EGUSPHERE-2024-2695

Responses to Community Comments

This document provides the responses (in blue) to community comments.

Juan P. Boisier, on behalf of all co-authors.

Comments by Kyra Boek

I. SUMMARY, CONTRIBUTION AND RECOMMENDATION
The manuscript by Boisier et al. assesses water stress in Chile from the mid-20th century to the end of the 21st century under various scenarios, specifically focusing on the megadrought from 2010 to 2022. The authors developed spatially distributed national-scale datasets on hydroclimatic data, water availability, land use, and water use. They used these to calculate the Water Stress Index (WSI) for all major basins to quantify the intensity of water stress during past, present, and future time periods. Findings indicate high to extreme water stress in semi-arid regions during the megadrought, with future projections indicating worsening conditions due to increased water consumption and reduced availability. The study suggests using the WSI for effective water security assessment and adaptation planning.

The novelty of this research lies in its integration of newly created high-resolution datasets with long-term projections, providing an overview of water stress in Chile. This is interesting, as not many papers focus on water stress in a whole country (but rather in a regional or basin-specific context); especially not a country that is a large as Chile. If successful, this approach can inform national-scale policymaking and improve water management strategies across varying climate zones.

The manuscript is well-written, with clear and concise language that effectively communicates complex concepts. The structure is logical, making it easy to follow the progression of the research and its findings. The figures are displayed clearly, enhancing the understanding of the data and results.

In principle, the study fits the scope of HESS, as it uses an interdisciplinary approach to look at spatial and temporal characteristics of water resources in Chile and gives concrete examples of socio-economic impacts of water stress. The use of novel datasets also contributes to the advancement of hydrological modelling. However, the use of these novel datasets in the context of the WSI – a simple index which is typically used on a continental or global scale – may reduce the robustness of the study's outcomes, especially given the lack of model validation and sensitivity analysis. Therefore, while this study provides a comprehensive initial indication of water stress in Chile, I believe there are several potential shortcomings that might need to be addressed before final publication in HESS. In addition, I will discuss several smaller issues and suggestions that I encountered during my study of the manuscript and that I believe could benefit the authors in improving their work.

R: We appreciate the recognition of our paper's strengths and the constructive feedback on its limitations, which we respond below.

II. GENERAL COMMENTS

1) Firstly, I question the applicability of the WSI at a national scale in this study. While commonly used for continental or global assessments (e.g. Gosling & Arnell, 2013; Pfister & Bayer, 2013), its use on regional or national scales is limited. When used on smaller scales, the WSI typically focuses on

specific contexts, such as food security (Gheewala et al., 2014) or cross-region comparisons (Milano et al., 2013). However, this study goes further by suggesting the WSI can inform policy, but as Milano et al. (2013) note, sub-regional studies with local stakeholder input and detailed catchment data are essential for effective water management. The WSI can compare future water stress across regions but provides only a rough indication of stress, missing key regional differences in water use, governance, and access, which are key for effective water management. Although the authors suggest that water security goals can be achieved through metrics like the WSI (p1, line 27; p22, line 498), they do not specify which complementary metrics are necessary for a complete assessment. To strengthen the study, the authors could discuss the limitations of the WSI and recommend using more detailed hydrological models (e.g., SWAT, VIC) and additional metrics to better address regional variability.

R1: We agree with the reviewer on the limitations of using WSI to establish water security goals. In our responses to the Referees 1 and 2, we address these limitations and propose modifications in the revised manuscript. We invite the reviewer to read the response document, particularly R4 to RC1 and R2 to RC2.

2) The study also lacks model validation and sensitivity analysis for the WSI and key variables. Validating the WSI output against (independent) previous estimates – for example by using the Nash-Sutcliffe Efficiency or Kling-Gupta Efficiency – would improve the scientific contribution. Validating the output of the current model to those obtained in previous studies that followed similar methodologies would provide insights into the consistency and reliability of the model under different conditions. For instance, the assessment by Ferreira et al. (2023) on precipitation and hydrological droughts in South America and work by Alvarez-Garreton et al. (2022) in which the WSI was calculated across 277 basins in Chile could serve as valuable references. Additionally, the study by Perveen and James (2011) on the scale invariance of water stress and scarcity indicators provides a critical perspective on the robustness of these indicators across different spatial scales, highlighting the need for such validation.

R2: Rather than a model, the WSI is a metric derived from water availability and use data—datasets that are themselves estimated with models. We agree that, beyond what is presented in Appendix B, a more detailed validation of the datasets developed in our study would help better assess uncertainty.

Directly comparing WSI values from our study with those reported in previous studies is not feasible due to significant methodological differences. Alvarez-Garreton et al. (2022) estimated WSI based on allocated water rights rather than actual water consumption. This approach can significantly misrepresent actual water use, and addressing that limitation is one of the key contributions of our work. Beyond global assessments, we are not aware of other peer-reviewed publications that assess the WSI across Chile.

Regarding a more detailed comparison of datasets, we refer the reviewer to our responses to Referee 1—specifically R6, R7, R8, and R9 for water availability, and R6 for water use benchmarks. Regarding the latter, in the revised manuscript we will include a new table summarizing national water use totals by major sector and comparing them with estimates from other sources (see R6 to RC1 for further details).

3) Furthermore, the results of the WSI are not supported by a sensitivity analysis, leaving it unclear how variations in water availability, land use, or water demand affect the final WSI values. Adding a sensitivity analysis would give a clearer understanding of the model's reliability under different conditions. An example of how to implement such a sensitivity analysis can be found in the paper by Milano et al. (2013), where a regional sensitivity analysis is performed. The authors could add this analysis either in the methodology section or as part of the results, explaining the specific assumptions tested and the range of variability explored.

R3: We agree that a sensitivity analysis of the WSI would be useful. However, this type of analysis falls outside the scope of the present manuscript, which already includes a substantial amount of new data and analysis. In this study, we focused our efforts on developing new datasets on water availability and water use in Chile, with a spatio-temporal resolution that did not previously exist. We evaluated these datasets through mass balance assessments at the basin scale (see response R9 to RC1) and by comparing our water use estimates with other available sources (see response R6 to RC1). A full uncertainty analysis of the WSI would require error estimation for the underlying datasets, which is beyond the scope of this study. However, we acknowledge its importance and consider it a valuable direction for future work.

4) A second general comment concerns the use of the Hargreaves-Samani (HS) formula to estimate potential evapotranspiration (PET). Since this formula is primarily based on temperature, this raises concerns about its suitability for projecting future PET under changing climate conditions. Previous studies (e.g. Alexandris et al., 2008; Temesgen et al., 2005; De Souza Nóia Júnior et al., 2019) show that the HS method tends to systematically overestimate PET by up to 20%, particularly in regions with high average temperature, wind speed, or relative humidity. This overestimation is a critical issue when using the HS formula for long-term projections, as the projected increase in temperature may lead to an overestimation of future drought conditions. While the authors adjust for wind biases and align HS with the Penman-Monteith (PM) formula, uncertainties remain, particularly for long-term trend projections. I suggest adopting the PM method or conducting an uncertainty analysis to assess the reliability of HS estimates. Although Appendix B validates the ET simulations, it doesn't fully address how the corrected HS method performs over time, especially under future climate conditions. A validation process using the same datasets and temporal scales as the analysis would enhance confidence in the projections.

R4: We largely agree that a more physically constrained formulation of PET is desirable. The approach to PET estimation was extensively discussed within our team during the methodological design stage. The method we ultimately adopted aimed to meet several objectives—most importantly, to provide a consistent way to estimate PET for both the historical period and future projections. The reference atmospheric data is based on CR2MET, a dataset designed to accurately capture precipitation and temperature over continental Chile. However, it does not include other variables required to compute Penman-Monteith PET (wind speed and humidity). Similarly, for climate model projections, these variables were not considered due to limitations in CR2MET (which served as the reference dataset for downscaling) and, in part, to enable the evaluation of multiple climate models for uncertainty assessment, rather than restricting the analysis to those providing a broader set of variables (we note that the processed data for this study sum various TBs in volume).

Given these constraints, we chose to estimate PET using temperature data via the Hargreaves-Samani method, and to partially improve this estimate through a climatological adjustment using Penman-Monteith, as described in Section 2.1. We acknowledge that this approach does not capture certain effects, such as changes in wind speed over time, which can influence PET. However, for the purposes of this paper, we estimate that such effects are of second-order importance. Although ET in some regions of Chile is energy-limited, in general, ET variability is largely controlled by soil moisture within the region of study, which is primarily driven by precipitation. Additionally, changes in ET have less influence on water availability than changes in precipitation. Properly assessing these mechanisms within a contrasting territory like Chile is of great interest but would require a regional-scale coupled model simulations, rather than statistically downscaled ESM data, to account for land–atmosphere feedbacks. This, however, is far beyond the scope of this paper.

A revised text will state more clearly the limitations on the method adopted for estimating PET.

5) The authors should also clarify whether the novel land and water use datasets were cross validated with independent datasets to enhance the credibility of the findings. Additionally, including uncertainty ranges for key variables, such as precipitation and water demand, would further

strengthen the results by providing a clearer understanding of potential limitations and the confidence in the model's outcomes.

R5: We agree. Please see our response on this matter above (R2).

6) Finally, the study does not clarify whether water availability is estimated based on hydrological years or calendar years. This distinction is important since omitting storage changes between years can lead to inaccuracies in estimating water availability, particularly in regions where hydrological processes are influenced by large seasonality. Other studies on droughts (e.g. Xiao et al., 2017; Yuan et al., 2024) use hydrological years (or water years) to ensure that as much as possible of the surface runoff during that (hydrological) year is attributable to the precipitation during the same hydrological year. I would advise to use hydrological years for the hydrological balance (Section 2.3, p6-7), or to explain the use of calendar years. Given Chile's diverse climate, the authors should consider whether a single hydrological year is appropriate nationwide or if region-specific years should be applied. At a minimum, a brief discussion on this issue, with suggestions for future research, would improve the study's clarity.

R6: CR2MET as well as the mass balance model used to estimate evapotranspiration operates on a daily scale. Annual computations of water availability, or for water use data, are based on calendar years. Please note that the choice between hydrological and calendar years is not particularly relevant in this study, as the results are presented in terms of climatological (multi annual) averages.

III. SPECIFIC COMMENTS

Firstly, I randomly fact-checked some of the statements made by the authors. The results of this check can be found in the following two specific comments.

i. WSI threshold value

The use of the value of 0.4 (or 40%, line 41-42, p2) to indicate water-stressed regions seems arbitrary, as some studies use a value of 0.2-0.4 to indicate moderate water stress (e.g. Milano et al., 2013; Gosling & Arnell, 2013) or even 0.1-0.5 (Gheewala et al., 2014). Oki and Kanae (2006) describe 0.4 as "a reasonable, although not definitive, threshold value." In the current paper, the threshold of 0.4 seems to be definitive, but the authors should clarify that this is not the case. While in line 499-500 (p22) the authors say that "regarding the WSI, stress levels are well-defined worldwide in relation to their impacts on watersheds," the five references that are listed to support this statement do not carry the same information:

• Falkenmark, 2013a: this paper talks about water stress in relation to climate change on national and regional scale, but it does not mention WSI levels nor their impact on the watershed-scale.

• Falkenmark, 2013b: this paper also talks about consequences of water stress, but again it does not mention the WSI (or any other index).

• Rockstrom et al., 2014: although the WSI is mentioned (as Falkenmark Index), the paper does not mention stress levels in relation to their impact on watersheds. General problems regarding water scarcity are listed, but it seems like the WSI is used to give a general idea of water stress worldwide rather than a national-scale/watershed-scale indicator.

• Oki and Kanae, 2006: this paper uses the WSI on a global scale.

• Grafton et al., 2012: this paper is not listed in the reference section at all.

A simple solution to this problem is to include a discussion on the value of 0.4 as a threshold, and to remove the statement that WSI stress levels are well-defined worldwide in relation to their impacts on watersheds. Moreover, if the authors still wish to use the paper by Grafton et al., 2012, they should include this paper in the reference section.

We generally agree with the reviewer. Regarding the 40% WSI thresholds, please refer to our responses to related referee comments, particularly R2 to RC2. We will remove Grafton et al., 2012 from the text, as it was a mistake. Thank you for pointing that out. The other references are being adapted in our proposed manuscript revision. In particular, we will replace Falkenmark´s 2013 papers with Falkenmark et al. (2007) and remove Rockström et al. (2014) in this context (see the list of new references in our response to the referee comments).

ii. Total water use in Chile at present

In line 348-349 (p14), the authors state: "Considering both consumptive and non-consumptive uses, the total water use in Chile is estimated to be around 100 km3 per year at present. This value is similar to, albeit slightly higher than, other independent estimates." I checked the three references that were used for this statement.

• DGA, 2017: this document is written in Spanish but if I am not mistaken, I would say that based on "Cuadro 4.17-1" and "Cuadro 4.17-2", p47 & 48, the total water use is over 160 km3/year (sum of "Demanda Consuntiva 2015" and "Demanda No Consuntiva 2015" (consumptive and non-consumptive demand)). These numbers are based on a calculation of the water demand per district.

• Fundacion Chile: Based on table 5, the "registered" water consumption is 3.335,44 m3/s which corresponds roughly to 105 km3/y. This is the water consumption based on the Water Use Rights as mentioned in the current paper, so the actual water consumption is likely much higher than that.

• FAO and UN Water, 2021: This source only shows water stress per continent so I am not sure where they found a specific number for Chile.

If my conclusions based on these documents are correct, the authors can omit the problem by stating that the value is lower than other independent estimates.

This was addressed in our responses to the referee comments. In particular, we are adding a new table that includes a proper comparison with those datasets. Please refer to our response R6 to RC1 for further details.

iii. Research questions

The research questions could be more specific. Instead of "What have been the historical water stress levels of the basins in Chile?" (p3, line 78), it could specify: "What have been the historical water stress levels of the major basins in Chile according to the Water Stress Index (WSI)?" In the second research question (p4, line 78-79), it is asked what the drivers of changes in water stress levels are. While the authors look at climatic and anthropogenic drivers, they do not specifically look at factors such as population growth and technological advances. Therefore, I suggest to specify the drivers that this study looks at. E.g., "What have been the climatic and anthropogenic drivers of changes in these levels?" or: "What have been the drivers of changes in water stress levels in Chile, and how have factors such as climate change, land use, and water use contributed to these changes?"

We agree and change the formulation of these questions:

> *(...). Understanding the causes of water stress is critical for designing strategies aimed at mitigating unsustainable water uses and adapting to future climate conditions.* ***Therefore,*** *this study aims to fill this gap by addressing the following research questions:* ***What have the historical water stress levels of the basins in Chile been, according to the water use-to-availability ratio? How have trends in climate and water use influenced changes in these levels?*** *What can be expected under future climate scenarios?*

iv. Final remarks

The term "significant(ly)" occurs 21 times. This word suggests that these claims have statistical validation, but this is usually not the case. Consider replacing this word with terms like "substantial(ly)" or "notable(ly)" where appropriate (e.g., p14, line 353; p17, line 392; p18, line 436; p20, line 460)

We agree with the suggestion and will carefully review the text. We will adjust or replace instances where “significant(ly)” may have been misleading, opting for the suggested terms such as "substantial(ly)" or "notable(ly)" where appropriate.

p1, line 1: the use of the word “evidenced” in the title conveys the idea that the increase in water stress is not merely observed but backed by strong, concrete data. In my opinion, this level of certainty is insufficient supported by the research methodology. A simple solution to this problem is to replace “evidenced by” by a term like “indicated by” or “revealed through”. This way, the title still reflects that the study sheds light on the issue but does not claim to provide absolute evidence.

We agree and will take the reviewer's suggestion for the title: *Increasing Water Stress in Chile Revealed by Novel Datasets of Water Availability, Land Use, and Water Use.*

p1, line 13: Clarify why the term "megadrought" is used, as it is not explained in the introduction.

We will mention the megadrought in the revised introduction:

> *(...). This situation is partly due to a long-term drying trend and decreased water availability in the region (Boisier et al., 2018), which has been exacerbated by a decade-long* ***meteorological*** *drought since 2010,* ***the so-called megadrought*** *(Boisier et al., 2016; Garreaud et al., 2017, 2019). (...)*

p1, line 15: the authors mention that water-intensive agriculture raises questions about the contributions of water extraction to high stress levels. However, they do not come back to this question. Therefore, I think it would make more sense to ask about the contribution of water consumption, as this is what the paper is largely about. Alternatively, the authors could mention the answer to this original question in the conclusion section.

We do not fully understand this comment. In what sense do we not come back to this question? The influence of water use (or consumption) on water stress levels is central to the study, including the particular role of irrigated agriculture.

p1, line 15: “the contributions (...) on high water stress levels”; should be “to”.

Corrected.

p1, line 26: using the WSI to assess “one” of the several aspects of water security seems like a vague description; what aspect of water security is assessed with the WSI?

This point is discussed in detail in various sections of the main text. Pease see our response letter to referees, particularly R2 to RC2. In the abstract, we opted to keep it concise.

p2, line 32: while this definition is important, the opening of the introduction could be stronger by starting with a statement related to water security to catch the reader’s attention.

Thank you for the suggestion, but we prefer to use the definition provided in the literature to introduce the topic.

p2, line 41-42: the authors could consider providing a table with an overview of WSI thresholds (no/ high/extreme water stress).

In the revised manuscript we will provide a clearer explanation of these thresholds (please refer to our response R2 to RC2). We prefer not to include additional tables in the main text, as these levels are described in several published papers and documents.

p3, line 65: "decade-long drought", what type of drought is referred to? E.g. hydrological/ meteorological/socio-economic/…

It is hydrometeorological, but mostly described from a meteorological perspective. We will add meteorological in this sentence.

p3, line 67: what is meant by Water Use Rights?

We will explain water use rights in the revised manuscript:

> *(…). Water scarcity issues have also been attributed to limitations in the water management system* ***defined in the Chilean Water Code (Congreso Nacional de Chile, 2022). This system is based on Water Use Rights (WURs), the legal entitlements that define the amount and timing of water access for consumptive uses (e.g., drinking water, irrigation) and non-consumptive uses (e.g., hydroelectricity). A major limitation of this allocation scheme is that it does not account for climate variability and decreasing water availability (Alvarez-Garreton et al., 2023a; Barría et al., 2021a). (…)***

p3, line 72: Groundwater overexploitation is mentioned as an important contributor to water stress, but it is not fully addressed in the paper. Consider including it in the discussion or conclusion.

We have addressed this topic in our response to the referee 1 (please see R3 and R4 to RC1).

p4, Table 1, line 96: "irrigation fraction (IV) fraction"; the word "fraction" appears twice.

Corrected.

p6, line 162: "To ensure accurate seasonal representation, (...)"; does this also include important weather phenomena like El Niño?

This sentence states that the method is applied by accounting for the probability distribution of variables separately for each month. This ensures that the mean, percentiles, extremes, etc., are correctly represented according to the seasonal cycle in these parameters. The spread in variables' distribution reflects the actual variability, so the manifestation of ENSO in Chile is inherently accounted for—but mostly at the interannual scale, rather than the seasonal cycle.

p6, line 164: TN should be TN (in subscript).

Corrected.

p7, line 183: "account" should be "accounts".

Corrected.

p7, line 193: "Long-term land-use dynamics is (...)"; should be "are".

Corrected.

p7, line 193-194: "many other factors"; could the authors give an example?

We do not find the phrase "many other factors" in these lines, but rather the sentence: "Long-term land-use dynamics is a key factor to consider when estimating changes in water use over time and for many other applications." An accurate dataset of historical land use allows for studying a wide range of topics related to land-use changes, including their interactions with climate, socioeconomic drivers, historical processes, etc.

p9, line 245: "is" should be "are".

Corrected.

p9, line 247: Subscript "IR" unclear (it looks like eIR now).

It seems correct in our records.

p9, line 250: UNC,LU should be UNC,LU (in subscript).

Corrected.

p10, line 286: "regions in the globe" should be "regions of the globe"

Corrected.

p12, Figure 2: the color used to indicate a ΔP of +25 and -25 mm/yr are too similar. This problem can be solved by making the values above 0 greener (rather than yellow).

Yes, the color palette is predefined, but we will try to modify it to improve clarity.

p12, line 311: the authors refer to Fig. 2 but this should be Fig. 3.

We actually refer to the observed changes depicted in Figure 2. We will change that sentence to make it more clearly related to the figure:

> *Besides geographical differences, Chile's hydroclimate exhibits significant temporal variability.* ***Over*** *the long term, local precipitation records show a clear downward trend****, as shown by the changes in mean annual precipitation between the periods 1960-1990 and 1990-2020*** *across much of the country (Fig. 2).*

p13, line 311-313: "The spatially distributed CR2MET dataset shows a precipitation decline consistent with observations, a match that matters given the subsequent use of this dataset for basin-scale assessments." Is this statement based on a statistical test or purely on visual analysis? The authors say that it matters that the observations match the CR2MET data, but this statement would be stronger if they could show that these datasets show significant correlation.

We argue that, in addition to the day-to-day or year-to-year covariance with observations—validated in CR2MET-related papers and documents—the figure shows consistent behavior regarding long-term trends. In our opinion, this does not require further statistical testing. Please see our response R9 to RC1, where the evaluation of the CR2MET data is discussed.

p13, line 319-328: Could the Andes Cordillera also explain discrepancies between this study and others, given the hydroclimatic effects noted earlier (p2, lines 49-50)?

Yes, some local effects related to the Andes barrier on the westerly flow (not fully captured by ESMs), in addition to other conditions that are systematically biased in the models (e.g., sea surface temperature in southeaster Pacific), may be affecting climate projections for Chile. However, these effects have not yet been clearly constrained by observations.

p15, line 372 and 373: "litters" should be "litres" or "liters".

Corrected.

p23, line 524: "water-demand" should be "water demand" to match other occurrences of this term in the document.

Corrected.

p24, Table 2: in the first scenario description, "(2020-2020)" should be "(2000-2020)"

Corrected.

p25, line 568: "near 30% reduction" should be "nearly a 30% reduction".

Corrected.

p25, line 586-591: the conclusion could be slightly improved by concluding with a strong final statement, e.g. something along the lines of "Achieving long-term water security in Chile will require not only better governance and infrastructure but also a commitment to sustainable water use practices across all sectors. This study provides critical insights into how such goals can be realized."

Thank you for the suggestion. We will consider adding something along these lines in the revised manuscript. Please see the related comment on this section and the proposed changes in our responses to Referee 1.