Answers to comments by Referee #1

Future projections of High Atlas snowpack and runoff under climate change

March 9, 2021

Comment 1 *One element that needs to be discussed in the manuscript is that the runoff coefficients are not only impacted by climatic parameters but also by surface conditions. Significant changes in vegetation cover, land use or agricultural practices often have a greater impact, and this aspect is absent from the manuscript.*

Answer: Every modelling framework relies on its own assumptions, which translate into uncertainties. In many climate change studies, the runoff coefficient is assumed to be more dependent on climatic parameters than on surface conditions (like vegetation) for long term changes. Still, you are correct, and it deserves to be added to the discussion. We do not explain differences in average RCs across catchments, nor can we say anything about long-term variability in RCs forced by non-climatic parameters, like land use change. Our projections also tacitly assume that parameters other than climate variables will remain the same. Still, land use changes may not be critical in the case of the High Atlas since most of the area under study is uncultivated, naturally lacking tree cover and sparsely populated. It has not experienced large-scale land use changes in the last few decades. However, climate-change driven trends in vegetation cover may still affect runoff efficiency in the region. It is also unclear to what extent enhanced groundwater pumping in the Oum-Er-Rbia watershed since the 1980s may have modified the natural water balance and, indirectly, RCs in mountain catchments.

Comment 2 *no reference to the literature except to the previous publication of the authors, while an important body of work exist for the Mediterranean and Morocco :*
Driouech et al https://doi.org/10.1007/s41748- 020-00169-3
Drobinski et al https://doi.org/10.1007/s10113-020-01659-w
Cramer et al https://doi.org/10.1038/s41558-018-0299-2
Lionello et al https://doi.org/10.1007/s10113-018-1290-1
In particular, climate projections on runoff already exist for the basin of interest, see Jaw et al https://doi.org/10.1016/j.ejrh.2015.02.008 Tramblay et al http://doi.org/10.1007/s11269-017-1870-8

Answer: We thank the reviewer for the references. A more thorough literature review and comparison to previous results is required. We suggest adding the following sentences to the

second paragraph of the introduction:
"Still, climate projections over Morocco – and generally the Mediterranean – agree on robust warming and drying trends under greenhouse gas forcing (Cramer et al. 2018, Lionello et al. 2018, Drobinski et al. 2020, Tuel et al. 2020c). By the end of this century, average winter temperatures in the High Atlas could be 2-4C higher, and precipitation 25-45% lower, depending on the emissions scenario (Driouech et al. 2020, Tuel et al. 2020b)."
"Future trends in runoff in the High Atlas under climate change have been investigated by Jaw et al. (2015) who analyzed simulations with the Variable Infiltration Capacity model forced by regional climate model output. They found a general tendency to reductions in streamflow, with a strong sensitivity to the forcing model's precipitation trends. Tramblay et al. (2018) took a simple water balance approach, equating long-term net precipitation with water availability, to estimate future changes in dam storage across North Africa. In the High Atlas, they projected a 40-to-50% decline in water availability under business-as-usual by the end of the 21st century."

Comment 3 *Page 2, line 38 : what is a "parametric snow module" ?*

Answer: The choice of the word "module" can be confusing indeed. What we meant here is that the authors chose to include snow in their model by using a simple temperature-based parametric representation of snow accumulation and melt. We would rephrase the sentence as *"Marchane et al. (2017) developed runoff projections for the Rheyara catchment, south of Marrakech and part of the Tensift watershed, by running conceptual monthly water-balance models incorporating simple temperature-based parametrizations of snow accumulation and melt."*

Comment 4 *Section 2.2: it is not clear why the authors consider TRMM rainfall, while daily precipitation data is available at 7 locations (line 75). Why also mention CHIRPS rainfall if it is not used in the study, as the author state*

Answer: A gridded precipitation product is required for our simulations. While it is possible to interpolate station data using a precipitation lapse-rate, the density of stations in our study area is very small, which is why we prefer to rely on TRMM. Figure 3 discusses the adequacy of this dataset, and further details can be found in Tuel et al. 2020 J Hydrology. CHIRPS data is also used in this study to discuss the robustness of the runoff coefficient model (see Table 1).

Comment 5 *Section 2.3: The authors should justify why they rely on only one RCM, when nowadays large ensemble of climate model experiments are available, such as the Euro-CORDEX (Jacob et al https://doi.org/10.1007/s10113-020-01606-9) or Med-CORDEX (Ruti et al https://doi.org/10.1175/BA D-14-00176.1) initiatives. It is well established that to obtain robust projections it is necessary to consider several combinations of GCM/RCM, and the use of only one RCM strongly reduce the relevance of the work (see Fernandez et al https://doi.org/10.1007/s00382-018-4181-8). In addition, the RegCM version 3 is rather outdated (2006) since the current version is RegCM-4 (https://www.ictp.it/research/esp/models/regcm4.aspx).*

Answer: We should have indeed commented on this choice. Our goal here is to build on the carefully-designed regional projections developed specifically for the region by Tuel et al. (2020). The choice of forcing GCM as well as MRCM parametrisation are discussed in detail in this reference. Also, we are not using RegCM3 but MRCM, a much improved version of this RCM.

MRCM is simply based on RegCM3 the same way that RegCM4 is based on RegCM3. Admittedly, we do not explore the full range of uncertainties (warming or precipitation trends, RCM configurations, etc.) and this must be mentioned in the manuscript discussion, which we propose to do as follows:
"The limitations of our approach introduce additional uncertainties. We rely on a single regional climate model, and thus do not explore the full range of uncertainties related to model configuration and parametrizations. Still, the regional simulations we use here have been specifically tailored to the area, particularly the choice of driving GCMs, and validated against a range of observations (Tuel et al. 2020). The fact that the uncertainty in snowpack projections under RCP8.5 is small could be further explored by using other GCM/RCM combinations, especially ones that lead to less warming than projected in our three-member ensemble. As to the uncertainty in precipitation trends, it is difficult to reconcile. As shown by Tuel and Eltahir (2020), the magnitude of future wet-season precipitation in Northwestern Africa is mainly determined by that of changes in Mediterranean atmospheric circulation. Spread in dynamical trends is difficult to reduce for this region. A GCM-selection approach based on storylines could be relevant to determine plausible scenarios (Shepherd 2019)."

Comment 6 *Page 5, line 142: the bias correction method is not detailed. What kind of approach is used beside the use of CDFt? In such a mountainous area, and since this study consider several variables in RCM simulations (temperature, precipitation, humidity...) a pixel-by-pixel and variable-by-variable bias correction with CDFt without considering the spatial correlation and inter-variable dependencies can lead to strong uncertainties. See Vrac et al https://hess.copernicus.org/articles/22/3175/2018/ It is quite surprising that the authors seems to apply a simplistic method for bias correction of RCM outputs while they develop a high-resolution physically based framework for snow simulations.*

Answer: The CFDt method is the base of our bias-correction approach. You are correct to point out that our pixel-by-pixel approach does not take spatial correlation and inter-variable dependencies into account, and the R2D2 method could be interesting to apply here. R2D2 allows to correct for spatial correlations in the same variable, as well as inter-variable correlations in time. For inter-variable correlations, one challenge though is that the target datasets used in the bias-correction come from different sources (TRMM, MODIS, etc.), and thus we should be very careful about relying on their correlations when correcting the data. To that end, it would be better to correct from a single observational dataset that includes all the variables we use, but obviously such a dataset does not exist. For spatial correlations, R2D2 could improve our approach. One caveat though is that since we fit the snow model based on long-term snow cover annual cycles (instead of an actual time series of observed snow cover), the role of inter-annual variability is somewhat put aside, and the simple CDFt would probably perform just as the more complex R2D2 method (since they will both yield roughly the same seasonal and long-term values). Overall, other uncertainties (and they are admittedly many) most likely dominate.

Comment 7 *Page 6, line 178: similar to my comment above, how do you compute catchment-averaged October-May precipitation ? with observed rainfall or TRMM ?*

Answer: Catchment-averaged precipitation is computed with MRCM output data, biased-corrected with TRMM. Station data is only used to validate the use of the TRMM dataset.

Comment 8 *Page 6, line 177: watershed-specific fixed effects, are the parameters fixed according to size, land use etc. ?*

Answer: In this simple model, watershed-specific effects are not specified other than by the model intercept. We did look at whether the value of the intercept could be related to simple watershed metrics like elevation or slope distribution, land use, etc. but found no clear relationships.

Comment 9 *Page 6, line 180: It is not clear if these sentences are results of sensitivity analysis or results of previous works*

Answer: The sentences from lines 180-184 relate to results from previous works and more citations, in addition to Davenport et al. 2020, should be added in a revised version (e.g., Berghuijs et al. 2017 https://doi.org/10.1002/2017WR021593; Duan et al. 2017 https://doi.org/10.5194/hess-21-5517-2017).

Comment 10 *Page 6, line 185: Only precipitation and temperature are bias-corrected ? Line 125 the author state they use 6-hourly wind speed, specific humidity, air temperature, precipitation, and downward longwave and shortwave from the RCM simulations. Later on in the text, relative humidity seems to be an important driver of change, therefore better explanations on the method used to bias correct this parameter (and others) are required*

Answer: This is a mistake, all the variables used in the model are bias-corrected (precipitation and temperature using satellite observations, other variables using the ERA-Interim reference run. The sentence "with temperature and precipitation bias-corrected as described previously" should simply be removed.

Comment 11 *Page 7, line 188, I don't understand this sentence "Therefore, we use the ERA/MRCM precipitation data, bias-corrected with TRMM,"*

Answer: The paragraph is indeed confusing. We simply mean to recall that precipitation in the MRCM runs is bias-corrected with the TRMM data (lines 125-128). These sentences would be removed in a revised version since the information is already contained in section 2.3.

Comment 12 *Page 8, line 213: the author mention a "statistical downscaling of the MRCM output to 1km,", but nothing in the method section about this. How is it possible to downscale 12km RCM simulations to 1km, with "reference" precipitation being TRMM data at 25km spatial resolution ? there is a confusion here between "downscaling" and "bias correction".*

Answer: More details are given in Tuel et al. 2020 J Hydrology but your remark makes it clear that we need to be more explicit about the methods. The approach involves a mixture of bias-correction and downscaling. The snow model is run at a resolution of 1km, but the bias-correction is applied at various resolutions depending on the resolution of the target datasets. Temperature is bias-corrected based on the MODIS data at a 1km resolution. Precipitation is bias-corrected at the TRMM resolution of 0.25°. Wind and humidity data are bias-corrected at the 12km resolution of the MRCM runs. Precipitation, downward longwave and shortwave,

wind and humidity are then further downscaled to the MODIS 1km resolution, using equation (1) (line 138) for humidity, but keeping the same value for precipitation, radiation and wind (i.e. no elevation correction).
We suggest moving the end of section 2.3 to a new first section in the methods and to reformulate it as follows: *6-hourly wind speed, specific humidity, air temperature, precipitation, and downward longwave and shortwave are extracted from the MRCM output over our domain. For all three GCM-driven simulations, as well as the ERA-Interim driven run (hereafter referred to as ERA/MRCM), air temperature and precipitation data are bias-corrected at the 6-hourly timescale using MODIS LST-derived air temperature and TRMM precipitation at their native resolutions as respective targets, via the CDF-transform method (Michelangeli et al. 2009). Bias-corrected temperature data is thus obtained at a 1km resolution, and bias-corrected precipitation data at a 0.25° resolution. Alone among the three GCMs, the IPSL-CM5A-LR model exhibits a negative bias in wet days that we correct at each grid cell by randomly generating wet days of magnitude drawn from the corresponding distribution of wet-day precipitation in the TRMM dataset. For bias correction, reference periods for "perfect" observations are 1998-2011 for TRMM and 2000-2011 for MODIS. The corresponding periods in the simulations are the same for ERA/MRCM, and the 1992-2005 and 1994-2005 periods, respectively, for each of the GCM-driven simulations. All bias corrections are performed for the cold (November-April) and warm (May-October) seasons separately. Additionally, we use wind speed, downward long- and shortwave radiation and specific humidity from the ERA/MRCM simulation over the 1982-2005 period as reference, since no observations are available. The corresponding variables in each GCM-driven simulation are therefore bias-corrected using the ERA/MRCM data as target.*
All bias-corrected variables at resolutions coarser than the MODIS 1km grid used for the snow model are then further downscaled to a 1km resolution. Wind, radiation and precipitation data are left unchanged, but specific humidity is downscaled based on an empirical lapse-rate μ estimated at each time step:

$$\log(q) = \log(q_{12}) + \mu \cdot (z - z_{12}) \tag{1}$$

where q_{12} is the specific humidity in a given 12-km resolution grid cell of elevation z_{12}, and q the downscaled value at elevation z.

Comment 13 *Section 4, results I see no validation of the methods applied, prior to produce future scenarios. What about the efficiency of bias-correction ? What about the efficiency of panel regression to reproduce the inter-annual variability of runoff coefficients ? Since the authors rely on TRMM rainfall, it would be interesting to see a comparison of the model driven by either observed rainfall or TRMM to reproduce discharge dynamics*

Answer: The efficiency of the bias-correction approach in general is discussed in detail in Tuel et al. 2020 J Hydrology, where we also compare to station data (not used in the bias-correction). We should add a reference to it in the revised version. The quality of the bias-correction is also discussed (though indirectly in the current version) when describing the performance of the individual simulations in reproducing accurate snowpack dynamics (snow cover on Figs. 5 and 6, snow-to-precipitation fraction on Fig. 10 and sublimation on Fig. 11). The results could be detailed, for instance with Table 1 which shows how the GCM-driven simulations compare in terms of input data and snow model output to the observations.
Regarding the efficiency of the panel regression method, it is already shown in Figure 12 and

discussed starting at line 256. However we agree that showing the performance of the model in reproducing river discharge for the recent (post-2000) period, using TRMM precipitation, with a cross-validation approach, would be useful. Results are shown in Fig. R1.

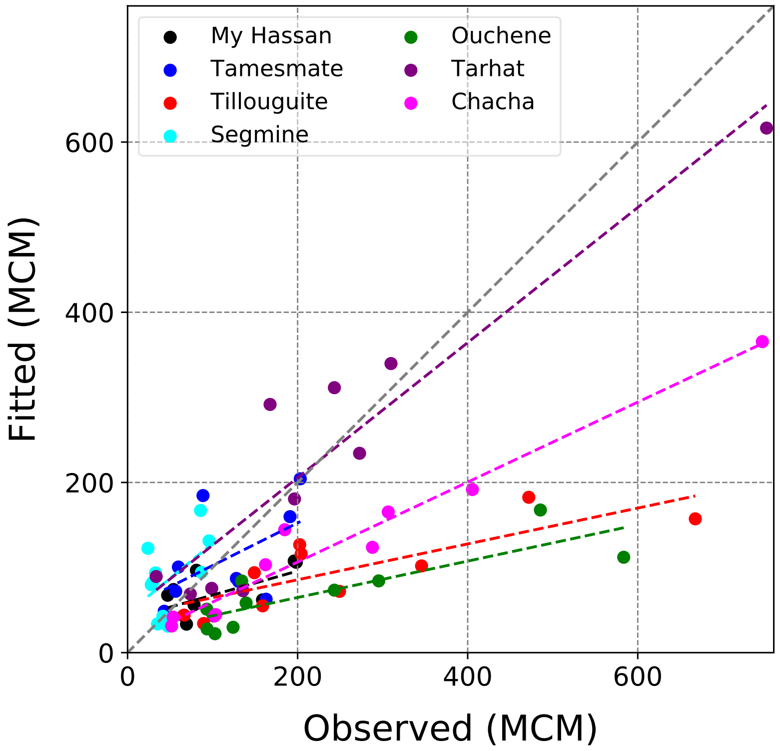

Figure R1: Observed and predicted annual October-May discharge (MCM) for the seven sub-catchments (2001-2010). The prediction is made using TRMM precipitation and runoff coefficients predicted by the statistical runoff coefficient model fitted on 1982-2000 data only.

Comment 14 *Page 10, line 280: it should be noted that the Oum Rbia basins has several areas with karstic functioning*

Answer: This is an important point to add to the discussion indeed. We suggest adding the following sentence on line 278: "Kartic areas are in addition quite frequent within the Oum-Er-Rbia watershed (Akdim 2015), with important implications for infiltration, aquifer and spring regimes in our study area."

Comment 15 *Page 10, line 294-297: this is not a result and should be in the introduction. The "Source: Direction de la Recherche et de la Planification de l'Eau, Rabat" is not in the reference list. This is not a result of the present study since the data and method used to obtain this result are not presented.*

Answer: You are correct and this sentence should be moved to the introduction.

Elevation	Ann. prec. (mm)[1]	DJFM prec. (mm)[2]	Ann. snow (mm)[3]	DJFM snow (mm)[4]	Snow frac. (%)[5]	Melt (mm)[6]	DJFM Temp. (°C)[7]	DJFM wind (m/s)[8]	DJFM RH (%)[9]	DJFM SWE (mm)[11]	Sublim. (mm)[13]
	419	194	95	70	23	87	4.5	2.6	52	6	8
All	433	217	84	63	19	80	4.4	2.6	56	6	5
	406/460	205/232	65/120	50/74	11/33	53/132	3.9/4.8	2.5/2.6	55/59	3/8	4/5
	609	275	354	218	58	254	-6.4	3.2	62	108	100
≥3500m	634	295	349	230	62	274	-5.9	3.1	62	100	75
	566/680	244/352	271/467	199/265	40/92	242/501	-6.4/-5.8	3/3.2	61/63	64/155	55/86
	586	262	306	191	52	252	-4.2	3.0	63	75	54
3000-3500m	626	293	345	213	58	303	-3.8	2.9	64	65	42
	564/670	256/326	226/510	185/245	36/90	220/437	-4.5/-3.3	1.8/2.9	62/66	43/92	31/47
	457	205	187	123	41	162	-0.3	3.0	59	25	25
2500-3000m	476	227	217	130	48	201	-0.5	2.9	61	19	17
	426/514	205/240	127/358	107/146	28/82	130/330	-1.2/0	2.8/2.9	59/63	12/27	14/18
	444	198	118	81	27	111	3.3	2.8	54	7	7
2000-2500m	453	221	137	81	31	132	2.9	2.7	57	4	5
	414/487	207/235	77/239	62/101	17/53	77/228	2.3/3.4	–	56/60	2/7	5/6
	409	182	47	39	11	46	7.0	2.5	49	2	1
1500-2000m	415	206	33	19	8	32	6.9	2.4	53	0	1
	394/440	193/225	17/52	13/23	4/12	17/52	6.5/7.2	2.4/2.5	52/56	0/1	0/2

Table 1: Snow model results and input data, averaged for the whole study area and various altitudinal bands: (1) Annual precipitation; (2) December-to-Mach (DJFM) precipitation; (3) Annual snowfall; (4) DJFM snowfall; (5) Annual fraction of solid precipitation; (6) Annual snowmelt; (7) DJFM air temperature; (8) DJFM wind speed; (9) DJFM relative humidity; (10) DJFM average snow cover; (11) DJFM mean snow water equivalent; (12) Fraction of area with ≥5% snow cover in DJFM; (13) Annual sublimation. For each elevation range, the top line indicates "observed" values for the 2001-2010 period (precipitation: TRMM; temperature: MODIS; wind and RH: downscaled ERA-Interim; snow cover: MODIS; and other variables: assimilated snow model results from Tuel et al. (2020a)), the middle line shows the 3-GCM average under the historical scenario (1995-2005 only), and the bottom line shows the 3-model range.