Dear Reviewer,

Thank you for your comments and suggestions. Our response is indicated below your comments in blue font.

General comments

This is valuable research, where changes in the climatic water budget (CWB) in the Alps, and future water scarcity risks are analyzed at an unprecedented spatial detail (1 km with a very good representation of mountain orographic factors).

It is also a work of great topical relevance and very timely, given the current 2022 heat wave in Europe and the water scarcity in the Alps.

This work explains well current and future alpine drought development mechanisms and shows how such events will become more likely in the future, despite possible year-round greater water availability in the Alps. Positive CWB signals are mainly caused by increasing rainfall, particularly during winter. On the contrary, the negative CWB changes during summer are caused by both increasing AED and slightly decreasing rainfall. Because of less snowpack contribution, water provision in the summer will become less constant and more connected to the liquid precipitation variability, and therefore, given the higher temperatures, summer water scarcity is more likely.

The figures are excellent ad full of information. Results are very interesting, especially for the spatial detail, which allows us to identify the local impacts of Alpine orography on the water budget. One relevant result is the finding that in the far future period the CC signal is much stronger in mountains than in lowlands.

I am looking for further research, showing how the 2022 summer drought will be compared to 2003 in terms of return period!

However, several methodological details need further clarification, especially on evaporation and glacier runoff estimation, and should be addressed before publication, as indicated below and in the specific comments.

- I am surprised by the strong increase of precipitation of the RCP 4.5 scenario for 2070 – 2100. Since most of the results are depending on this, and we know that climate models are still more uncertain in predicting precipitation trends than temperature, a discussion on the reliability of this high-precipitation RCP scenario would be very helpful.

  Yes, you are right, and thank you for your important comment! Precipitation projections are indeed highly uncertain, particularly in the Alpine region due to the complex generation of precipitation and the high fraction of convective precipitation. However, it is a feature seen also in other publications, e.g., https://doi.org/10.1002/2017JD027176. Here, an increase in precipitation in winter (+17 mm/d), spring and autumn (+4 mm/d) is contrasted by a decrease in summer (-4 mm/d), for RCP4.5 in the far future (2070-2099), which in total points towards more precipitation on an annual basis. This is in line with our study.

We will definitely discuss this feature in more detail and add the relevant literature (see above).

- I am also a little surprised you do not find an important increase in Evaporative Demand. I am just wondering if this could be related to the quite simplified approach used to estimate evapotranspiration ET. This point deserves further discussion.

  Yes, we will further discuss this crucial aspect of the paper in more detail. See particular suggestions below.

- **Title and journal.** The title triggers curiosity, but then reading the paper I found much more than this. The best parts of the paper are the high-resolution scenarios of climatic water balance (CWB) for Austria. So, the title is a little bit limiting. Changes in drought risk are only one of the aspects. I am also wondering if this paper fits better with HESS instead of NHESS.

  Thank you for your kind comment on the paper. You are right, there is more in it than just the drought aspect. However, we think that all the other topics rotate around the drought topic, which is from our perspective the most important point of the paper. This is the reason we choose to publish in NHESS, whereas HESS would also make perfectly sense. We do not know if a change in journal would be easily to accomplish and/or require an entirely new submission. In the interest of time, we thus for now prefer to stick to NHESS.

Specific comments

Abstract. Please specify that you consider only meteorological drought.

Yes, good point. We will specify that we are explicitly considering meteorological drought.

1. **Introduction (and title).**

L68 "How will future surface water availability change" I am not sure if the term surface water availability is correct. You are working with RCMS and a very simplified hydrological representation of the processes. What you get is mostly the water budget. Then you work on the CWB, which is simply a good indicator of the real surface water availability. Why do not also refer to CWB in the title?

Ok, thank you for your comment. It is true, that we do not fully represent the surface water availability, so referring to the CWB in the title would be more appropriate.

To have an estimation of surface water you need to model also water infiltration, vadose zone hydrology, and groundwater recharge losses… which is of course beyond the purpose of the paper.

2 Data:

L80 "The broadly used RCP8.5 scenario is intentionally not included here, since its emission pathway is highly unlikely from today´s emissions trajectories," Sure? Are you so optimistic about the future?

The literature on criticism considering overemphasising the RCP8.5 scenario is constantly growing. To make the results not too overloaded we decided to display RCP4.5 and RCP2.6 as one pathway which is optimistic and plausible and another one that is more optimistic. We will therefore better argue on limiting to those two, also highlighting the latest literature on RCP scenarios. Furthermore, we will add to the discussion an assessment on potential RCP8.5 implications to broaden the spectrum of the paper.

L87 "EURO-CORDEX downscaling," Could you provide more information on the approach? How is orography taken into account for the downscaling?

Thanks for your comment, we will discuss the downscaling procedure in more detail. Orography is indirectly taken into account, since the reference data (SPARTACUS) does explicitly consider orographic effects (cold air pools, inversions etc.) see these two publications for details:

https://doi.org/10.1007/s00704-017-2093-x

https://doi.org/10.1007/s00704-015-1411-4

3 Methods:

L119 "snowmelt model," Ok it is essential to consider snow melt in the CWB, but then you need a good snow melt model. Please provide here or later more information on how snow melt is modeled.

Yes, good point, we will add this rather relevant information in the methods section. We will add additional information of the extended degree-day approach described in Olefs et al. (2020).

"**3.2 Atmospheric Evaporative Demand"** AED is a key parameter. For this is essential the calculation of ETP. Which is the accuracy of the calculated ETP? How has been validated?

Yes, AED calculated by the given formulation of a re-calibrated Hargreaves equation and is evaluated in the referred publication:

https://doi.org/10.5194/hess-20-1211-2016

In comparison with station data Penman-Monteith (nearest neighbour gridpoint) and a Penman-Monteith estimate using data from the Austrian nowcasting system INCA the given method (ARET) is performing rather well. See this table from the publication indicated above:

Table 2. Error characteristics of ARET and INCA against station data.

	Bias [mm day^{-1}]		RMSE [mm day^{-1}]		RE [%]	
	ARET	INCA	ARET	INCA	ARET	INCA
January	−0.01	−0.05	0.29	0.34	1	−7
February	−0.17	−0.30	0.60	0.65	−12	−25
March	0.04	−0.23	0.84	0.89	4	−14
April	0.80	0.66	1.34	1.59	35	28
May	0.79	0.51	1.38	1.58	29	19
June	0.19	−0.24	1.42	1.80	6	−8
July	0.39	0.31	1.29	1.58	12	9
August	−0.09	−0.01	1.16	1.42	−1	1
September	−0.14	−0.10	0.96	1.11	−6	−4
October	−0.15	−0.06	0.57	0.69	−8	−3
November	−0.03	0.01	0.43	0.54	2	5
December	−0.16	−0.18	0.39	0.43	−15	−18
Year	0.12	0.03	0.89	1.05	4	−1

Another key question on which I have some concern is how AED based on potential ET makes sense in an environment where real ET could be very different from FAO conditions, having very different land covers such as forests, rocks, and so on.

This is indeed a very good question. However, we think it is far beyond the scope of the paper. It would be quite interesting to see these questions tackled in a separate paper.

L158 “Herein, SG-CL is driven with gridded observations and the OEKS15 dataset for the reference and future projection runs, respectively.” The statement is not clear to me.

We will rephrase this sentence to:

“Herein, SG-CL is driven by gridded observations and the historical simulations of OEKS15 for the reference period and with scenario simulations of OEKS15 considering near and far future time periods.”

“**glacier runoff**” This is also a key component of the CWB. The approach used is also not well explained. How a change in glacier area was estimated? How all the area above 2500 m is considered? Why this 2500 threshold?

Thanks for this important point.

We expand on the points raised referring to key literature

The model GLOGEM computes glacier mass balance and associated geometry changes for each glacier individually as described comprehensive in (Huss and Hock 2015) and (Huss and Hock 2018). The climatic mass balance is calculated at a monthly resolution based on near-surface air temperature and precipitation time series. Total mass changes are used to adjust each glacier’s surface elevation and extent on a yearly basis using an empirical parameterization (Huss et al. 2010). 2500 m a.s.l. is used as a threshold for areas potentially impacted by glaciers as this is approximately the elevation above which glaciers can occur in the study area (e.g., Fischer et al. 2015).

Fischer A, Seiser B, Stocker Waldhuber M, et al (2015) Tracing glacier changes in Austria from the Little Ice Age to the present using a lidar-based high-resolution glacier inventory in Austria. Cryosph 9:753–766. doi: 10.5194/tc-9-753-2015

Huss M, Hock R (2015) A new model for global glacier change and sea-level rise. Front Earth Sci 3:1–22. doi: 10.3389/feart.2015.00054

Huss M, Hock R (2018) Global-scale hydrological response to future glacier mass loss. Nat Clim Chang 8:. doi: 10.1038/s41558-017-0049-x

Huss M, Jouvet G, Farinotti D, Bauder A (2010) Future high-mountain hydrology: A new parameterization of glacier retreat. Hydrol Earth Syst Sci 14:815–829. doi: 10.5194/hess-14-815-2010

"**3.5.2 Frequency Analysis - Return Periods**" Also here some parts are not clear.

How a return period can be calculated with not-stationary data?

Thank you for your important comment!

Of course, non-stationarity is an issue with respect to estimating return periods of a given variable. However, we tested the CWB for trends in the observational data, which revealed no significant trends in each season and class of altitude. This is a rather robust signal, since inter-annual variability of the CWB is known to be rather large. Given the short time periods of 30 years, insignificance of trends is rather plausible. This enables to assume that the data is stationary given no significant linear trends and justifies the application of the return period analysis.

We did the same for the climate scenario data, where we tested each of 30-year periods under consideration (3 periods, 2 classes of altitude, 4 seasons and 24 climate model runs), where we found only 12% of the total number of 30-year periods showed significant trends in the CWB. Given the large uncertainties already present in the climate scenarios, we argue that this minor fraction of non-stationary time periods would not alter the general conclusions of the return period estimates.

However, we will discuss this topic both in the methods and in the discussion section, because it is a rather relevant aspect of the applied methods.

10-y tr is not a too-low return period for an extreme drought? For floods, usually, much larger Tr are considered.

Yes, that´s true. However, we thought of also displaying a more moderate drought event threshold. Since there is a more in-depth analysis of the 2003 event we think it would be worthwhile to keep this information on moderate drought changes.

4 Results:

Results are very interesting, especially for the spatial detail, which allows us to identify the local impacts of Alpine orography on the water budget. I am looking at what the 2022 summer drought will look like in terms of return periods in comparison with the 2003 one!

Yes, this would be really interesting, perhaps a topic for another paper with a wider historical context (HISTALP Data).

Fig 3 I am surprised by the strong increase of precipitation of the RCP 4.5 scenario for 2070 – 2100. Since most of the results are depending on this, and we know that climate models are quite uncertain in predicting precipitation trends concerning temperature, a discussion on the reliability of this high-precipitation RCP scenario would be very helpful.

Yes, we will discuss this feature in more detail as also indicated at the beginning.

Fig 4d and L357. I am surprised that the annual average of mountain regions is only 34 mm/year. Given the total precipitation that should exceed 1000 mm/year and ET that should not exceed 500 mm/year, 34 mm seems a very little number since mountains act as “water towers.

The altitudinal range of the mountainous areas are from 700-2500 m a.s.l., which rather wide. So, there are high proportions of areas near this lower bound, exhibiting higher values of AED. When averaging over the whole area, this might disturb the signal and perception that the mountainous areas should indicate higher values of the CWB.

Fig 4e. It is very interesting the increase of uncertainty in spring with the farthermost scenario. It is due to the major role of liquid precipitation? Please comment.

Yes, we will have a close look into this feature. Most probably, it is the uncertainty in the liquid precipitation and the antecedent snow pack conditions and subsequent snow melt.

Fig 4f and L 292. What do you mean by rainfall? Only liquid precipitation? It is not clear!

Thank you, we will specify in more detail. We will add the definition of terms in the methods section. Here, rainfall is meant as liquid precipitation.

Fig 5. Really beautiful Figure where the effect of elevation on the P/T relevance on changes in CWB is very clear. It would be nice to see the same Figure for different P and T scenarios may be in the supplementary material. What would happen without such a strong P increase?

L371 Fig 6c not 7c

Thank you, we will correct that.

L386 What is the drought duration? How do you define a drought period?

Here, a drought event is defined as a period in time where the CWB deceeds a certain threshold in the given season. This means, there is a hard definition for the drought duration, 3 months, which arises from the seasonal considerations.

Acknowledgments: Is it possible to acknowledge in addition the ADO project?

Yes, of course, we will acknowledge the ADO project.