Authors' Response to Reviews of

Shifting water scarcities: Irrigation alleviates agricultural green water deficits while increasing blue water scarcity

Review #2

General comments

This paper sheds light on the interlinkages between green (GWS) and blue water scarcity (BWS) as simulated by the global gridded vegetation model LPJmL. The results are presented for the 2015–2019 period with a sufficient level of detail and supporting visualisations. The main finding suggests that irrigation helps humanity to alleviate GWS impacts on crops, but this comes at the cost of increasing BWS. As such, this paper contributes to the growing body of literature highlighting the need to analyse both green and blue water use simultaneously when addressing water stress impacts on crop production worldwide.

However, as always, there are multiple things the authors can improve upon. My main concern lies in the definition of GWS and how robust the results are. Choosing 0.2 as the main GWS threshold seems rather arbitrary and leaves me wondering how different the results would be if the authors had chosen 0.1 or 0.3 instead (I explain below in detail). Adding to this, there is no clear section on the validation of the underlying global estimates by LPJmL. The full list of potential issues to address is provided below.

We would like to thank Reviewer 2 for the thorough and detailed review. We greatly appreciate the constructive feedback and insightful questions, which will significantly contribute to improving the quality of the paper. We provide below our point-by-point responses together with our proposals for implementing the suggested improvements.

Specific comments:

Regarding the definition of GWS:

1. Authors use the 0.2 GWS threshold as earlier applied by Rosa et al. (e.g. https://doi.org/10.1093/pnasnexus/pgad117). The assumption is that such a level of GWS would force farmers to irrigate to avoid large yield declines. However, this assumption is rather arbitrary and does not rely on actual crop yield decline simulations (as you also acknowledge in L245-246). As far as I know, LPJmL is capable of simulating crop yields, so why not using those to define reasonable thresholds per crop functional type (CFT)? I can imagine that some crops indeed can have substantial yield declines at 0.2 GWS, but others might not be so sensitive.

We thank the reviewer for this very valuable comment. We acknowledge this limitation, and to address it, we propose a revised approach that derives green water stress thresholds directly from simulated yield responses – as described in the summary response to all reviewers. With this approach we intend to ensure a closer and more meaningful linkage between green water stress levels and their agricultural impacts. Our preliminary findings indicate differences in the CFT yield response with regard to GWS: while many CFTs exhibit yield losses of approximately 20–30% at a green water stress level of 0.2 under rainfed conditions, certain CFTs (e.g. pulses and temperate cereals) experience considerably higher losses under similar stress conditions.

We will describe this enhanced methodological framework in detail in the Methods section and compare the resulting CFT-specific thresholds, as well as the associated stressed agricultural areas, with the previously applied uniform thresholds of 0.2 and 0.4. Finally, the Discussion section will address the implications and limitations of this refined approach.

2. Connected to the above, consider reframing the analysis from only two main GWS categories (0.2-0.4 and 0.4-1.0) to sequential steps of 0.2 or 0.25 to have graduality in GWS exposure (e.g. no GWS, minor, moderate, severe). Authors already do this for Fig. 4, for example, why not in the main text and Table 1 too?

Thank you very much for this proposal. Connected to the adapted new approach, we will also reframe the GWS thresholds/categories to sequential steps of 0.2, in order to show a graduality in the GWS exposure. We will define the thresholds as following:

0-20 % yield decline = low GWS
20-40% yield decline = moderate GWS
40-60% yield decline = high GWS
60-80% yield decline = severe GWS
80-100% yield decline = extreme GWS

We will adjust the text accordingly and include a new figure that illustrates the CFT-specific green water stressed areas.

3. Authors define green water as "*the plant available rainwater held in soils which sustains the growth of crops and pastures*", and then they also state that "*irrigation is shown to alleviate green water stress*". I find this a bit confusing. If green water = rainwater, then how come irrigation can alleviate GWS? The total water stress (from insufficient green+blue water supply) can indeed be reduced by supplementary blue water supply via irrigation, but green water scarce areas remain green water scarce by definition of what green water is. There is still not enough rainwater in the soil even after adding irrigation, and this statement would hold unless the precipitation patterns and/or soil texture change. Therefore, please consider rephrasing the parts where such confusion can occur or perhaps use total water stress (or some other general term) to make the distinctions clearer.

Thank you very much for pointing out this inconsistency. We propose adjusting the green water definition to: "plant available soil moisture from rainfall and snow melt which sustains the growth of crops and pastures." We further acknowledge that blue water cannot alleviate GWS, but that it can alleviate/compensate for plant water stress if GWS is high. With irrigation, GWS remains unchanged but the stress level that plants experience is alleviated by supplementing blue water. We will rephrase the parts accordingly.

Regarding the definition of BWS:

1. Is there a particular reason why water use (as I understand, that means withdrawals) as opposed to water consumption (actual water volume removed from a catchment) is used? To my knowledge, most recent BWS studies use the latter since a large part of the withdrawals stays within the same catchment, and thus, should not contribute to BWS in a long term.

Thank you for this valuable comment. In the original manuscript, we did not apply a consistent definition of water use: WU_{irr} was calculated with LPJmL and represents the applied irrigation water at the field whereas WU_{dom} and WU_{ind} represent water consumption. In response to the feedback, we will recalculate WU_{irr} to consistently represent irrigation water consumption as follows:

$WU_{irr} = WD_{irr} + E_{conv} - RF_{blue}$ *(Eq. 5),*

where WD_{irr} represents *the applied irrigation water* which depends on the simulated irrigation system*,* E_{conv} *the evaporative conveyance loss* and RF_{blue} *the water return flow.*

The global average annual irrigation water consumption in our time period 2015-2019 is approximately 1213 km^3yr^{-1}, which is comparable to estimates calculated in other studies (e.g. Mc Dermid et al., 2024: 1195 +/- 99 km^3yr^{-1}). We assume that the blue water stress will be highly influenced by this change and will decrease accordingly.

2. It is not clear where water use estimates for domestic and industrial use are taken from, and whether they account for monthly variability. Please add more details.
3. It is not clear whether the authors consider groundwater withdrawals as well as livestock, electricity generation, and mining water supplies. Please elaborate.

Thank you for this important question. In our study, we used the dataset from Flörke et al. (2013) as input, which includes the water consumption of households, industry and livestock. In response to the reviews we propose to change the input and use a dataset by Huang et al. (2018) instead which is more up-to-date and includes more sectors. This dataset was created by spatially and temporally downscaling national (and U.S. state-level) sectoral water-withdrawal estimates from AQUASTAT and USGS, providing a monthly 0.5° gridded dataset for the period 1971–2010. It includes the sectors domestic, electricity generation (cooling of thermal power plants), livestock, mining, manufacturing and irrigation. For our updated analysis, we will use the data for the first five sectors as input to LPJmL (whereas consumptive water use for irrigation is computed internally).

We will update the Method section accordingly and further acknowledge data gaps and time coverage as limitations in the Discussion section and elaborate on how the inclusion of additional sectors and longer time periods could influence the results. We expect blue water stress to (slightly) decrease because global industrial water withdrawal estimates by Flörke et al. (2013) are higher than estimates of Huang et al. (2018).

Regarding the model setup:

1. I find the description of LPJmL inputs in the main text insufficient. This study relies entirely on results from this model, so, as a reader, I would expect to have a very

detailed description of input data and main assumptions in Section 2.2, and validation, limitations, and uncertainties in Section 4. Please try to elaborate. I provide several suggestions below.

We acknowledge that the description of the model setup as well as of the input data can be enhanced. We will improve section 2.2 accordingly by describing the LandInG toolbox as well as the input data in more detail.

2. In addressing 3a, I would recommend moving Table S1 into the main text, as a reader needs to see the main input data sources and their description. Also, consider adding a column with spatial resolution next to "Time period".

Thank you for this suggestion which we will adopt.

3. When describing the land use and CFTs, please explain:
   - where the LandInG toolbox gets rainfed/irrigated crop maps from, and whether those maps cover spatiotemporal changes

We thank the reviewer for this relevant question. The LandInG toolbox derives rainfed and irrigated crop maps from a combination of country-level and gridded datasets, including FAOSTAT, MIRCA2000, AQUASTAT, MON, RAM, and HYDE. Country-level datasets provide information on crop-specific harvested areas and changes over time, while gridded datasets provide spatial detail or temporal dynamics but may lack crop-specific information. LandInG integrates these sources to create a harmonized, gridded dataset at 0.5 resolution, covering 1500–2017, by disaggregating country-level data to grid cells and resolving inconsistencies between datasets. This processing allows the toolbox to capture spatiotemporal changes in rainfed and irrigated crop distributions while maintaining consistency with crop-specific and irrigation-specific information available at coarser, e.g. national, resolution. We will explain the LandInG toolbox in more detail in section 2.2 accordingly.

   - where crop calendars are from, and whether they are static or dynamic

We use the GGCMI Phase 3 crop calendar described in Jägermeyr et al. (2021) which is static in sowing dates and variety parameters, meaning that seasons start the same day in each year, but the length of the growing season is variable according to temperature variations. We will add these details to the revised Method section.

   - what share of primary crops from the crop list provided by FAOSTAT is covered (otherwise, it is not clear what your scope is), see L85-87. Also mention if perennial crops are covered.

We adopt 100% of the cropland area from the HYDE database, which corresponds to 100% of the FAO cropland. However, a significant share of this area may be classified as the category "others", which aggregates all crops not parameterised specifically as CFTs. Many FAOSTAT primary crops fall under this "others" category, e.g. all FAO primary crops of the category "Seeds & Oils" except sunflower and rapeseed, and also many vegetables. "Others" include also some perennial crops, e.g. coffee, cocoa and tea. The 12 parameterised CFTs cover ≈ 60% of the global agricultural area while "others" cover ≈ 40%. We will provide more detail on these aspects in the revised Method section, and mention in the Discussion how the less specific parameterisations may influence results.

4. When describing the soil, please specify whether the root zone is dynamic or kept static over the growing season, mention how many soil layers are simulated, and whether the shallow groundwater is considered (since capillary rise can support some rainfed crops).

In our model setup, the root zone is kept static over the growing season. The soil column is represented by six layers, of which five are hydrologically active, with depths of 0.2, 0.3, 0.5, 1.0, and 1.0 m (see Schaphoff et al., 2018). While shallow groundwater is implicitly considered through the implementation of baseflow, capillary rise is not represented. We will incorporate these clarifications into the revised manuscript.

5. When describing the irrigation, clearly state what irrigation systems are considered (furrow, sprinkler, drip), how and when irrigation is applied, and whether conveyance losses are simulated.

We thank the reviewer for this helpful comment. In our study, each country is assigned a share of the three irrigation systems (furrow, sprinkler, and drip) following the approach of Jägermeyr et al. (2015). These national-level shares are further disaggregated to the grid cell and crop functional type (CFT) level using a decision-tree approach that accounts for crop- and soil-specific suitability. Irrigation is applied according to these distributions, which are updated annually. Conveyance losses are simulated and depend on the irrigation systems as well as soil conditions. For pressurized systems (sprinkler and drip irrigation), the conveyance efficiency is set to 0:95 while for surface irrigation conveyance efficiency is connected to different soil saturated hydraulic conductivities (see Jägermeyr et al., 2015 (Table 1)).

Regarding validation, limitations, and uncertainties:

1. As mentioned earlier, I would expect to see a sub-section on validation, limitations, and uncertainties in Section 4. The presently provided comparisons mainly look at GWS and BWS in terms of hectares. However, the underlying gridded estimates of green and blue ET, as well as sectoral blue water demand, are not properly validated. Please add such comparisons (global total and/or gridded levels), maybe a brief summary for the main text and an elaborated version in SI. Otherwise, it is difficult to judge whether the LPJmL model outputs are reliable before even diving into GWS/BWS analysis.

Thank you very much for this valuable suggestion, we agree that a sub-section on validation, limitation and uncertainties in the Discussion section will be beneficial, so we will add it.

2. For green and blue ET (i.e. water consumption) estimates, you can have a look at https://doi.org/10.1038/s41597-020-00612-0 and https://doi.org/10.1038/s41597-024-03051-3, but there also might be more studies published recently.

We propose to expand the global overview table in the manuscript (Table 1) to include additional hydrological variables, such as evapotranspiration. We will also check spatial patterns and discuss them in comparison with findings from other studies (e.g. those suggested here). We already validated the evaporation rates of this study with evaporation rates measured at eddy flux towers (in the Supplementary Material) and will point to this more clearly.

3. Sectoral blue water use can be obtained from global hydrological models provided by ISIMIP as well as AQUASTAT

The sectoral water use input dataset of Huang et al. (2018) that we propose to use instead of the input dataset of Flörke et al. (2013) is supported by observational studies and well evaluated through uncertainty analyses (Huang et al., 2018). We will discuss these results in the revised manuscript.

4. Lastly, I would recommend having an additional sub-section describing 1) main limitations and uncertainties (coming from input data, LPJmL-specific ones, etc.) and 2) discussing how those limitations and uncertainties affect the reliability of the results. For example, how BWS could change if the authors used a water consumption-based approach instead of water withdrawals or how different EFR methods could affect the results. No need to run additional simulations to provide an uncertainty range, but it is always a good practice to be open about the weaknesses of the selected methodology, so the follow-up studies can address those.

Thank you for this helpful suggestion. We will add a dedicated sub-section in the revised manuscript outlining the main limitations and uncertainties, including those related to input data and LPJmL-specific assumptions. We will also discuss how these limitations may affect the reliability of our results.

Technical corrections:

1. L13: Authors define GWS as “soil moisture limitation on crop growth” in the abstract without mentioning that it only concerns rainwater.

As mentioned above, we propose to adapt the definition of GWS and will adjust it here accordingly.

2. L30: Please consider using more recent publications to support your statement.

We added two more recent references: Mialyk et al., 2024 and Chukalla et al., 2025.

3. L32: Authors mention green water scarcity without defining what it is.

We will add a definition here.

4. L36: Rainfed croplands can also get blue water via capillary rise, see https://doi.org/10.1088/1748-9326/ad78e9 . Also, correct the citation for (X. Liu et al., 2022) to fit journal guidelines (check through the manuscript for other instances).

We will include capillary rise as a water source for rainfed agriculture.

In our manuscript, we cite two authors named Liu who published in the same year [2022], which is why we distinguish them in the citation using initials.

5. L47: Maybe use EFR instead of “environmental water requirements” since you already introduced it a few lines earlier.

Will do.

6. L62: Define what the LPJmL abbreviation is.

Will do.

7. L98-99: It is not clear why running the 1901-2019 period with a 3500-year spin-up is needed. The analysis is mainly for 2015-2019. So why starting in 1901, and why so many spin-up years? Please elaborate.

The 3,500-year spin-up period has been applied for the natural vegetation in order to bring the plant functional type (PFT) distributions as well as the carbon and nitrogen pools into a dynamic pre-industrial equilibrium. This spin-up is generally needed so that pool sizes and simulated dynamics are not affected by model drift or inconsistencies between an initialization of pools and internally computed rates. The subsequent land-use spin-up (1500-2014), allows the model to incorporate historical land-use changes. From 1901 onwards, we have a transient climate input (before it was shuffled climate input from 1901-1930), which is needed for including the effect of historical land use and climate on agricultural soil properties, amongst others. After this, the analysis period (2015-2019) starts. We will explain this in more detail in the Supplementary Material.

8. L112: Please explain what 0.01 rate is (units? physical meaning?) and how it was selected.

We adopted the baseflow rate from Döll et al. (2003), it has the unit 1/d, which means that 1% of the current buffer volume is released as baseflow each day.

9. L120: Based on what 8mm/d as max is defined?

In our study, we used the revised maximum daily transpiration rate (E_{max} = 8 mm day^{-1}) from Fader et al. (2010). We will include this reference in the revised manuscript.

10. L126-127: Please provide references to the coefficient and scaling factor (or explain how you calculated them)

Thank you for this comment, we will add references for both variables: Huntingford and Monteith (1998) for the scaling factor (from which we took the average value) and Priestley and Taylor (1972) for the coefficient.

11. Please add units to Eq. 2 and 3.

Will do.

12. How do you calculate annual representative GWS values based on monthly numbers? I find it a bit unclear. Perhaps add an equation to better explain L137-139.

To derive annual GWS values from monthly outputs, we calculated the mean across all months. We acknowledge that this approach smooths out seasonal variations, which is why we present seasonal maps in Figure 2.

13. L148: WUdom, WUind, and WUirr are not defined.

We will define the water use input data accordingly in the revised manuscript.

14. L187-188: consider adding a sentence with additional global GWS estimates for each year or simply provide a range (min, max) during 2015-2019.

Will do.

15. In Table 1, the total area for ILIM under GWS>0.2 is larger for rainfed crops (426+213 Mha) than for all crops combined (431+161 Mha). Please double-check.

Thank you for raising this point. This happened due to an inconsistency in the calculation as averaged values of rainfed and irrigated cropland were summed up. We will calculate the areas differentiated into rainfed and irrigated in the revised manuscript.

16. L197-198: If 37% of all croplands are under GWS>0.2, then logically the remaining lands (63%) are under GWS<0.2. However, the authors say it is only 13%. Please double-check.

50% of the cropland experience GWS > 0.2 with the INO scenario (no irrigation), 37% with the ILIM scenario (limited irrigation). The 13% therefore state that "13% of the global cropland area experiences GWS <0.2 due to the alleviating effect of irrigation" (line 198).

17. L202-204: From the text, it is not immediately clear what the difference is with L195-197. After a few minutes, I finally realised that it is only about irrigated grid cells here (rainfed excluded). Please make it clear for the reader.

Thank you for this comment, we will rephrase the sentence so that it is directly clear that these values refer to grid cells with irrigation.

18. L204: If indeed only irrigated cells are analysed, please explain why there are still large GWS areas remaining (once you switch on irrigation). Is it because some irrigated cells cannot have access to a sufficient blue water supply, so the crops remain under water deficit? If so, how realistic is this assumption? I would expect that such underirrigation is not that common globally, since farmers can always dig into groundwater reserves or simply ignore EFR (perhaps mention this in the sub-section on limitations and uncertainties).

Thank you for this remark. Figure 4b presents results for all agricultural areas within grid cells that contain irrigation, meaning both rainfed and irrigated cropland. Due to that fact, it is possible that even grid cells that have irrigation contain higher levels of GWS. We chose this approach to ensure comparability with the BWS index, which is not CFT-specific and refers to the entire grid-cell area. We will clarify this more explicitly in the revised Results section.

19. L296: Consider providing a few examples of green water management options.

Thank you for this suggestion, we will include examples of green water management options in the revised manuscript.

As you can see, I took a great responsibility of being the "*annoying reviewer #2*". Hope my constructive criticism will be helpful in improving the quality of this paper, which I am truly looking forward to reviewing in the next round of revisions. Good luck!

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