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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Evapotranspiration feedbacks shift annual precipitation-runoff relationships during multi-year droughts in a Mediterranean mixed rain-snow climate" *by* Francesco Avanzi et al.

Francesco Avanzi et al.

francesco.avanzi@polimi.it

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Dear Colleague,

Thank you very much for the thoughtful review of our paper. Please find below our point-by-point reply to comments, including our intended changes to the manuscript. Your comments are in italics, with our replies are in plain text.

The manuscript by Avanzi et al. is generally well-written and well-referenced. They utilize a hydrologic modeling approach to quantify how a mountain watershed responds

to short duration (sub-decadal) drought episodes, and find that how simulated evapotranspiration responds to drought conditions is a major issue with regards to runoff estimation. While unsurprising, this finding is important to motivate improvements in ET in hydrologic models in mountain regions. Overall, the modelling approach is acceptable, but will need some additional information and analysis (noted below), especially with regards to the ET estimation approach and some additional longer simulations.

I am also a bit skeptical of the fact that a groundwater model was not used to show whether or not such a model is necessary to accurately capture hydrologic responses in volcanic, subsurface flow-dominated basins. I liked the shift identification approach for changes in precipitation-runoff relationships. I also thought it was a valuable addition to include a range-wide assessment to highlight not only the application of the approach but also to show how basin response to drought varies as a function of elevation.

We agree that our results motivate improvements in ET parametrizations, especially since the observed shifts extend to the majority of water basins in the Sierra Nevada and occur regardless of predominant geology. We also agree that coupling a groundwater model to PRMS could shed further light on shifts in the basin water balance, and potentially precipitation-runoff relationships. While a coupled version of PRMS is available (GSFLOW), setting it up with the same level of spatial data and parameterization as it was done for PRMS would be a major new study, requiring an effort that goes well beyond the scope of this manuscript. Lacking the data needed to describe groundwater flow in the basin with a high level of rigor, it is our assessment that the simplified simulation of groundwater processes in PRMS is appropriate to meet the aims of this study. In this simplified setting, PRMS and many other hydrologic models with similar process representations are currently being used by water and energy forecasters in California and elsewhere to predict water supply at various time scales. Highlighting that such hydrologic models are prone to performance drops during shifts in precipitation-runoff relationships thus addresses an important aspect of operational hydrology. We will certainly add more discussion on this point in our paper, including suggestions to perform coupled experiments in the future.

Some general and specific comments follow below.

General comments: 1. The recent 2012-2015 drought period considered by the authors is inconsistent (P2 L33) with the official declaration period of drought on the website provided (P3 L25), which gives 2012-2016. The modelling exercises will need to be repeated to include water year 2016 if the authors would like to stick to the official declaration of drought. As they do note that the results are sensitive to the duration of drought episodes, it would be worth including (and comparing) the 2012-2015 (what could be called 'peak drought' conditions) and 2012-2016 (the 'official drought') periods.

We agree to extend the analysis to the 2012-2016 period. While WY 2016 had average precipitation, including it may give further insight into the drought response. Note, however, that there are multiple definitions of drought, and the one on the DWR web page refers to impacts on water users.

I think it would strengthen the paper to include some additional modeling studies of longer droughts, even if these are hypothetical. Many water agencies (and model experiments) use 'design' droughts based on a few known drought episodes lumped together or informed by past extended droughts. While I see equation 3 was applied by lumping results together on Page 6, I would like to see how a few iterations of a design drought of decadal (or longer) scale change (or do not change) the results.

We agree that scenarios involving longer droughts can inform water management, and hope that our results can improve those studies. As to using scenarios in the current study, that would involve developing different data sets that would have less certainty in assessing model response. All results in this paper rely on measurements, including detecting shifts in precipitation vs. runoff, assessing the performance of the PRMS model during droughts and wet periods, and comparing estimated and modeled water-balance components. While paleoclimatic datasets suggest prolonged, multi-

decadal droughts in California, it would thus be challenging to generate the observational dataset we need to fully apply our methods. In addition, drought vs. non-drought conditions in California have a strong interannual character because of the quasiperiodicity of El Nino–Southern Oscillation (https://doi.org/10.1002/2014GL062433), meaning that investigating these shorter time scales is functional to answering our research questions. We will add more discussion on this point.

2. PRMS is a fine modeling approach, but in regions with known groundwater/surface water interactions, such as the volcanic Almanor sub-basin, why was a groundwater model not also developed and coupled to PRMS (i.e., GSFLOW?). I understand this may not improve results, but without showing that it does not, it leaves the reader wondering if such an addition would improve results.

Please see response to first general comment, above.

3. P8 L5: The authors misconstrue the definition of 'warm snow drought', perhaps because the definition can be misleading if no thresholds in precipitation anomalies are specified. Though the 2012-2015 drought may have had greater precipitation than the 1977 drought, many of these years in both drought periods satisfied the dry snow drought conditions demonstrated in Hatchett and McEvoy (2018). Warm snow droughts should only be defined by years with near to above-normal precipitation and below average snowpack.

We agree and will clarify this point in the manuscript.

4. P8 L25: Snow/rain ratios should not be based upon a single point in time value of accumulated precipitation and snowpack at that time (April 1), as this neglects numerous factors that may be controlling the state of the snowpack on this date. For example, if a third of an above-average snowpack was lost during a warm, humid period in late March, the value on April 1 would not correctly represent the fact that otherwise a year had experienced above normal snow and perhaps above normal rain/snow ratios. Just a hypothetical example. I suggest calculating snow fraction over the course of the year *and using the total precipitation estimated as snow divided by total precipitation (ending on Apr 1) as a more robust metric.*

We agree with this suggestion and will try to recalculate results in Section 3.1 and Figure 2 using the suggested metric. A caveat here is that data reported in Figure 2 are monthly, and daily data may be not publicly available at some of the precipitation stations considered. Also, some of these precipitation data may lack co-located air-temperature (and optionally relative-humidity) data to estimate phase partitioning between rain and snow, and vice versa. During our revision, we will assess data availability from this standpoint and will propose our best estimate.

5. The procedure used to estimate ET is very interesting, but as this method has not yet been accepted by the scientific community (as noted by a submitted paper on P5 L4 and L6), I need to see comparisons of these results with some standard, easily implementable methods to calculate ET.

The procedure is a modification of the well-cited approach by Goulden et al. (2012), which has been used in multiple papers since then (see references to our work, below). The Rungee manuscript mentioned on P5 L4 and L6 will be submitted to HESS by R. Bales (Rungee has moved on to a new position), and if accepted as a discussion paper all methods related to the updated ET product we used here will be freely accessible online by the time the revised version of this manuscript will be submitted. The authors welcome public comments on the method in the Rungee manuscript, which moves from one independent variable (NDVI) in published papers by Goulden/Bales to 2 independent variables (NDVI and precipitation). Adding the additional variable both recognizes the responsiveness of ET to precipitation, independent of NDVI, and provides a lower error. The accuracy of this ET product will be also discussed in the revised version of this manuscript, and we will include an analysis of the 2-variable vs 1-variable approach in the supplement as needed. There is no independent, accurate spatial product for ET for the Feather River basin; however, we present a leave-one-out cross validation in the Rungee paper. Finally, we are also creating a DOI for the

flux-tower datasets and ET products in that paper, which are published in the Rungee (2018) paper cited in our manuscript.

References

1. Goulden, M.L., R.G. Anderson, R.C. Bales, A.E. Kelly, M. Meadows and G.C. Winston, "Evapotranspiration along an Elevation Gradient in California's Sierra Nevada," Journal of Geophysical Research, September 2012, Vol. 117, G03028.

2. Goulden, M.L. and R.C. Bales, "Mountain Runoff Vulnerability to Increased Evapotranspiration with Vegetation Expansion," Proceedings of the National Academy of Sciences of the United States of America, September 2014, Vol. 111, No. 39, pp. 14071-14075.

3. Goulden, M.L. Bales, R.C. California forest die-off linked to multi-year deep soil drying in 2012–2015 drought, Nature Geoscience 12,632–637 (2019)

4. J.W. Roche, M.L. Goulden, R.C. Bales. Estimating evapotranspiration change due to forest treatment and fire at the basin scale in the Sierra Nevada, California. Ecohydrol., 2018

5. R.C. Bales, M.L. Goulden. C.T. Hunsaker, M.H. Conklin, P.C. Hartsoug, A.T. O'Geen, J.W. Hopmans, M. Safeeq. Mechanisms controlling the impact of multi-year drought on mountain hydrology, Scientific Reports, 2018.

6. A.W. Fellows | M. L. Goulden. Mapping and understanding dry season soil water drawdown by California montane vegetation, Ecohydrol.2017

6. The concept of climate-driven ET elasticity is very cool. I would recommend a schematic figure be added to highlight this concept. The additional model runs using longer drought episodes (warm and dry versus cool and dry at decadal time scales) could play into making this figure more robust by helping constrain the temporal and climate condition sensitivity of the elasticity.

We will work on a schematic as suggested.

7. The map figures (Figure 1) are not up to publication standards. They need to be projected with latitude and longitude coordinates. The bins of precipitation are far too large and substantial precipitation variability is lost. The inset map needs to be projected and should show the entire west coast of North America (or at least the western United States). I suggest simply binning precipitation by 50 mm increments to better highlight topographic gradients. The geologic mapping appears to be hand drawn (and is of very poor quality) and needs to be markedly improved. The map should also show the locations of the stations used in the study since they are referenced in the main text.

We will revise Figure 1 as suggested. The geological map was not hand drawn and is an adaption of the USGS National Atlas in which groups were aggregated based on the main geology (volcanic, sedimentary, granitic).

Specific Comments: P3 L11: Please change all instances of Oroville Lake to the correct title, "Lake Oroville"

P3 L26: When referring to climate, one must not neglect temperature if discussing precipitation. Please change to "dry, hot summers and wet, mild winters".

P3 L28: It would help to add a figure demonstrating the basin hypsometry to quantify 'most'. Does "most" mean 59% or 92%?

P4 L4: Add 'anomalous' before 'low'

P4 L13: Should be "Cascade Range".

P4 L14: Suggest to add more geologic context, specifically on soils.

We will incorporate all comments above in the revised manuscript.

P4 L22: Please provide the temporal resolutions used. Perhaps a table could be used?

We agree and will add a Table to the Supporting Information summarizing details about the temporal-spatial resolutions of all data used.

P4 L22: What are the resolutions of the spatially distributed indices?

The temporal resolution is annual (see P4 L22). The spatial resolution is discussed on P5 L9-10. We will include this resolution on P4 L22 too.

P4 L29: What types of precipitation gauges were used? Were gauges heated? Are there concerns for undercatch?

Precipitation gauges used in the river basin are managed by various agencies, with the California Department of Water Resources collecting and archiving the data. We will add the responsible agency to Table S1. The design of these sensors resembles the one in use by the SNOTEL network throughout the western US (https://www.wcc.nrcs.usda.gov/about/mon_automate.html). Most gauges are unheated and some are manually measured by on-site agency personnel. Most are also located in small clearings where wind speed is low, which suggests that undercatch is locally low, especially below the seasonal rain-snow line. Nevertheless, we agree that undercatch increases in snow-dominated regions and will add all these details, plus a discussion of the effect of undercatch on our results, to the manuscript.

P5 L1: Which PRISM products were used, 4 km or 800 m? Monthly or daily?

The spatial resolution is reported on P5 L9 (800 m), while the temporal resolution was annual (see on P4 L22). Note that PRISM data in this paper were also used through the DRAPER approach to estimate precipitation input data for PRMS (see on P5 L29ff). For this second scope, we used monthly polygon surfaces of precipitation contours (P5 L29). All this information will be summarized in the Supporting Information.

P5 L25: What alpha level was significance assessed at?

This information is reported on P9 L19 but we will include it at P5 L25 as well.

P8 L1: I'm a bit confused here, is the paper referring to flow below Oroville, or total inflow to Oroville? If the former, these numbers need to be prefaced by discussion of water deliveries that may have been subject to changed allocations in response to the drought. Similarly, the storage value in the reservoir doesn't really add much, given that a reservoir's operational goals might be to completely drain the reservoir by the end of the water year. If additional context is provided, then this number becomes meaningful. Last, please correct 'norm' to the proper spelling 'normal'.

Based on our understanding of the technical report cited at line 2 (DWR, 1978), this is full-natural flow at Lake Oroville, which is comparable to inflow to the reservoir and is independent from water-allocation decisions downstream of the dam. It is true that reservoir storage is highly seasonal, but it also responds to multiple objectives that may require reservoir level to be maintained to a certain high level (e.g., recreational reasons, hydropower production, multi-year carryover for water supply, etc). We will add this context as requested and also correct the word 'normal'.

P8 L3: For consistency with the discussion on the 1987-1992 drought on P8 L8, please include the temperature anomaly for the 2012-2016 (note I used the official period there, not 2012-2015)

P8 L9: Section should be singular.

We will include all these corrections in the revised manuscript.

P8 L9: The start of this paragraph is a bit strange, i.e., is there any reason to suspect that runoff seasonality should not be preserved? I think this sentence could be removed or replaced with some more insightful findings. Perhaps discuss any temporal shifts?

Runoff seasonality depends on snow-rain proportion and ultimately on the timing of precipitation input to the system, both of which may be affected by droughts. Therefore, runoff seasonality may in principle not be preserved. We will revise this paragraph to clarify this point; we will also discuss temporal shifts as kindly suggested.

P10 L11: Can you add some additional clarity about these winter precipitation events? I would expect these to be extreme precipitation rain-on-snow events to produce peak flows, but as it is written, any precip event could generate a peak flow event.

The Feather river lies across the seasonal rain-snow transition zone. As such, rain-on-snow as well as mixed rain-snow events are frequent. These events significantly increase streamflow compared to periods with no precipitation, as typical hydrographs on the river show (see https://pubs.usgs.gov/sir/2004/5202/sir2004-5202.pdf, page 27). In the revised manuscript, we will include some of these hydrographs in the Supporting Information to clarify this point.

P10 L30: I am balking at the use of 'observed' water balance, as that implies that the water balance has been completely observed, when in reality it is merely an estimate based upon models for precip (PRISM) and ET (NDVI-GAM approach). "Estimated" might be a better word but could confuse readers against "modeled". I leave this to the authors to ponder if a better descriptor could be used

We agree and will use 'estimated' in the revised manuscript. In so doing, we will add one sentence in Section 2.3.3 to clearly define what we mean with 'estimated' and 'modeled'.

P11 L8: What is "soft data"?

Measuring sub-surface-storage decline is challenging, meaning one has to rely on indirect observations (e.g., magnitude of low flows or rate of seasonal flow from springs). Some of these indirect observations are discussed in Freeman (2011). This was our intended meaning of 'soft data', but we will revise the manuscript using some of the wording we used here to clarify this point.

P12 L25: Suggest to add a citation for the second and third sources of uncertainty.

Bales et al. (2018), which is cited at the very beginning of this paragraph, is the main citation for all the sources of uncertainty in this paragraph. We will clarify this point.

P13 L6: I like the concept of tree mortality (despite the myriad complexities controlling tree mortality in addition to ET like pests, disease, etc), but I feel like this sentence detracts from the previous, powerful statement defining climate elasticity of ET. Ending the paragraph with this definition and instead another elucidating sentence about the value of this metric would be a strong way to close out this subsection.

We will edit the section as suggested.

Table 1: Please add the period of record means (or medians) for each variable. These statistics are not provided in Figure 2 as the caption implies.

We will add the period of record as suggested. Figure 2 reports annual average maximum and minimum temperature, annual quartiles of cumulative precipitation and April1 SWE, and annual April1 SWE / cumulative precipitation. We aggregated these values to obtain statistics in Table 1. This was our intended meaning of caption in Table 1. This will be revised.

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