Reply to Editor and Reviewers

Manuscript #	esd-2014-84
Title	Propagation of biases in humidity in the estimation of global irrigation water
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Thank you very much for taking the time to read our manuscript and for providing useful comments regarding the content.

We have revised the manuscript according to the comments received and have made the text clearer. As Referee #2 pointed out, the usage of the technical terms irrigation water demand (IWD) and irrigation water abstraction from rivers (IWAR) were confusing in the previous manuscript. For clarity and consistency, we have decided to report consumption-based water amounts instead of withdrawal-based amounts and have rerun all of the simulations. Consequently, not only the text but also the figures and tables have been completely revised.

We have also made major revisions in Sect. 2.2, to clarify the terminology we used and the analysis settings. The amendments that we have made are listed here, and we hope our manuscript has been improved to the extent that it can now be considered for publication in the ESD.

Yours sincerely,
Yoshimitsu Masaki (Corresponding author)

Comments from Referee #1 (Dr. Hempel)

Thank you very much for evaluating the manuscript and for providing encouraging comments.

1) *For the bias correction the author selected an additive approach. This is per se valid. It should, however, be noted that the other variables used as input (except for temperature) where corrected with a multiplicative approach. The latter preserves the relative trend in monthly data rather than the absolute one.*

--- The main reason for adopting an additive approach for the bias correction of relative humidity was to preserve the range of variability. Please note that potential evapotranspiration calculated with the physical formulae (see Appendix) is proportional to the humidity deficit, $e_{sat} - e$. Because $e = e_{sat} * RH$, potential evapotranspiration directly depends on the RH value. The variability of the relative humidity in an original GCM is preserved by an additive approach (with an incremental ΔRH), whereas the variability of the relative humidity (with a multiplier α) is amplified by a scale factor of α. That is,

$$RHcorrected = RHuncorrected + \Delta RH$$

$$var(RHcorrected) = var(RHuncorrected) \qquad \text{[i.e., independent of } \Delta RH\text{]}$$

$\therefore$ var(PotEvapTrans corrected) is proportional to var(PotEvapTrans uncorrected) for an additive approach, and

$$RHcorrected = \alpha * RHuncorrected$$

$$var(RHcorrected) = \alpha * var(RHuncorrected)$$

$\therefore$ var(PotEvapTrans corrected) is proportional to α * var(PotEvapTrans uncorrected) for a multiplicative approach.

A multiplicative approach strongly depends on the uncorrected value. If the same multiplier $\alpha = 1.5$ is applied to a set of data (50% RH, 25% RH), we obtain the corrected humidity as (75% RH, 37.5% RH). The incremental correction δRH (= RHcorrected – RHuncorrected) amounts to (+25% RH, +12.5% RH). That is, the multiplicative correction is strongly sensitive to the original magnitude of RHuncorrected.

In the revised manuscript, we have added a brief explanation of this at the beginning of the 2nd paragraph in Sect. 2.4.2 (P12 L5--L7).

2) **(A)** *A general problem of correcting relative humidity is that the values shall be*

bounded to the interval 0 to 1. Question is how to tried values out of this range. The authors decided to just cut this values. Althogh it is not mentioned explictly, I suppose this means setting values <0% to 0% and thoses >100% to 100%. (B) *This is a common precedure, but it should be noted somewhere how often thoses cuts are required (percentage of cutted values relativ to all bias corrected values).* (C) *Moreover, values above 100% are not necessarily unphysical. Supersaturation does to some extend occur in individual GCMs. It should be checked and noted for the 5 GCMs used here how often supersaturation occured already in the not bias corrected data sets.*

---(A) Thank you. This is already shown on P91 (L14-15) in the previous ESDD manuscript (P12 L13--L14 in the revised manuscript).

(B) We have conducted a new analysis of the statistics, and have included the results in the revised manuscript in the last half of Sect. 2.4.2, together with a new Table 2. The number of such truncations depends largely on the season, latitude (i.e., atmospheric temperature), and the GCMs. A large number of truncations at 100% RH occur in low temperature (< 0 [degC]) conditions, typically at high latitudes in the winter. However, we consider that the errors arising from these truncations are very marginal in our analysis because (1) in such low temperature conditions, the absolute value of evapotranspiration is extremely small and approaches zero, (2) crops are not cultivated at high latitudes in the winter, and (3) irrigated croplands are less widely distributed at these latitudes. The discussion above has been added in the 3rd and 4th paragraphs of Sect. 2.4.2.

(C) Thank you for this insightful comment. We agree that supersaturation itself is not necessarily unphysical. In particular, cloud formation processes in the upper troposphere (such as cirrus) are often accompanied by supersaturation. Supersaturation near the surface is less frequent, but it does occur.

We have added statistics regarding the frequency of supersaturation in the original humidity data in the last half of Sect. 2.4.2 and Table 2a of the revised manuscript.

Comments from Referee #2

In general, the study setup is well designed and aims at on open research question suitable to be published in ESD. However, the conceptual design involves some *drawbacks and, to my regards, some mistakes, that are not discussed in the manuscript and might affect the overall study results. First, I present some general concerns, thereafter I relate to more detailed comments and questions. I suggest a major revision to address these points.*

--- Thank you for your comments, which we note are mainly from the perspective of irrigation water. The definition of the terminology we used was insufficient and there was a misleading mixture of the consumption- and withdrawal-based amounts of water. This might have led to confusion you and prevented the reader from fully understanding the text.

We have made major revisions to amend these points, with the complete replacement of figures and tables related to irrigation water. We have revised and enlarged Section 2.2 to clearly convey the terminology to readers. We have also numerically reported the consumption-based amount throughout the manuscript. We believe that the mistakes you found in the previous manuscript have been successfully corrected by these major revisions. Although the main target of this study was to determine biases in atmospheric humidity and their propagation into hydrological variables, we hope that these major revisions will also be welcomed by potential readers in related fields of research, such as agricultural water management.

- (1-A) *Simulated irrigation water demand is only based on Epot, soil moisture and precipitation is not accounted for.* (1-B) *Furthermore, it is assumed that actual evapotranspiration (Eact) always equals Epot. I do not agree with this assumption, but one could argue that it holds true for the IWD scenario (potential irrigation water demand).* (1-C) *However, the study also relies on this assumption for the IWAR scenario (actual irrigation water abstraction), which is constrained by local water scarcity and thus Epot cannot equal Eact by definition. Accordingly, Eact is likely to be less dependent on humidity and thus the bias effect on irrigation water demand might be overestimated here.*

---(1-A) We are afraid but this is not the case, because IWD was evaluated based on the soil water deficit by solving a water balance. We have explained this in the last half of Sect. 2.2 in the revised manuscript. We have also revised the Introduction (P3 L6) and the last paragraph of Sect. 2.3 to clearly present our analysis settings to readers.

IWD is the amount of water that needs to be supplied to keep soil moisture at the target level ($0.75W_{fc}$ except for rice) during a growing season. IWD is affected not only by Epot (outflow from soil moisture) but also by soil moisture and precipitation (inflow to soil moisture). In other words, IWD is not calculated as the Epot*duration; rather, IWD indicates the potential amount of water required to compensate the soil water deficit.

(1-B) The assumption $E_{act} = E_{pot}$ only holds over irrigated cropland during the growing season under the IWD simulation. It is idealistic, but has proved rather useful for global-scale simulations and has been widely adopted in earlier research (e.g., Döll and Siebert, 2002). See also our later reply to your comment regarding P88L19(B).

(1-C) As mentioned above, we defined Eact = β * Epot (as Eq. (B4) in Hanasaki et al. (2008)). We assumed that irrigation water was primarily abstracted from rivers if riverine water was available, but was otherwise supplied from non-renewable blue water. Thus, β was always 1.0 over the irrigated area during a growing period, which allows Eact=Epot to hold.

In the previous manuscript, we did not fully describe the supply from non-renewable blue water. In addition, we used withdrawal-based quantities and two different analysis settings for IWD and IWAR. This was very confusing.

In the revised manuscript: (i) we have clearly mentioned the priority order of water abstraction water resources, including non-renewable blue water (P6 L16--L19 and P8 L2--L9), (ii) we have introduced consumption-based IWCR instead of the former IWAR (P8 L1--L9), and (iii) IWCR is defined as the portion of IWD that is available from rivers (P8 L1--L9). See also our reply to your comment regarding P88 L19(A). From these revisions, readers can easily obtain a clear image of IWCR and easily understand the quantity in a unified manner with IWD.

We agreed with you that IWCR is less sensitive to humidity. On the other hand, the sustainable use of surface blue water under future climate conditions is a central research issue in the analysis of the impacts of climate change. We consider that IWCR can be used as an indicator of surface water availability for irrigation. If the humidity biases affect IWCR, the bias correction of humidity will propagate into the surface water availability. If this is the case, the bias correction will be important not only for estimating evapotranspiration but also for considering future surface water availability.

We have also expanded and revised the description on IWCR in the last half of Sect. 2.2. Through these revisions, we believe that the new manuscript has been made more consistent.

- (2-A) *Moreover, why is it important to study bias impacts on potential withdrawal amounts? Since it is an artificial value (in this study: 3129km3/yr, ensemble mean, year 1971-2000) the impact of humidity bias might be much smaller in reality.* (2-B) *I assume it might be more relevant to investigate the bias effect on actual water consumption, since much of abstracted water returns to the riverine system.*

--- (2-A) In the revised manuscript, we have used consumption-based quantities, which directly reflect water consumption in the cropland, rather than withdrawal-based quantities. Moreover, we have shown IWCR, as well as IWD, because IWCR indicates water consumption with the availability of riverine water.

We agree with you to an extent, in that IWD indicates the "potential" value. It is a challenging task to obtain a completely "realistic" amount of water at the global scale, because irrigation water is abstracted not only from rivers but also from groundwater and local reservoirs. However, we understand that IWD is the next-best indicator to represent the global amount of irrigation water. In fact, IWD has been used to evaluate the global amount of irrigation water in earlier works (e.g., Döll and Siebert (2002), Wada et al. (2013)). Because IWD indicates the maximum amount of water required for irrigation under the condition that crops do not suffer from water stress in the existing irrigated cropland, IWD can also be used to estimate how much additional water would be required to maximize agricultural production under water stress-free conditions. An understanding of future potential water demand would also be useful for the impact assessment of climate change.

(2-B) Thank you for your comment. Following your advice, in the revised manuscript, we have reported consumption-based quantities throughout the manuscript. We have also amended the mixture of withdrawal- and consumption-based quantities in the previous manuscript, as you point out in (3-B) and elsewhere.

- (3-A) *It is unclear from the manuscript how irrigation water abstraction is derived, how it is calculated, and how irrigation efficiencies are incorporated.* (3-B) *There is a conceptual error with the definition of IWAR (defined here as water abstraction, diverted from rivers, section 2.2) but it is later argued that IWAR is in line with irrigation water consumption from other studies (p. 94 l. 25). This indicates that actual irrigation water abstractions simulated here (1269 km3/yr ensemble mean, year 1971-2000) are substantially below competing studies (irrigation water water withdrawal/abstraction estimates are generally ~2200-2500 km3/yr, and consumption ~1100-1400 km3/yr; e.g. Wada and Bierkens, (2014), Döll et al., (2014)).* (3-C) *More* *generally, in the manuscript target variables need to be defined more clearly and transparently in respect of recent literature (withdrawn/diverted irrigation water vs. consumed/depleted irrigation water, role of returnflow, irrigation water losses (beneficial and non-beneficial) etc.). As it is correctly cited in the introduction from Vörösmarty et al. (2005).*

---(3-A) Thank you. In line with your comments (1-A) and (1-C), we have augmented the description of IWD and IWCR in the last half of Sect. 2.2 in the revised manuscript. We believe the methods used to estimate irrigation water abstraction are now clearly presented. Because the amount of water is reported as a consumption-based quantity rather than a withdrawal-based quantity in the revised manuscript, we have removed the description of the irrigation efficiency.

(3-B) In the previous manuscript, withdrawal- and consumption-based irrigation was occasionally mixed up. We apologize for this error and thank you for your comment. Our IWDs (consumption-based) now range between 1,145 km^3/yr and 1,312 km^3/yr for 1971—2000, which is within the range of earlier works (1,100—1,400 km^3/yr). As we noted in our reply to (1-B), we have revised the text and now use a consumption-based water amount throughout the manuscript.

(3-C) In line with your other comments, we have added a description of IWD and IWCR in the last half of Sect. 2.2. We have also changed the terminology to be consistent with those used in recent papers. We have unified the terminology, such as "water consumption (instead of <lost> in the previous manuscript) by evapotranspiration", according to your suggestion.

We believe that the mistakes you found in the previous manuscript have been successfully corrected by these revisions.

- (4-A) *The study lacks a quantification to what extend bias-corrected GCM simulations of precipitation still affect IWD/IWAR estimates. It is concluded that bias in humidity is the only factor explaining the range in irrigation water demand across GCM outputs during the reference period, but also in future climate scenarios.* (4-B) *For the reference period the precipitation effect might indeed be small, but it needs to be shown, as it is done for air temperature.* (4-C) *For future time periods this assumption might not hold true, since GCMs develop individual precipitation patterns, which strongly affect irrigation water demands (at least in other studies).*

--- (4-A) We did not intend to state "bias in humidity is the only factor for explaining the range in the irrigation water demand" for both the past and future periods. As summarized in Tables 4 and 6, the use of different GCMs or different RCPs also leads to a range of estimated values of irrigation water (e.g., Wada et al. (2013) and Haddeland et al. (2014) reported differences between GCMs and RCPs). As we also explained in the Introduction, the biases in temperature and precipitation also affect hydrological variables. (e.g., Haddeland et al. (2012) discussed the compound effects of temperature-precipitation-radiation-humidity effects on hydrological variables). However, the meteorological forcing data we used in this study were bias-corrected based on the method proposed by Hempel et al. (2013). Only humidity remained uncorrected. We do not intend that the effects of biases in humidity overwhelm those in temperature/precipitation.

To convey our standpoint to readers correctly, we have revised the Introduction (P4 L21--P5 L2), Conclusions (P26 L28--L29) and elsewhere (e.g., P18 L24--L26). We have also added a brief explanation of our standpoint again at the end of the 1st paragraph of the Conclusions (P26 L1--L4). We believe the revised conclusions are improved and well-balanced.

(4-B) In the revised manuscript, in line with your comments regarding P124 Fig.5 (later), we have added similar maps for precipitation (Fig. 6). The corresponding explanation is given in the 5th paragraph of Sect. 3.1. These revisions, in addition to the existing monthly profiles of the biases in precipitation in Fig. 2, provide a large amount of information to readers regarding the precipitation effect.

(4-C) We agree with you that even if GCM biases were corrected during the past period, because future change trends have GCM-inherent characteristics, the temperature and precipitation for future periods may differ among GCMs. Thank you for your observation.

To fill the logical gap you have suggested, we have made in the following three revisions.

First, we have added monthly profiles of future meteorological elements projected by the GCMs in Fig. 7 to show the magnitude of differences in the future temperature and precipitation between the GCMs.

Second, we conducted "a sensitivity experiment with a hypothetical +/- 5% RH" for the future periods. This experiment was able to extract the effect attributable to differences in humidity, because other meteorological forcing data were identical in three experiments with different conditions (-5% RH, original, +5% RH). By showing the sensitivity to humidity for the future periods, we demonstrated that sensitivity in the future is similar to the present, even if future temperature and precipitation differ among the GCMs due to GCM-inherent variations. We chose RCP8.5 because it was expected to have the maximum GCM-inherent variations among the four RCPs. We have added a new Sect.3.4.2 to explain this.

Third, as shown in Fig. 9, the monthly patterns in the anomaly for potential evapotranspiration or other hydrological variables had an opposite phase to that for the relative humidity, even for the future period.

Moreover, we have separated the discussion for the future period from that of the present in the 2nd paragraph of Sect. 4.1. Because we have also mentioned the GCM-inherent climate change trends in this section, we believe the discussion is now well-balanced.

We have revised the manuscript to carefully address your concerns. We believe that these results have strengthened our initial statement.

- Stylistic language editing of the manuscript might straighten some unclear or imprecise formulations.

--- We have revised the manuscript to provide clear and precise information to readers. Please note that both the original and revised versions have been edited by a professional English editing service.

More detailed comments:

- It is not common to use the term "irrigational water" or "irrigated water", as it is used here it is sometimes arbitrary. I suggest to use more precise terms that are commonly used, e.g. "irrigation water demand/consumption/withdrawal" etc.

--- Thank you for your comment. We have used the phrases "irrigation water demand/consumption/withdrawal" when we discuss the amount of water.

- Check consistency of tense in manuscript

--- We have corrected the errors. The manuscript has been checked by a professional English editing service before submission.

p 82 line 11: unclear formulation, meteorological data sets relate to GCM outputs?

--- We have revised the expression (P2 L9--L12).

p 83 line 7: irrigation water is not "lost" through evapotranspiration, it is depleted or consumed, but only water that is non-beneficially consumed, i.e. "lost" during conveyance or irrigation application (soil evaporation etc.) con be considered losses.
--- Thank you for identifying out our confusing terminology. We have used "consumed" instead of "lost" in the revised manuscript (P3 L7 and elsewhere).

p 84 line 3: In addition to model parameter estimates and issues with input data, irrigation systems itself are in most models not sufficiently represented and only rough indicative estimates for irrigation efficiency are employed.
--- We agree with your opinion. Thank you for the comment. We have mentioned the irrigation scheme in the revised manuscript (P3 L25--L27).

p83 line 20: references needed here
--- We added references (Wada et al. (2013), Bruinsma (2011) and Elliott et al. (2014)) at this point in the revised manuscript (P3 L16--L19).

p 86 p5: Please give a reason why you don't use the reservoir module, I assume abstraction rates would be more realistic using it?
--- In this study, we focused primarily on the relationship between humidity biases and irrigation water demand. The next step will be to assess how to meet demand taking riverine and other water availability into account.

p 86 line 12: "The human impacts of irrigational, municipal and industrial water abstraction from rivers for consumptive use were determined." How were they determined?
--- We avoided a detailed explanation by citing Hanasaki et al. (2008a, b); however, this was inconvenient for readers. In the revised manuscript, we have provided summarized descriptions in the 1st paragraph of Sect. 2.2 (P6 L19--L24).

p 86 line 18: Which crops/crop types are simulated by H08?
--- We did not explain this in the previous manuscript. We have added an explanation at the end of the 2nd paragraph of Section 2.2 (P7 L13--L16).

p 86 line 19: "The soil water of cropland is lost through evapotranspiration and runoff." better use depleted instead of lost.

--- In the revised manuscript we have used "consumed" (for consistency with our reply to your comment regarding P83 L7) instead of "lost" (P7 L2--L3).

p 86 line 20: Please clarify why evapotranspiration estimates are not linked to soil water content.

---Thank you for your comment and we apologize for confusing you. During the editing of the text before submission, the words linking to "the former" were omitted. As we explain in Sect 2.3. (eqs. (1) – (3)), our evapotranspiration scheme depends on the soil water content.

We have corrected the explanation in the revised manuscript (P7 L3--L5).

p 86 line 23: There are more recent publications on the extent of irrigated cropland. Please clarify why you do not use e.g. Siebert et al. 2014: A global dataset of the extent of irrigated land from 1900 to 2005.

--- The simulation setting here was designed prior to the publication of Siebert et al. (2014). Please note that the Siebert et al. (2014) paper appeared online as an ESDD paper on December 2, 2014, only 20 days in advance of our manuscript submission (December 25, 2014).

We believe Siebert et al. (2005) still provides valuable data regarding irrigation. We fixed the land use data at 2000 because the main target of this paper was humidity biases. The fixed conditions were beneficial for extracting humidity bias effects, without interfering with other socio-economic historical changes.

p 86 line 25: Double-cropping information is based on what kind of data?

--- Thank you for this question. The information was based on Döll and Siebert (2002), and we have added this to the revised manuscript (P7 L10--L11).

p 86 line 27: Mosaic 4, what "other" uses does it represent? It is not shown along with the other mosaics in Figure 1, it is not mentioned in any Table or Figure. If not important, consider to skip.

--- Thank you for this comment. The use of Mosaic 4 might confuse readers because it refers to non-cropland. We have removed it from the revised manuscript (P7 L6--L10).

p 87 line 4: Why is irrigation water consumption consequently not shown in this study? I'd assume it is very important to show the effect of humidity bias on depleted water, maybe even more important than diverted water, since half of it returns to the riverine *system.*

--- Thank you for your suggestion. As mentioned in our earlier replies, we have revised the text and now use a consumption-based amount of water throughout the revised manuscript.

p 87 line 5: "…irrigational water demand (IWD) and simulated irrigation water abstraction…" IWD not simulated?

--- Both are simulated. The word was incorrectly placed in the original text. Because our analysis is based on a hydrological simulation, there is no need to declare it here. We have removed "simulated" from the revised text (P7 L17--L19).

p 87 line 6: Please elaborate how IWD and IWAR are derived. How are efficiencies from Döll and Siebert et al. (2002) incorporated?

--We have provided a detailed explanation of IWD and IWCR in the last half of Sect. 2.2 in the revised manuscript. Because we use a consumption-based amount of water in the revised manuscript, we have removed the description of irrigation efficiency from this line.

p 87 line 10: Water is not only lost through conveyance, irrigation efficiencies also account for losses during water application to the plant. But it is important to separate consumptive losses from return-flow, which might be recoverable for downstream users. Please clarify how you incorporate indicative values for water use efficiency by Döll and Siebert (2002). These values do not reflect that a substantial amount of "lost" water remains in the system.

--- Thank you for this comment. In the revised manuscript, we have completely replaced the old description of withdrawal-based IWD and IWAR with a consumption-based IWD and IWCR. Through these major revisions, the problematic descriptions that you pointed out have been removed.

p 88 lin 19: This is one of the more important critique points. (A) *Eact = Epot can only hold true for IWD, right? In IWAR simulations, W might fall below 0.75 in case of local surface water scarcity.* (B) *More generally, I do not agree that Eact is not constrained by soil water content. In this regard the irrigation type is very important, even when using surface irrigation systems, the surface soil layer might not be saturated anymore soon after the irrigation event, while the root zone is still well watered. In this case soil evaporation is reduced and Eact does not equal Epot. Using drip system further* *decreases soil evaporation.*

(A) Thank you for your careful reading of our manuscript. We understand that readers are obliged to understand two simulation settings for IWD and IWAR, which might be confusing.

Through the major revision of the analysis of irrigation water, we introduced IWCR as a portion of IWD and used the consumption-based demand that can be supplied from rivers (the last paragraph of Sect. 2.2). Through these changes, we believe that IWD and IWCR can be understood based on a unified concept. This is less confusing and beneficial for readers because our primary focus was on the effect of biases in humidity.

(B) Your comment is correct and reasonable for plot-scale hydrological simulations; however, it is very difficult to collect information regarding irrigation type at the global scale.

For example, farmers alter their irrigation management depending on cultivars, growing stages, and seasons, e.g., repeated inundation/drainage of water during the growing period. Water use also varies with irrigation types (sprinklers, drip system, ditch inundation). We understand that such water-saving irrigation technology, which is particularly important in water-limited areas, is also taken into consideration in some pioneer studies (e.g., Döll et al. (2014) *Water Resour. Res., 50, 5698*) that have been published recently. This information might be available at plot- to regional-scales, but is not available at the global scale.

In the revised manuscript, we have mentioned this in the last paragraph of Sect. 2.3, and also in the 3rd paragraph of Sect. 4.3 (P25 L5--L7) to ensure that our discussion is well-balanced.

p 89 line 12: "(crop type, crop calendar, etc.)" In my opinion, most important are irrigation management and irrigation technique.

--- We have explicitly mentioned irrigation management/technique in the revised manuscript (P10 L6--L8).

p 92 line 15: Although soil moisture is not considered for calculating irrigation water demand, what is the contribution of different precipitation regimes, even though biascorrected?

---We considered soil moisture conditions for calculating IWD because this gives the total amount of water required to compensate the soil water deficit when it drops below 0.75Wfc during the growing season. Thus, when heavy rainfall continues for a couple of days, the croplands do not need to be irrigated because the soil moisture is above the threshold. When the soil moisture drops below the threshold, irrigation is resumed again.

p 93 line 12: "That is, small humidity biases over irrigated cropland are beneficial for suppressing their effects on irrigational water." Unclear formulation

--- We have added a description before this sentence to fill the logic gap (P15 L1--L5).

p 94 line 25: This is wrong! You compare water withdrawal/abstraction from your study with water consumption from other studies. Please clarify. Please elaborate why you arrive at withdrawal amounts way below competing studies. Roughly speaking, agricultural consumption is generally assumed to amount to ~1100-1400 km3/yr and agricultural water withdrawal to ~2200-2500 km3/yr.

--- Thank you for your comments. We have revised the text and use consumption-based values in the revised manuscript (2nd paragraph of Sect. 3.3).

p 98 line 18: This is wrong! Bias in GCM precipitation is small during reference period (needs to be shown!), but GCMs develop individual precipitation patterns in future scenarios. These individual patterns strongly affect irrigation water demands.

--- Thank you, there was a logic gap in the previous manuscript. In the revised manuscript, we have separated the discussion between the present and future periods. Please see the details in our reply to your comment (4-C). We have added new results from the sensitivity experiment for the future period to fill the logic gap (Sect. 3.4.2 and 2nd paragraph of Sect. 4.1). Figures 6 and 7 have also been created to support our logical reasoning.

p 124: show Figure 5 also for precipitation

--- We have added a similar map for precipitation (Fig. 6).

p 125: why IWAR only for MOSAIC0? not clear from Methods section.

--- This is due to a restriction of our current code. We are developing a new version of the simulation code that implements the scheme for each mosaic.

p 129: Why only HadGEM shown? What is the reason to select HadGEM? Why only June and August? Monthly sums might be more informative.

--- In this figure, our intention was to show the different characteristics at monthly scales. To show several tens of figures of the results obtained with the 5 GCMs for each month would cause the key message from the figures to be unclear. There are differences between the GCMs, but they are relatively small. Thus, we chose HadGEM as an example.

We consider that the two example months of June and August are informative and allow an understanding of the differences arising from the rainy/dry seasons in the Asian monsoon region. Monthly summations would remove the monthly dependence. Additionally, for the purpose of examining the differences in the rainy/dry season, it is important to choose results only two months apart between June and August, and with relatively small temperature differences between the two months because the sensitivity is expected to be dependent on the temperature.