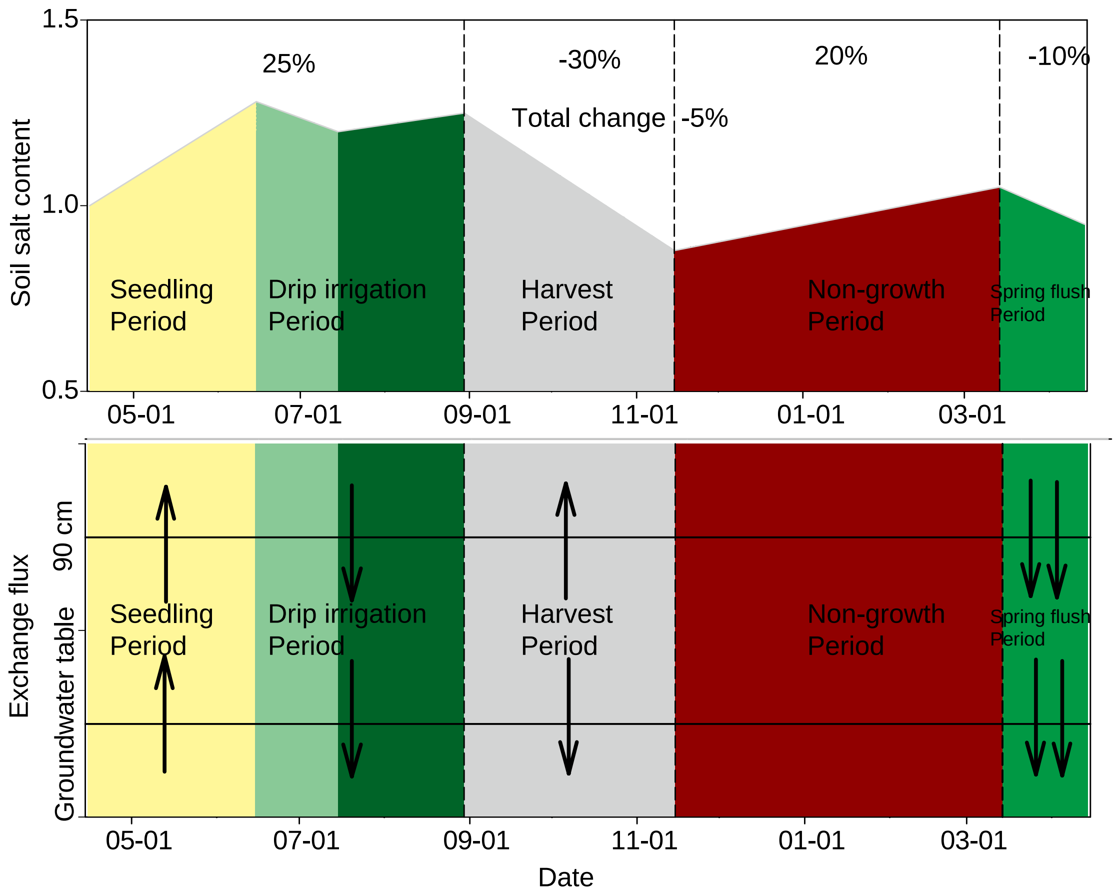

Fig.A Relationships between soil salinization and exchange flux

2. More attention should be paid to uncertainty in EC data.

EC data provided observed evidences of water evaporated from control volume. However, this data includes many uncertainties for estimating ecosystem evapotranspiration, such as closure of energy balance as authors mentioned. According to the data and methodology, uncertainty in EC measured ET was eventually introduced into exchange flux (EF) of the control volume, which is very critical for understanding groundwater table dynamics and salt cycle. Uncertainty in ET derived from EC data has important consequences to results of this study. Figure 4 shown daily upward EF could be larger than 10 mm/day. It was larger than I thought. I was wondering that to what extent the estimation of EF was affected by the uncertainty in ET. Soil water contented data of multiple layers has been collected. It can be used to quantify these uncertainties.

Response:

The error analysis including uncertainty in ET measured by EC has been carried out and the results are shown in a separate section (Section 4.5). We agree with the referee that there will be some uncertainties in EC data such as the energy imbalance. However, although the reasons underlying the energy imbalance has been investigated by numerous researchers over the past few decades, these are complicated and not yet fully understood, and Eddy Covariance is still regarded as the most reliable instrument to determine ET. The ET results obtained by EC and sap flow in this experimental field in 2012 are shown in Fig. B. The consistent trend provides more confidence on EC measurements (Zhang et al., 2014).

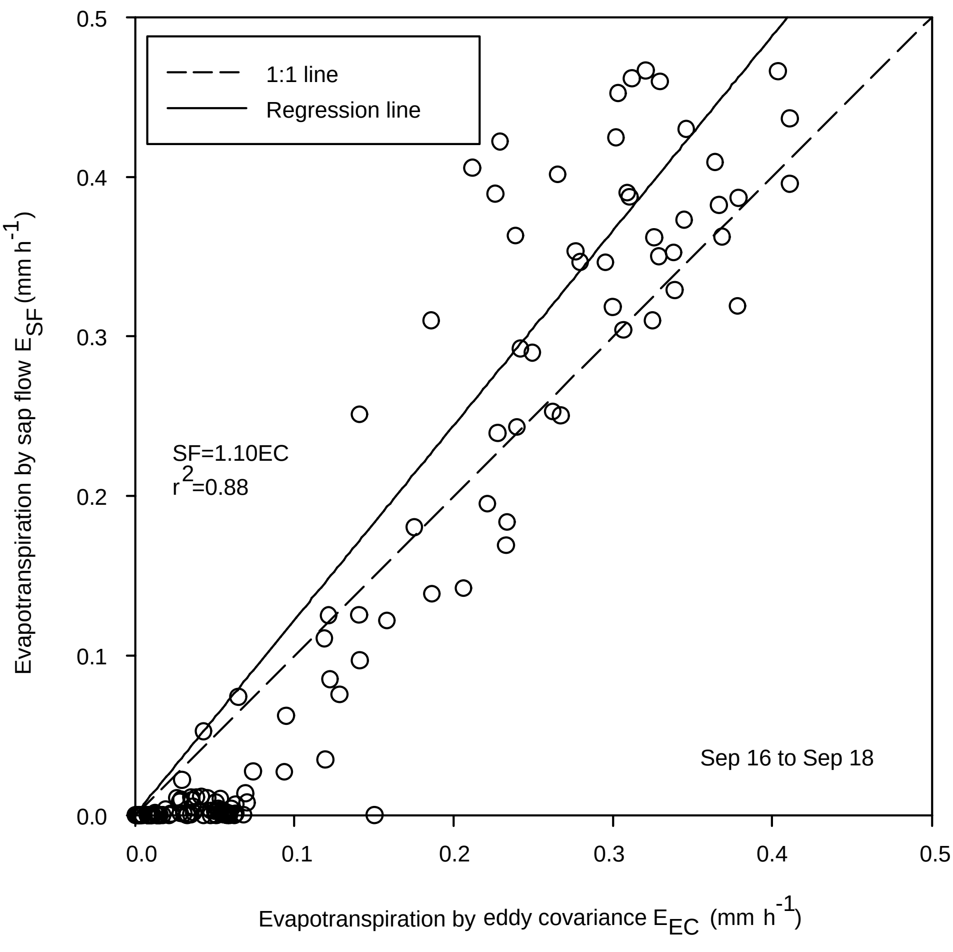

Fig.B Evapotranspiration obtained by Eddy Covariance and Sap Flow

Specific Comments:

1. Providing maximum rooting depth at the experimental site.

Response:

Done as suggested.

2. Using mm as unit of soil and water depths in the context, figures and tables.

Response:

Done as suggested.

3. P1789-L7: exchange flux (EF) is too general. Cannot recall easily and directly what specific process it represents.

Response:

Thank you for the comment. Exchange flux (EF) specifically refers to the water flux between the different soil layers in this paper. EF has been used to simplify the expressions. More descriptions of exchange flux have been added to the introduction and methodology sections to make the definitions clearer.

4. P1789-L10: Lateral flow can be considered as one component of runoff. Should R here be defined as overland flow?

Response:

R here can be defined as overland flow and the definition has been revised in the paper.

5. P1789-L14: during the flooding period, the depth of groundwater table is less than 0.9 m. Thus, control volume was NOT always above the groundwater table. And, please introduce how equation (1) was tackled under this situation.

Response:

We have modified Section 3.3 (Methodology) to make it clearer. During period of spring flush, the groundwater table exceeds the bottom boundary of control volume. Under this situation, R and F_L in the control volume also can be ignored due to the homogeneous irrigation condition and short duration of *water ponding and shallow groundwater table. The water flows along vertical direction are dominant rather than those on horizontal direction. Therefore, the Eq. (1) also can be used in the spring flush period.*

6. P1790-L19: why measurement of SWC at 150cm does not represent SWC at depth at 130 170 cm as those intervals centred on measurement points at depth of 100 and 120cm?

Response:

Thanks for the comments. There are two reasons that the SWC at the depth of 150 cm was assumed to apply to the soil level at 130–200 cm but not the soil level at 130-170 cm: (1) The change of SWC in deep soil layer has trivial effect on the water balance analysis. (2) SWC is more homogeneous in the deep soil layer, thus the SWC at 150 cm can be applied to the broader range.

7. P1791-L20-22: "The sum of …. during period 1" What does this sentence mean?

Response:

The precipitation were 3.5 and 23.2 mm during period 1 in 2012 and 2013, respectively. Meanwhile, the upward EFs at 90 cm depth were 53.5 and 36.5 mm in 2012 and 2013, respectively. The sum of precipitation and EF was almost the same for the two years (2012: 3.5+53.5=57 mm; 2013: 23.2+36.5=59.7 mm), indicating the consistent water demand for evapotranspiration during Period 1.

The sentence has been revised to be clearer.

8. Discussion section: if salt balance will not be analysed, the discussion should be shortened significantly.

Response:

We have modified the Section 5 (Discussion) to make it more concise.

In this paper, the exchange flux and groundwater dynamics have been studied *based on the field experiments under water-saving irrigation condition. The results have been discussed in the sub-section "5.1.3 Balanced development stage", the key part of Section 5. Since the experiments were mainly implemented in the cotton field under mulched drip irrigation, information and data about other irrigation methods were mainly collected from the relevant literatures. The results from experiments and information from literatures were both discussed in the Discussion Section.*

While we acknowledge that our field experiments were specifically carried out in a cotton field under mulched drip irrigation condition, our results can indeed be QUALITATIVELY extended to other crop fields under different irrigation methods. With these extensions, we can discuss the interactions between social and hydrological systems in this hyper-arid inland oasis, which is also the purpose of this special issue 'Predictions under change: water, earth, and biota in the anthropocene'. Such broad perspective also can help us gain deep insight into the multifaceted effects of irrigation method conversion and achieve a sound policy for sustainable water management.

9. P1901-L13-15: "The results show that …. than in spring and autumn". Please provide which figure or table supports this conclusion.

Response:

The subsurface flow was analyzed in Section 4.2 (P1794, L 25 – P 1795, L 5) and this conclusion was drawn based on the Table 3.

The lateral flow out of the analysis zone during Periods 2 and 5 was expected to be high due to the recharge caused by irrigation and the high groundwater table. However, in Table 3, the outflow rate was only 1.0 and 3.0 mm day^{-1} during Period 2, indicating that the lateral flow into this zone was also significant. In fact, snowmelt happened during spring and summer, and the precipitation is also concentrated in the summer period in the mountainous areas. They resulted in significant subsurface flow into this zone during Period 2. Similarly, snowmelt in spring led to the subsurface flow into this zone, *resulting in the fact that the net lateral flow was larger during Period 3 than during Period 1.*

10. Figure 3: Define “IP” and “GWTD” here and for hereinafter use.

Response:

Done as suggested.

11. Figure 3: Make the width of bars equal to corresponding width of time interval. Space between bars was not easy to understand.

Response:

Done as suggested.

12. Figure 3: why downward EF occurred before next IP event between 5-13 and 5-20 of 2013?

Response:

Precipitation occurred on May 14 and 17, and downward EF occurred on May 16 and 17. The reasons for the mismatch of the dates may be that the soil water storage affected the EF and delayed the soil water movements, which should be further explored.

Reference

Zhang, Z., Tian, F., Hu, H., and Yang, P.: A comparison of methods for determining field evapotranspiration: photosynthesis system, sap flow, and eddy covariance. Hydrol. Earth Syst. Sci., 18, 1053-1072, 2014b.