

Anonymous Referee # 1, 29 Mar 2023

- The authors would like to thank Referee #1 for their time and feedback. Their suggestions will greatly improve the reasoning and clarity of this manuscript.
- Author note: Referee comments are reproduced in Times New Roman font. Author responses to referee comments will be included as bulleted, Arial-font text beneath the relevant comments.

Thank you for the opportunity to review this paper, it was an engaging exercise. Overall this paper presents an interesting and technically sound approach to develop, calculate, and predict seasonal flow metrics using available hydro-climatic data to facilitate adaptive water management for an upcoming spring and summer season. The results suggest this could be a very useful approach for local water managers and stakeholders in the study watershed. A few major issues to be addressed:

I am not sure that this study really took a functional flows approach as the authors suggested. In the conclusions these hydrologic metrics are framed as decision-support metrics, which seems a more appropriate couching for what has been done. It is reasonable to state that the metrics are linked with ecological functions, but that is not the same as applying a functional flows approach or the functional flow metrics developed for CA. Next, this study focused on a single watershed, but I would encourage the authors to try to broaden the paper discussion and limitations sections to how this can or can't be applied in other settings. For instance, the analysis relied on long climate records and a detailed hydrologic model. Are these data always needed for such an analysis? Are the findings transferable to other systems? Additionally, there are a lot of great ideas that are raised in passing that could use additional thoughtful discussion and citations to really bring to light. My remaining concern is the quality of writing in the manuscript, including terminology, grammar, sentence and paragraph structure issues. I must note that the repeated references to other chapters of your dissertation feels unprofessional. As a reviewer I would like to know that you have put in your best effort to make this a clean, standalone manuscript and not simply submitted your dissertation chapter to a journal directly. I look forward to reviewing a revised version with these changes addressed so the reader can really focus on the interesting research and implications.

- We identify 3 main critiques in the general comments provided by Referee # 1.
- Regarding the framing as a functional flows exercise: the manuscript introduction and discussion has been revised to clarify that this study does not apply a typical functional flows approach; rather it uses the existing functional flows framework to provide context for a proposed new approach to developing a decision-support tool that would be specific to a target watershed.
- Regarding the recommendation to broaden the discussion for applying to other settings: additional text has been added to the discussion to outline how this approach could be applied to other watersheds (with abundant hydroclimate data). But we also point out that the assessment of the feasibility of this generalization would be beyond the scope of the subject study and should be explored in a future investigation.

Response to Referee # 1

- The proposed edits will improve the writing quality and readability of the manuscript. References to dissertation chapters has been removed and other structural changes have been made (i.e., moving the model diagnostics figures to an Appendix) to produce a standalone manuscript.

Specific comments:

1. Abstract: In general, I would suggest to provide more general, compelling information and not include variables, multiple units, HUC#, etc in an abstract. See additional in-line comments.

- We have revised the abstract as follows:
- Abstract. In undammed watersheds in Mediterranean climates, the timing and abruptness of the transition from the dry season to the wet season have major implications for aquatic ecosystems. Of particular concern for resource managers in many coastal areas is whether this transition can provide sufficient flows at the right time to allow passage for spawning anadromous fish, which is determined by dry season baseflow rates and the timing of the onset of the rainy season. In (semi-) ephemeral watershed systems, these functional flows the dry season baseflow and rainy season onset timing also dictate the timing of full reconnection of the stream system. In this study, we propose methods to predict, approximately five months in advance, two key hydro-meteorologic metrics in the undammed rural Scott River watershed (~~HUC8-18010208~~) in northern California. ~~Both~~ The two metrics are intended to quantify the relative transition timing from the dry to the wet season, and to characterize the severity of a dry year. The ability to predict these metrics in advance could ~~and~~ support seasonal adaptive management. The first metric is the minimum 30-day dry season baseflow volume, ~~V_{min}, 30 days~~, which occurs at the end of the dry season (September-October) ~~in this Mediterranean climate~~. The second metric is the cumulative precipitation, starting Sept. 1st, necessary to bring the watershed to a “full” or “spilling” condition (i.e. initiate the onset of wet season storm- or baseflows) after the end of the dry season, ~~referred to here as P_{spill}~~. As potential predictors of these two ~~values~~ metrics, we assess maximum snowpack, cumulative precipitation, the timing of the snowpack and precipitation, spring groundwater levels, spring river flows, reference ET, and a subset of these metrics from the previous water year. ~~We find that, t~~ hough many of these predictors are correlated with the two metrics of interest, ~~of the predictors considered here, we~~ find that the best prediction for both metrics is a linear combination of the maximum snowpack water content and total October-April precipitation. These two linear models could reproduce historic ~~values~~ metrics of V_{min}, 30 days and P_{spill} with an average model error (RMSE) of 1.4 Mm³ / 30 days (19.4 cfs) and 25.4 mm (1 inch), corresponding to 49% and 37% of mean observed values, respectively. Although these predictive indices could be used by governance entities to support local water management, careful consideration of baseline conditions used as a basis for prediction is necessary.

2. 1-2 sentences should not be standalone paragraphs, as is currently done several times (L51, L72, L124, L369, etc). Please revise your paragraph and section structures accordingly.

Please use “functional flows” throughout to be consistent with the published literature

- The paragraphs on L51, L72, L75, L124, and L369 have been edited or combined with other text as suggested.
- Terminology has been standardized to use “functional flows” throughout as suggested.

3. Tense issues throughout manuscript, particularly in the Results - past, present, and "have been...". Choose one (I suggest past tense to be consistent with most journal articles) and use consistently throughout.

- The tense of the Results section has been revised to be consistently past-tense.

4. Some steps in the methods remain unclear. For instance, 2.3.1. is the first mention of a "model" and there is no indication of what type of model you are trying to develop or why (e.g. linear regression modeling to predict X as a function of Y...).

- The sentence "In this study we used linear regression modeling to predict watershed behavior at the end of the dry season (the response) using data available the previous spring (the predictors)" has been added to the beginning of the Methods section for clarification.

5. There are some grammatical and spelling issues to be addressed (see inline comments) Some terms could be more clearly defined or concepts more clearly described (e.g. echo effect, partial one-year holdover, GSP).

- These grammatical and spelling issues have been revised as described in responses to inline comments below.

6. Please provide a more clear explanation for the selection of the Q spill threshold. Looking at Panel A in Figure 4, since individual hydrographs cannot be clearly distinguished I find myself struggling to fully understand how you visually determined this threshold.

- We have conducted a more detailed analysis, based on rainfall-runoff responses in dry and wet seasons, to support the selection of the Q spill threshold. This has been concisely described in the Results section and included in more detail in an Appendix.

7. Figure 8 and other map figures – add scale bar, north arrow, and in Fig 8 additional points of reference in the Scott watershed (e.g. gage locations, etc). Also, there are a lot of figures. Could any be combined or moved to SI to simplify the message?

See other inline edits in PDF.

- A scale and a north arrow have been added to Figures 3 and 8.
- Figure 8 (wells) and Figures 9-14 (model evaluation figures) have been moved to an Appendix.

In-line edits:

Page 1:

If you are talking about natural/unimpaired flow patterns, then I don't think concern is the right word for trying to understand the natural processes that will vary from year to year

- The phrase “for resource managers” has been added to clarify the concerned party (see revised abstract in response to Specific Comment 1).

which? FFs have not been defined yet

- The two key flows have been identified by name rather than being grouped under the term “these functional flows” (see revised abstract in response to Specific Comment 1).

flow metrics?

- Text has been clarified to say “hydrometeorologic metrics” rather than “hydrologic metrics”, to reflect the distinction that only one of the two metrics is directly related to flow; the other concerns cumulative rainfall. (see revised abstract in response to Specific Comment 1).

Remove text “(HUC8 18010208)”

- The suggested text has been removed (see revised abstract in response to Specific Comment 1).

Remove text “in this Mediterranean climate”

- The suggested text has been removed (see revised abstract in response to Specific Comment 1).

Page 2:

Suggest replacing “ones” with “those”

- The text has been revised as suggested.

To...

- The text “by water agencies to inform adaptive management decisions” has been added to clarify the sentence.

Suggest replacing “functional ecosystem flows are a” with “the functional flows approach is a...”

- The text has been revised as suggested.

Remove text “(see Chapter 1 of this dissertation)”

- The text has been removed as suggested and replaced with the text “(Moyle 2002)”.

Replace “modern” with “current” or “ambient”

- The text has been revised as suggested.

Replace “baseline” with “unimpaired” or “natural”

- The text has been revised as suggested.

Remove text “HUC8”

- The text has been revised as suggested.

What type of decisions?

- The text “such as agricultural cropping choices or regulatory water use restrictions” has been added for clarification.

Replace text “For example,” with “Specifically”

- The text has been revised as suggested.

Do you have a citation for this strong statement? Is that analysis done in the CDFW 2015 study, or is that citation in reference to the second noted linkage, to spawning habitat?

- The linkage to spawning habitat and the citation has been clarified.

Page 3:

Suggest inserting text “runoff”

- The text has been revised as suggested.

Page 4:

Replace “functional flow types” with “functional flows” or “functional flow components”

- The text has been revised as suggested.

Add “r” to correct typo

- The text has been revised as suggested.

Which concept? FFs? threshold-based runoff response? Be specific

- The text “basin-scale, threshold runoff storm response concept from the temporal scale of a season” has been added for clarification.

This paragraph needs citation

- Two citations and additional clarifying text have been added per suggestion.

Page 6:

Suggest changing word choice of “hydraulic response”

- The phrase “hydraulic response” has been replaced with “flow surge”.

Standardize “functional ecosystem flow” as “functional flow(s)” throughout

- The text has been revised as suggested. In this specific instance the word “ecosystem” has been removed.

Citations needed to support paragraph including manuscript line 130

- Citations have been added as suggested.

That is the fall pulse as we defined it... or I'm not clear what you are describing and how it is different than the fall pulse. There is also a start of wet season metric and a wet season baseflow metric. Could you clarify how your additional proposed metric differs from what already exists?

- Specification of a slow increase in flow, rather than an abrupt “pulse”, has been added to the text for clarification.

Tense: replace “can use” with “used”

- The text has been revised as suggested.

Page 7:

Tense: make “test” past tense

- The text has been revised as suggested.

Consider alternate word choice for “durable”

- The phrase containing the word “durable” has been removed from this paragraph.

Page 8:

Remove text “and Chapter 1 of this dissertation”

- The text has been revised as suggested.

Replace text “uncertain” with “unpredictable”

- The text has been revised as suggested.

Insert text “station records”

- The text has been revised as suggested.

This appears to be the first mention of a "model", at least in the last few pages as far as I am seeing. The type of modeling being done needs to be clearly stated upfront.

- The sentence “In this study we used linear regression modeling to predict watershed behavior at the end of the dry season (the response) using data available the previous spring (the predictors)” has been added to the beginning of the Methods section for clarification.

Page 10:

Figure 3 should have a scale bar and north arrow

- The figure has been revised as suggested.

Page 11:

has this (“partial one-year holdover term”) been defined already? Otherwise a definition would be helpful

- The term has been replaced by a short definition.

Change “indicate” to “indicates”

- The text has been revised as suggested.

This (determination of the QSpill threshold) is a critical part of your methods and could benefit from more detail on how this was determined.

- A more detailed analysis, based on rainfall-runoff responses in dry and wet seasons, has been conducted to support the selection of the Q spill threshold. This is concisely described in the Results section and is included in more detail in an Appendix.

Page 12:

Figure 4 Panel A has a key but the line is not visible.

- This oversight has been corrected – the line has been added to Panel A.

Page 13:

Tense issues throughout results - past, present, and "have ...". Choose one (I suggest past tense to be consistent with most journal articles) and use consistently throughout

- The tense of the Results section has been revised to be consistently past tense.

Well identifiers: use a shorter identifier or move to SI if critical information.

- The well identifiers are not critical and have been removed.

Page 17:

“Conversely” not an appropriate first word in paragraph

- The text has been revised.

“cumulative ET0 Oct-Apr”: confusing order, be consistent

- The text has been revised for consistency.

Is this something you are certain of, or your interpretation? Either way, why is this?

- The text has been revised for clarity.

“echo” effect: I am not familiar with this term. If you use this because you have seen it in other papers, cite it, and it should not be in quotes. Otherwise do not come up with new terms and just describe clearly what is happening

- The text has been revised as suggested.

very long sentence, consider splitting into 2

- The sentence has been shortened.

Replace “worse” with “larger”

- The text has been revised as suggested.

Page 22:

It would be more clear to label panels with letters for consistency - for this and following multi-panel figures.

- All multi-part figures in the main text have been labeled with panel letters. Figures 9-13 have been moved to an Appendix.

Page 23:

Replace “upward” with “increasing”

- The text has been revised as suggested.

Page 26:

Remove text “state of being”

- The text has been revised as suggested.

Page 28:

Figure 15: Is the third panel necessary?

- The third panel is necessary to illustrate that a long-term trend is not as visible in the DWR water year type schematic than in the quantitative indices P spill and V min.

Page 29:

Remove text “(see water budget information in Chapter 2 of this dissertation)”

- The text has been removed and replaced by the citation “(DWR 2004)”.

Page 30:

Could this not be done already with existing data?

- Existing data on human land and water use has been incorporated into the Scott Valley Integrated Hydrologic Model, and in the authors’ judgement, remains too coarse to confidently parse the year to year influence of human actions on summer surface flow, relative to climate inputs.

Remove text “referred to as Vmin, 30 days”

- The text has been revised as suggested.

In the introduction these are framed as functional flow metrics. These are very different concepts that should be reconciled

- The manuscript introduction and discussion have been revised to clarify that this study does not apply a typical functional flows approach; rather it uses the existing functional flows framework to provide context for a proposed new decision-support tool in a specific watershed.

Anonymous Referee # 2

- The authors would like to thank Referee #2 for their time and feedback. Their suggestions will increase the robustness of the analysis, and greatly improve the structure and readability of this manuscript.
- Author note: Author responses to referee comments will be included as bulleted, Arial-font text beneath the relevant comments.

4. Are the scientific methods and assumptions valid and clearly outlined?

YES, but the authors need to consider information criteria as an alternative to LOOCV for model selection. I think the data set could potentially support models with more than two predictors. Information criteria could identify if that is the case, but the authors chose to search only for two-parameter models. This potentially limits predictive ability and scientific understanding. And, the diagnostics of the final model need to be presented for the readers to fully assess its utility.

- Unfortunately, the AIC calculated for these models will not be able to identify the strictly best model, because the sample size is different for different combinations of predictors. This is because different hydroclimate records have different start and end dates, and some have missing values in some years.
- The referee correctly points out the benefit of additional model diagnostics in this manuscript, and the structural advantages of placing this diagnostic information in an Appendix. This was implemented as suggested.

10. Is the overall presentation well structured and clear?

NO. It is a chapter from a dissertation that has not been sufficiently reformatted to stand on its own and conform to standards of a journal article. The figures are not numbered in the order in which they are referred to in the text. As a result, the methods and logic are harder to follow. Fewer figures with better organization would improve readability.

- We apologize and agree. We have made several revisions (see also reviewer #1) to ensure that the manuscript now reads as a stand alone journal paper, not a dissertation chapter.
- The figure references were revised to reflect the order in which they appear.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

YES, but they are a little clunky. Shorter abbreviations and variable names would make the paper easier to read and understand.

- The abbreviation for $V_{\min, 30 \text{ days}}$ has been shortened to V_{\min} .
- The abbreviations for $SWJ_{\max, i}$ and $FJRS_{\text{Oct-Apr}, i}$ have been shortened to SWJ_i and $FJRS_i$.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

YES. The number of figures could be reduced. In particular, figures 9-14 could be put into supplementary material or an appendix.

- The figures were moved to an appendix as suggested.

14. Are the number and quality of references appropriate?

YES, except for references to alternative model selection procedures.

- Additional model selection references were added as suggested.

15. Is the amount and quality of supplementary material appropriate?

I didn't see references to any supplementary material, but I suggest that figures 9-14 could be put into a supplement.

- These figures were moved to an appendix as suggested.

General Observations

Manuscript hess-2023-41 “Seasonal prediction of end-of-dry season watershed behavior in a highly interconnected alluvial watershed, northern California” by Kouba and Harter provides an important contribution to the literature on behavior of alluvial river systems in Mediterranean climates. These types of river systems are ubiquitous around the globe and often have high ecological significance. In California and southern Oregon alone, these rivers support imperiled runs of anadromous fish, and the end-of-dry season behavior is critical to the migration and spawning success of these fish. The authors identify and propose predictors of timing of reconnection of surface flow in the Scott River, Klamath Basin, using key hydrologic indicators known five months in advance. The final predictive model uses peak snow water equivalent and total October-April precipitation as predictors. The authors use leave-one-out-cross validation (LOOCV) to select the best model, but they restricted model selection to a candidate set that consisted only of models with one or two predictors. This choice of methodology eliminates more highly parameterized models that could be better by some criteria and also restricts the amount of scientific insight than can be gained through the model-selection procedure. Further, presentation of additional model diagnostics, goodness-of-fit, and measures of predictive uncertainty beyond those depicted in figures 9-14 would improve the reader’s interpretation of how the final model will perform when used in a predictive capacity among a potentially larger set of models that could be considered. I highly recommend that the authors use an information criterion such as AIC for model selection. Although AIC and LOOCV are asymptotically equivalent for selection of regression models—and in this case may well end up producing the same model—application of AIC or other information criteria to a wider candidate set would offer much more scientific insight, especially if presented in the context of multi-model inference. I recommend acceptance upon major revision, with that revision consisting primarily of use of an information criterion for model selection. The manuscript would also benefit from some restructuring and from moving figures 9-14 (and any additional diagnostic figures) to a supplement.

- Unfortunately, the AIC calculated for these models will not be able to identify the strictly best model, because the sample size is different for different combinations of predictors. This is because different hydroclimate records have different start and end dates, and some have missing values in some years.
- However, the referee correctly points out the benefit of additional model diagnostics in this manuscript, and the structural advantages of placing this diagnostic information in an Appendix. This was implemented as suggested.
- Figures 9-14 and any other model diagnostics were moved to an Appendix as suggested.

Specific Comments.

1. The manuscript is obviously a chapter from a dissertation, and it needs quite a bit of structural rearrangement to be suitable for a stand-alone peer-reviewed publication. First, there are numerous references such as “See Chapter 1 of this dissertation” that need to be either replaced with formal citations of the dissertation or of other peer-reviewed papers or eliminated. Second, the figures are not numbered in the sequence in which they are first referenced in the text, which is contrary to most journal style guides and makes it really hard to follow the logic of the paper. Third, there are several acronyms used that are not defined. Most likely these were defined earlier in the dissertation, but they need to stand alone in this paper. Finally, again with respect to figures, figures 9-14 serve as model diagnostics and should be moved to a supplement.

- The chapter references have been removed as suggested.
- The figures are now referenced in order.
- Acronyms have been defined.

- Figures 9-14 (and any other model diagnostics) were relocated to an Appendix.

2. Mathematical quantities have very cumbersome notation. I appreciate that the nomenclature is complete enough to describe the quantity (e.g., $VV_{mmmmmm,30}$ ddddddd as 30-day minimum dry season streamflow volume), but after an initial definition, a much more concise symbol would make the manuscript easier to read. I think VV_{mmmmmm} would be sufficient. Once selected, make sure the variables are consistently presented in the text, tables, figures, and figure captions. That is not currently the case.

- The variable names have been shortened as suggested, and variables will be made consistent throughout the text, tables, figures and captions.

Additionally, the two equations in the manuscript, which are currently not numbered, are simply algebraic linear equations, which do not need explicit formulaic listing in the manuscript. The one on line 301 explicitly lists the predictor variables, which also have very cumbersome notation. A table listing the final model coefficients and their standard errors would be more informative and easier to follow.

- The final model coefficients and their standard errors have been included in a table.
- The authors propose to number and retain the simple algebraic linear equations for ease of communicating the prediction method to a broad range of stakeholders.

3. Although leave-one-out-cross validation (LOOCV) is a widely accepted and defensible model-selection method, the value of this research would be much greater if an information criterion (IC) such as AIC, BIC, or FIC were used as the model-selection technique. At the very least, the authors should acknowledge that IC are widely used and provide statistical justification for why LOOCV is used instead of an IC. Two good references on IC are Burnham and Anderson (2002) “Model Selection and Multimodel Inference” and Claeskens and Hjort (2008) “Model selection and model averaging.” In defense of LOOCV, both of these books indicate that for simple regression models like the ones the authors fit, LOOCV and Akaike’s IC (AIC) are asymptotically equivalent, meaning that they will select the same optimal model from a candidate set given a large enough sample size. So, it’s possible that use of AIC will produce the same optimal model as LOOCV in this case. Further, although the authors cite a reference that models be restricted to one or two predictors with a sample of size 80 to avoid over-fitting, this rule of thumb is not a good guideline to follow in the era of fast computing and default calculation of AIC and BIC in all of R’s model-fitting functions. The modern scientific literature is full of examples in which top models selected with an IC had three or more predictors, even with sample sizes smaller than 80. If this data set will only support a two-predictor model without overfitting, the IC will identify that, but without even entertaining the possibility in the candidate set, there is no way of knowing whether there may be a better model that has more than two predictors. Lastly, if an IC is used and the results are presented in tabular form showing model likelihoods, relative IC weights, and parameters included in each model, the scientific value of this research would be much higher, even if the IC selected the same optimal model as presented in the current version of the paper. Figures 9-10 and 12-13 in the current version present the different one- and two-parameter models, but it is difficult to tease out the same kind of information that would be readily apparent from an IC table. For example, figures 12 and 13 show that the best two-parameter model includes maximum SWE and Oct-Apr precipitation as predictors, with LOOCV error of 461. Looking at the single-parameter models, it is apparent that of these two predictors, Oct-Apr precipitation is by far the stronger predictors, with LOOCV error of 496. Addition of peak SWE improve the model relatively little. But, this observation would be much easier to glean and much more strongly quantified if the two models appeared in an IC table.

- Unfortunately, the AIC calculated for these models will not be able to identify the strictly best model, because the sample size is different for different combinations of predictors.

This is because different hydroclimate records have different start and end dates, and some have missing values in some years.

- Discussion of AIC as a common model selection tool has been added to the Methods section.
- Tables of model diagnostics (including likelihood, AIC, LOOCV, and R squared values) have been included in the Appendix, and discussed in the model selection methods.
- Tables are now presented for one-, two- and three-predictor models (in the Appendix).

4. Regardless of model selection technique, more model diagnostics are needed. Currently, LOOCV error and the scatter plots in figures 9-10 and 12-13 are the only reported diagnostics, making it somewhat difficult to assess whether all of the assumptions of linear model fits have been met. The scatterplots generally indicate that assumptions have been met, but R's standard four diagnostic plots would be a much better way to confirm that assumptions have been met. These should go in a supplement, but they should be included.

- The standard 4 diagnostic plots for the selected models and a brief interpretation have been included in the Appendix.

In addition, although RMSE is the model selection criterion in LOOCV and hence should be reported, it does not provide the reader with easily interpretable information about how good the model is in absolute terms. This is true of ICs as well. In either case, some other *relative* measures like MAR, R^2 , or Wald's z or t statistics on the estimated parameters (parameter value divided by standard error) or predicted values provide much more information about model performance in prediction. The best model by IC or LOOCV is the best model in the candidate set, but it may not be a very good predictive model. In this case, the best model for P_{spill} has an average RMSE of 20.7 mm, relative to observed mean P_{spill} of around 60 mm. This means that a 95% prediction interval around the estimated value of P_{spill} is roughly $2 \times 20 = 40$ mm on either side of a quantity with a mean value of 60 mm. Further, the "strong" correlations you refer to (e.g., top of page 17) are not very strong in reality. R values of 0.5 to 0.73 are equivalent to model R^2 values of 0.25 to 0.53, which are low to moderate at best for predictive models.

- The description of "strong" correlations has been clarified as suggested.
- Additional context regarding the overall utility of the predictive models will be added as suggested.
- R^2 values have been included in model diagnostic tables in the Appendix.

Some mention of attention to model assumptions should be indicated in the text. Related to this, the authors did assess the potential lags in some predictors, which is a good idea in any system with strong groundwater influence. However, using ARIMA models with appropriate lags included is a much more statistically defensible way to do this than explicitly lagging the predictors. For the purposes of correlation with other predictors (Figure 7), explicit lagging is fine, but in model selection, ARIMA models with different AR components can be included in the candidate set and ranked with the IC right along with all other models. Again, use of an IC and multi-model table would provide a lot more information about the role of antecedent watershed conditions on the response variables of interest.

- Model assumptions have been briefly addressed when discussing the four standard diagnostic plots for the selected models in the Appendix. The authors feel that the existing exploration of potential lagged effects is sufficient to document a much smaller impact than same-year hydroclimate conditions, and that unfortunately additional

discussion of lagged hydrologic effects using methods such as ARIMA models are beyond the scope of the present study.

- Multi-model diagnostic tables for one, two and three-predictor models for Vmin and Pspill have been included in the Appendix.

5. Some additional explanation of the characteristics of the three time periods would be helpful to provide context for the results. I suggest doing that in the introduction, rather than waiting until the results (line 240) to present that information.

I agree with the authors' delineation of the three time periods, but I suggest expanding a little more on climatic differences among the time periods. I agree that 1977 coincided with widespread implementation of groundwater irrigation, but some large-scale climate indicators such as the Pacific Decadal Oscillation also changed around 1977. In other parts of the West, most hydrologists would come up with the same time-period delineations as the authors have for the Scott River, and these would be based solely on climatic factors. Climate was fairly stable prior to the late 1970s, it was highly variable from the late 1970s through 1999 and included some very wet and very dry years occurring in close succession, and very dry from 2000-present. Also across the West these climate periods were coincident with changes to water use and irrigation practices—such as the increase use of groundwater in Scott Valley starting in the late 1970s or the widespread conversion from flood to sprinkler irrigation in other parts of the west prior to the late 1990s. The intersection of climate and water use has made it very challenging to unravel the relative contributions of climate change and water use/management to observed streamflow changes. The authors have done a good job of presenting quantitative analysis that is useful for predicting important hydrologic parameters regardless of how those parameters have changed over time and regardless of reasons for the change. That is a strength of this paper. But, my two cents is that the paper could be even stronger with definition and more discussion of the three time periods right up front in the paper's introduction.

- The three time periods have been introduced in the introduction, as suggested.
- Additional context, including the difficulty of disentangling the effects of human water use changes and decadal-time scale hydroclimate shifts on surface water availability, have been added to the introduction and discussion as suggested.

Line-by-line comments

These are in addition to and generally do not duplicate those made above, e.g., I don't identify each instance of a reference to the original dissertation here; the authors can find those with global find and replace.

Line 30. Suggest also citing use of the Surface Water Supply Index (SWSI) in other states like Idaho.

- A reference to the SWSI in Idaho and Colorado has been included in the text.

Lines 55-60. Provide quantities of agricultural and domestic water use relative to supply, e.g., "annual withdrawal of water for irrigation is XX Mm³ relative to a total annual basin supply of YY Mm³."

- The information has been added as suggested.

Figure 1 caption. Change "low-to-medium storage" to "medium-to-low storage" for consistency with Table 1.

- The caption text has been revised as suggested.

Line 177: Define CDEC

- CDEC has been defined the first time it is used in the text.

Line 188: Define CIMIS. If defined here, you can use the acronym only in Figure 3 and its caption.

- CIMIS has been defined the first time it is used in the text.

Response to Referee # 2

Figure 4. Nice figure! The reference line for Qspill is referred to in a legend but is not shown in panel A. Add the reference line.

- The reference line has been added.

Line 242: See comment above about moving this information to the introduction and expanding the climate discussion a little. Hence, I suggest “is coincident with” rather than “corresponds to”.

- The three time periods have been introduced in the introduction as suggested. Brief context regarding human management and climate factors has been added.
- The recommended word change has been implemented.

Line 262: change “or or near the valley floor” to “on or near...”.

- The text has been revised as suggested.

Line 278: R values of -0.11 to -0.24 provide little to no evidence for a lagged effect, not “moderate” evidence as the authors suggest. These R values are equivalent to model R^2 values of 0.01-0.06, which nobody would consider useful in a predictive capacity.

- The text has been revised to state that a lagged correlation with Pspill is not supported by existing data.

Figure 8. The text indicates that there are 74 wells in the groundwater basin. This map shows far fewer, presumably because many are so close to on another the symbols overlap. If this is the case, either explain that in the caption or make the symbols smaller.

- The figure caption has been revised as suggested.

Line 310: Provide context for these RMSE values. How large is the error relative to the typical (mean) observation?

- Context has been added as suggested.

Lines 315-318. Standard residual plots, including leverage plots, would be much more useful than the existing figures used to illustrate model diagnostics.

- These standard plots have been added to the Appendix.