TETRA TECH

Memorandum

To:	Steve Klein, EPA ORD
Cc:	Jon Butcher, Tetra Tech
From:	Hope Herron, Tetra Tech
Date:	Thursday, May 08, 2014
Subject:	Comment Reconciliation Memorandum, Peer Review Comments from Guillaume Mauger , Heather Golden, and Dan Isaak

Peer Review Comments and Reconciliation

The *Quantitative Assessment of Temperature Sensitivity of the South Fork Nooksack River under Future Climates using QUAL2Kw* (Quantitative Assessment) was distributed by EPA ORD for external review and comment by: Dr. Heather Golden, EPA, Ecological Exposure Research Division, NERL/ORD; Dr. Dan Isaak, U.S. Forest Service; and Dr. Guillaume Mauger, Climate Impacts Group, University of Washington. This memorandum lists all comments received by Dr. Golden, Dr. Isaac, and Dr. Mauger below. The comment reconciliation process includes identification of the responsible EPA and/or Tetra Tech author who will address the comment, whether and how the comment will be addressed (e.g., action).

The Quantitative Assessment will be included as a companion technical methods report in support of the regulatory Draft South Fork Nooksack River (SFNR), WA Temperature TMDL published by the Washington Department of Ecology in summer 2014. The Quantitative Assessment is one of the key research components in the *EPA Region 10 Climate Change and TMDL Pilot – Project Research Plan* (February 2013, EPA Publication EPA/600/R/13/028).

Peer Review Comments

Dr. Heather Golden Comments

1. ***Comment:*** Page 5: See comment 2. Could you say WDE or something else?
   - ***Response:*** Revised text. "Ecology" is the preferred shorthand of the Washington Department of Ecology. We agree that it is somewhat awkward, but that is the preferred usage. The text has been changed to explain the shorthand at first use.

2. *Comment:* Page 5: Ecology is a very odd citation. If that's correct, great, but if you can modify this it would help. Is there a lead author form Dept of Ecology?

- ***Response:*** No change. See response to Comment 1. The form of the citation is as recommended to us by the Washington Department of Ecology and has thus not been changed.

3. *Comment:* Page 5: From what data set are these estimated and is the vegetation status at time of the data set construction assumed to remain?

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- ***Response:*** Revised text. Sentence added to reflect how vegetation status data sets were compiled. The combined datasets yield the best possible estimate of existing vegetation aside from in-depth field work. The assumption that annual changes along these mostly forested stretches are minimal.

4. *Comment:* Page 6: What is the quasi- in respect to? Stream flow? It seems like a dynamic model, as referenced in: https://fortress.wa.gov/ecy/publications/publications/0503044.pdf.

- ***Response:*** Revised text. QUAL2Kw is not a fully dynamic model and does not produce continuous output time series; however, it does represent within-day (diel) variability in meteorological forcing and the heat budget. We have clarified this sentence to say that QUAL2Kw "assumes steady-state hydraulics but represents the diel heat budget and water quality kinetics."

5. *Comment:* Page 6: Now saying “Washington Ecology” rather than “Ecology”.

- ***Response:*** Revised text. Changed to "Ecology" for consistency.

6. *Comment:* Page 6: It would be good to reiterate here that the final parameterization and calibration values will be detailed in a separate document…unless that is incorrect. If it is, then that information is needed..

- ***Response:*** Revised text. Added disclaimer: "This section summarizes the development of the water temperature model for the SFNR TMDL. More exhaustive documentation will be provided in the forthcoming South Fork Nooksack River Temperature Total Maximum Daily Load, Water Quality Improvement Report, and Implementation Plan being developed by Ecology. The model parameters described below are believed to be in final form, but are subject to change until the TMDL is approved." The similar disclaimer was removed from Section 2.1.2.

7. *Comment:* Page 7: Are there citations for these reports?

- ***Response:*** Revised text. Soil Survey Report citations added in text and reference list.

8. *Comment:* Page 8: Really curious what these are. It’s understandable most of this info will be in the report referenced on lines 569-570, but providing teasers – such as the most sensitive parameters – would be helpful here.

- ***Response:*** Revised text. Text added to reflect the greatest sensitivity to the model of the tests run was to temperature boundary conditions (headwater, tributary, and groundwater inflow temperatures).

9. *Comment:* Page 10: It looks like you had to add some info on top of this figure. To avoid further distortions, please just clarify in the Figure caption which data are observed. Looks like all that don’t have “Modeled” appended to them in the legend?

- ***Response:*** Revised text. Following note added to figure caption: “Solid and dashed lines show model results; open and closed squares show observed temperature data.”

10. *Comment:* Page 10: Good info, linking back to comment 8.

- ***Response:*** Revised text. Sensitivity analysis results are now mentioned earlier.

11. *Comment:* Page 11: Are SNOTEL and Ecology stations referenced previously (missed that, if so) and/or are on a map somewhere? Need to introduce and reference both, if possible (I know SNOTEL is possible; Ecology is WDE?).

- ***Response:*** Revised text. Stations are named more accurately and introduced better. Map added to include flow and air temperature stations used for boundary condition development.

12. *Comment:* Page 11: Reference to other literature or reports that have done this? If not, perhaps reference PRISM Climate Group (http://www.prism.oregonstate.edu/), who use elevation in their regression equations to estimated precip.

- ***Response:*** No change. In this case we are simply interpolating between a high and low elevation station to adjust air temperature, not precipitation.

13. *Comment:* Page 12: Keep pointing this out, but that just sounds so odd. Ecology is a discipline, not an author and/or citation.

- ***Response:*** Please see response to comment 2.

14. *Comment:* Page 13: Somewhere, either in the table or text, if would be good to know the 7Q2 range (max, min) of stream temps under each scenario for each set of reaches. That would add three more columns to the table, which might be doable.

- ***Response:*** No change. This section is summarizing the TMDL, and the TMDL focuses on maximum stream temperatures. In addition, the TMDL runs are for specific, critical atmospheric conditions (e.g., 90th or 50th percentile of annual 7-day maximum air temperatures). Thus, the full range of stream temperatures at a given flow condition is not available. It is our opinion that including an estimate that would be the critical condition average of the minimum (which would essentially be the maximum estimate of the daily minimum averaged over space) would be more confusing than helpful at this point.

15. *Comment:* Page 33: So, this is stated as quasi steady state earlier. The “quasi” might make sense, if it relates to flow, but a full-on steady-state model doesn’t seem correct. Is it?

- ***Response:*** Please see response to comment 4.

16. *Comment:* Page 33: What model provides “exact” representations of anything? Suggest wording modification here.

- ***Response:*** Revised text. Modified: "While the VIC model predictions of the water balance differ from historic gage records for the SFNR, the fit is generally good for the critical late summer period."

17. *Comment:* Page 34: This is important for clarity because otherwise a reader might look for which segments/tributaries are flowing into one another – as stream segments might.

- ***Response:*** We appreciate the suggestion. Rather than altering the title, the following note has been added to the table: "Tributaries are un-nested and each discharges directly to the SFNR mainstem."

18. *Comment:* Page 35: Fix run-on sentence.

- ***Response:*** Revised text: “The same procedure was followed for each of the future climate scenarios and the ratio between future climate and current conditions for 7Q10 flows was calculated. Flow ratios varied by VIC grid cell (area-weighted if a tributary watershed was in two different VIC cells), and were applied to

the boundary inflow for each tributary. This process yields the climate-modified inflow estimate for the critical condition for each tributary catchment."

19. ***Comment:*** Page 37: Again, so not steady-state, right?

- ***Response:*** Please see response to Comment 4.

20. ***Comment:*** Page 40: Would it be possible to at least check if there is a pattern in the residuals to further recognize whether there is potential spatial structure in the residuals – or not? This would be done to further explain whether spatial covariance is potentially part of the unexplained variances in the OLS regression models.

- ***Response:*** No change. We agree that this would be a useful piece of information; however, the detailed discussion of these alternative approaches to predicting stream temperature has been deleted from the report at the suggestion of other reviewers.

21. ***Comment:*** Page 40: [Blank].

- ***Response:*** No response needed.

22. ***Comment:*** Page 51: Refine this sentence. This isn't true for all TMDLs.

- ***Response:*** Revised text. This should read "The SFNR temperature TMDL is based on a steady-state analysis…"

23. ***Comment:*** Page 51: Assumptions are imperative for modeling exercises, but this one is difficult to get on board with – particularly in the Pacific NW. Perhaps adding a sentence describing why this assumption was necessary and/or why it might not have a substantive effect on simulation results would help.

- ***Response:*** Revised text. The TMDL addresses critical conditions of reasonable maximum impact. The maximum impacts are expected to occur when solar radiation input is not limited by clouds. This does happen, at least occasionally, during late summer in the PNW.

24. ***Comment:*** Page 74: Suggested last paragraph be changed to "in sum, restoration of system potential vegetation on both the mainstem and tributaries of the SFNR has the potential to mitigate some of the impacts of climate change on water temperature, unt only until 2020. This finding highlights the importance of combining the implementation of system potential shading with other measures that provide cool water refugia during high air temperature events in order to improve the protection of cooler habitat beyond 2020." The techincal analysis does not show that there are good prospects (or bad prospects) of protecting the resource "throughtout the 21st century" iwth aidditional non-shade measures; that analysis was not done. While we can be certain that other measures will definitely help, i don't think we know if itiwill help enough. this last sentence suggests some level of measurable certainty for which there no analysis to support i.e. there is model model scenario that shows that the implementation of shade plus other habitat protection measures cools temperatures through the 21st century.

- ***Response:*** Revised text. Final paragraph altered to read: "In sum, restoration of system potential vegetation on both the mainstem and tributaries of the SFNR has the potential to mitigate some of the impacts of climate change on water temperature, but only through about 2020. This finding highlights the importance of combining the implementation of system potential shading with other measures that provide cool water refugia during high temperature events to protect the resource beyond 2020."

Dr. Daniel Isaak Comments

1. ***Comment:*** Page x: (lines 316, 331, & throughout document). Please clarify the historical baseline period used for reference whenever a future temperature increase is specified. I believe there were three different baseline periods used at various points in the document (1970-1999; "Current", and 1950-1999), which makes interpretations difficult at times.

- ***Response:*** Revised text. We have tried to clarify the language regarding the baseline period throughout the document. The baseline period for comparison of future to historic climate is 1970-1999 ("1980s"). The period 1950-99 is that used by CIG for the bias correction step of downscaling; it is not a baseline period for climate projections. We now use the word "current" only in the context of describing conditions that are analyzed in the TMDL modeling scenario, primarily to represent "current" vegetation and the analysis of 7Q10 and 7Q2 flows. For climate scenarios we use "historic", defined as the 1970-99 climate model scenario, as the baseline condition. For stream water temperature in the TMDL the "current" condition is based on the 90th percentile of air temperatures observed from 1995-2012. Future scenarios are created by adding the difference between future time slices (eg 2020s) and the historic 1970-1999 time slice in the VIC model output.

2. ***Comment:*** Page xi: Another restoration strategy could be reductions in the amount of water diverted out of the river channel if that use is common in the SFK Nooksack.

- ***Response:*** Comment noted. Agricultural diversions are a very small fraction of flow in the SFNR; therefore, this strategy has not been highlighted in this analysis. It may, however, be a part of the larger strategy addressed in the ESA recovery effort and companion Climate Change Qualitative Analysis.

3. ***Comment:*** Page 1: same comment as above.

- ***Response:*** Revised text. The cited source gives changes in ^{o}F. We have added changes in ^{o}C (in parentheses) for consistency.

4. ***Comment:*** Page 1: lines 392-403. This paragraph overstates the effect of stream flows on temperature, especially within a climate change context. Both studies below (which are sited in the report) examined the relative effects of air temperature and flow on stream temperature by decomposing the variance in historical time-series monitoring records. These records came from a diversity of streams (e.g., large/small, high/low elevation, coastal/inland) but revealed similar results. Over periods of time long enough to observe trends associated with climate change, increases in air temperature have accounted for 80%-90% of the changes in stream temperature; whereas changes in flow account for the remaining 10%-20%. This occurs because stream temperatures are generally more sensitive to air temperature variation than flow variation, and because long-term trends in air temperature are more pronounced than trends in flow. The former point regarding sensitivity is also supported in the description of relative sensitivities for the Qual2k model here given on lines 622-627.

Isaak. 2010. Ecological Applications, 20(5): 1350-1371.
Isaak. 2011. Climatic Change, DOI 10:1007/s10584-011-0326-z.

- ***Response:*** Minor change. The paragraph that is flagged addresses the direct effects of stream flow on temperature in passing in one sentence: "reductions in summer flows are likely to have a greater impact on stream temperature than the direct impact of increased air temperature (Cristea and Burges, 2010; Wu et al., 2012)." This indeed is the finding of the cited authors; for instance, Cristea and Burges say "Modeled increases in stream temperature due to global warming are determined primarily by the projected reductions in summer streamflows, and to a lesser extent by the increases in air temperature." Note that neither our document nor the cited documents are saying that stream temperature is more

sensitive to flow than to air temperature, just that the changes caused by reduction in low flows have a greater impact than projected air temperature changes. We agree that the historical time series studied by Isaak show a stronger effect from air temperature, but this is mostly because there have been more consistent trends in air temperature. Part of the debate is that Isaak's papers are focused on monthly average stream temperatures, whereas the TMDL analysis addresses daily maxima (as 7DADMax) and the maxima may be more sensitive to flow reductions. To help clarify things we have rewritten the sentence in question to say "large reductions in summer flows may have a greater impact on maximum water temperature in some PNW streams than the direct impact of projected increases in 21st century air temperature (Cristea and Burges, 2010; Wu et al., 2012)."

5. *Comment:* Page 10: line 615. RMSE for temperature model is very good but model fit was obtained using measurements taken in only one year (2007) and validated against only one year (2010). What sorts of climatic conditions prevailed during these years (warm/cold; wet/dry)? Fitting the model to multiple years of data would be preferable for obtaining more robust parameter estimates associated with wider ranges of climatic conditions.

- ***Response:*** No change. This refers to the error statistics reported for the TMDL model during the calibration process (under a different work order). The applications are actually single critical-day results for days in 2007 and 2010 and may not reflect the full performance of those models. The days in question were selected because they represented warm, dry critical conditions. The characteristics of the years containing these specific dates is not really relevant to what has been done thus far. We agree that fitting the model to longer periods would be desirable; however, QUAL2Kw assumes steady hydraulics and does not generate continuous time series; rather, applications to conditions present on any given day need to be developed separately. This is the standard protocol for Ecology temperature TMDL applications with QUAL2Kw; however, Ecology is considering additional model applications for the September time period when supplemental salmonid spawning and incubation criteria apply.

6. *Comment:* Page 17: Historical baseline conditions for CIG climate scenarios: Comparison of CIG scenarios for 2020s, 2040s, and 2080s is usually made against a baseline period of the 1980s (1970-1999). It was unclear whether this baseline was consistently used throughout the document or whether different baseline periods were used. Please clarify throughout the text. Discussion in subsequent paragraph (lines 835-837) seems to imply that comparisons are made relative to 1980s, but later the 1915-2006 period is more commonly used.

- ***Response:*** Revised text. Please see response to comment 1. The baseline period for climate comparisons is indeed the 1980s (1970-1999) and this has been clarified in the report.

7. *Comment:* Page 39-42: Approaches for developing regression models to predict boundary conditions. Much of this discussion might be eliminated or condensed into 1 paragraph at the start of section 4.2.5 on page 43.

- ***Response:*** Revised text. At the suggestion of several reviewers, these sections have been drastically shortened and combined. The focus is now on the final method.

8. *Comment:* Page 52-53: These projected changes in stream temperatures seem really large and inconsistent with regards to the rates of future warming. For example, temperatures are predicted to warm by 3.6C from Scenario 3 to Scenario 16 in the 2040s, but then warm only another 1C in the next 40 years in Scenario 22. Why does the rate of temperature increase slow down given that shade is held constant in these scenarios? It's also unclear what historical baseline period is used for comparison.

The observed historical rates of mean summer stream temperature warming for recent 30 year periods in the two studies cited above were about 0.22C/decade. More recent estimates of regional stream warming rates for the 44

year period from 1968-2011 put the rate closer to 0.1C/decade (D. Isaak, NorWeST project, unpublished analysis). Even if those historical rates were doubled in the future, it implies only a 1C – 2C warming by the 2040s, which is considerably smaller than the estimates given here. Granted, the QUAL2k estimates are for a short-term extreme like 7DADMax, which might be increasing at faster rates than mean temperatures, but it still seems like an overly aggressive prediction. Mantua's predictions in his 2010 paper for stream in this area were for 7DADMax & were generally in the 1C-2C range for A1B in 2040s and 3C – 4C range for 2080s.

But maybe much of these apparent discrepancies are due to the lack of clarity with regards to referenced baseline conditions? Because on page 60/61, in figures 5-8 & 5-9, the projected changes seem more sensible. In those figures, the baseline for comparison is listed as "current" conditions and perhaps that means 2010s. If so, it would be most useful to present results against this baseline throughout the report.

- ***Response:*** No change. A number of points are raised in this comment. In general, it must be remembered that the TMDL focuses on critical condition 7DADMax water temperature during periods of low flow and high air temperature. In addition, there is a built-in conservative assumption that the climate conditions for the TMDL calculations, which are based on the 90th percentile of air temperatures observed from 1995 through 2012, represent stationary conditions relative to recent predicted historic climate. Future climate response scenarios are built using the difference between future time slices and the CIG historic time slice for 1970-1999 centered at 1985, not 1995-2012. This difference is added to the TMDL critical conditions to create future impact scenarios. In essence, any systematic difference due to warming of the average underlying climate that occurred between 1985 and 2003 is treated as a margin of safety in the analysis.

  Regarding the progression from Scenario 3 to Scenario 16 to Scenario 22 (medium impact scenario with current change), it is indeed the case that a larger increase in maximum stream temperature occurs between now and 2040 than between 2040 and 2080. This is a characteristic of the CCSM3 model used for the medium impact scenario, which predicts that summer temperatures in the SFNR will increase by 3.3 °C by 2040, but only by 4.0 °C by 2080 (see Table 3-2).

  The comment cites work on historical rates of mean summer stream temperature warming of 0.22 °C per decade over the last 30 years and 0.1 °C per decade over the last 44 years. It is not appropriate to project these rates into the future if the air temperature response to increased greenhouse gases is accelerating, as is predicted by the climate models. Further, as noted in the comment, the changes in the critical condition addressed by the TMDL are likely to be more dramatic. The results of this study (see Table 4-5) suggest that water temperature is likely to increase by about 0.52 °C per 1 °C increase in air temperature at critical conditions if all other factors are held constant. As we have noted in section 5.2, higher ratios of water temperature change to air temperature change in this study are a result of decreases in the critical condition flow and changes in other meteorological impacts in addition to the air temperature change. Note that Mantua et al. (2010) predict future stream temperatures with a Mohseni-type logistic regression model that uses weekly air temperature as an independent variable that does not consider changes in flow.

  Figures 5-8 and 5-9 show the same data that appear in Table 5-1. Please see response to comment 1 for further clarifications regarding the terminology of current, historic, and baseline conditions.

Dr. Guillaume Mauger Comments

1. ***Comment:*** Page ix: Minor copy-editing note: only need one space after each period.
   - ***Response:*** The Tetra Tech style guide calls for two spaces after a period. Per the Quality Assurance Plan submitted for the Quantitative Assessment, the style guide will be followed for all deliverables.

2. *Comment:* Page i: This is somewhat misleading since the downscaled data does not include water temperatures.

- ***Response:*** Revised text: "These changes include several which are direct outputs from the VIC model such as air temperature and dew point temperature, as well as changes which are secondarily influenced by VIC model outputs such as headwater, tributary, and groundwater temperatures, as well as headwater and tributary flows."

3. *Comment:* Page x: same comment as above.

- ***Response:*** Revised text as noted in comment 2

4. *Comment:* Page x: Why not include specific numbers here, since you have them?

- ***Response:*** Revised text; Percentages have been included: "After the corrections made by the TMDL team to the model, 60-94% of the stream miles are expected to exceed the 7-day average temperature target under critical high temperature - low flow conditions in the 2080s."

5. *Comment:* Page 1: Should this be Mote and Salathé, 2010? (Climatic Change…).

- ***Response:*** Reference changed to (Mote and Salathé, 2010).

6. *Comment:* Page 1: I think the "transient" designation is confusing. Best to stick with "mixed rain and snow".

- ***Response:*** We feel that the "transient" term has been well-established in the literature and has been retained; however, we have provided some further explanation.

7. *Comment:* Page 1: Again: shouldn't this be the 2010 paper from Climatic Change? (Also Elsner et al.).

- ***Response:*** Revised text: Added reference (Mote and Salathé, 2010).

8. *Comment:* Page 1: Need to define these, as you did with snow and rain dominant, and explain why they have the greatest sensitivity (i.e., they're the closest to the snowline, so small changes in temperature substantially impact snow accumulation).

- ***Response:*** Revised text: We have further described transient systems and added the recommended explanation of why they are sensitive.

9. *Comment:* Page 1: 2010, Climatic Change?

- ***Response:*** Reference changed to (Mote and Salathé, 2010).

10. *Comment:* Page 11: This column is not described in the caption.

- ***Response:*** Revised text: Table caption is altered to "Calculated 7Q2 and 7Q10 Flows Compared to Calibration Conditions".

11. *Comment:* Page 11: This is not a station name, but a data product that could be general to many different stations.

- ***Response:*** Revised text and referenced name of station and source more clearly.

12. *Comment:* Page 12: Probably not an important detail, but nighttime temperatures are probably warmer in response to mature riparian vegetation.

- ***Response:*** Revised text. Expanded on microclimate complexity and cited Snoqualmie TMDL for why microclimate effect was chosen.

13. *Comment:* Page 15: 2009 is the white paper, 2010 is the peer-reviewed journal article in Climatic Change. Content is the same for both.

- ***Response:*** Revised text. New reference reflects proper citation of (Miles et al., 2010).

14. *Comment:* Page 15: You might want to note somewhere that the new AR5 / CMIP5 models perform very similarly to the AR4 projections you are using: i.e., they have the same climate sensitivity. Differences in projections in AR5 are primarily due to differences in greenhouse gas scenarios.

I don't know where in the text you would include this – sometime later, presumably – but it would also be worth noting that there are currently no downscaled hydrologic projections for the PNW using AR5, at least not at the resolution and specificity used here.

- ***Response:*** Revised text. The following was added to the end of Section 3.1.1: "Note: Recently, the IPCC began releasing updated GCM results for the Fifth Assessment Report (AR5). The climate models perform similarly to the AR4 projections for the PNW and have the same climate sensitivities. Differences in projected results are primarily due to differences in greenhouse gas emission scenarios. Work funded by the Northwest Climate Science Center is ongoing to develop downscaled hydrologic projections for the PNW using AR5, but results are not yet available at fine resolution comparable to the AR4 results."

15. *Comment:* Page16: Should this go in the section below, where you go into more detail on the nuts and bolts of downscaling?

- ***Response:*** The section in question describes what was done by CIG, so I think the current location is appropriate.

16. *Comment:* Page 16: See: http://www.hydro.washington.edu/Lettenmaier/Models/VIC/Documentation/References.shtml.

- ***Response:*** Revised text. Liang et al. and Gao et al. references added to VIC model citation.

17. *Comment:* Page 17: Note that 30 years is not enough to fully sample PDO cycles or really any decadal or longer-term variability… It's really a minimum that probably does well at removing inter-annual variability, and is chosen as a compromise between the need for some specificity in timing (e.g., 21st century average is probably not useful), and the need to average out random fluctuations.

- ***Response:*** Revised text. We agree that a 30-year window is not enough to fully sample decadal-scale variability. However, the text here is explaining the use of the 92-year historical series (modified to represent the statistics of a particular 30-year time slice). The reference to 30 years in the footnote was misleading and has been removed.

18. *Comment:* Page 20: Should these be defined in a footnote or elsewhere?

- ***Response:*** Revised text. Descriptive footnote added.

19. *Comment:* Page 21: Maybe add labels below each GCM name, such as “low impact” “medium impact” and “high impact”.

- ***Response:*** No change. As the low, medium, and high models are identified in the table immediately preceding this figure we do not think it is necessary to add the additional labels.

20. *Comment:* Page 22: Are these curves smoothed? They should only show one value per month. If they are smoothed, this should be stated somewhere in the text.

- ***Response:*** Revised figure and text. The monthly points are connected with smoothed lines to improve legibility. This has been noted in the text.

21. *Comment:* Page 25: Note that VIC estimates of RH are based on an empirical relationship with daily temperature and the difference between min and max temperatures… which might be worth noting, since (as with statistical downscaling) empirical relationships can omit important mechanisms of change.

- ***Response:*** Revised text. We thank the commenter for this explanation as the CIG documentation is not clear on this point. We have added caveats to the text. Further, because dew point temperature is not directly generated by VIC, we have eliminated the dew point graphs in this section.

22. *Comment:* Page 25: The dew point increase is primarily a response to warming (since humidity is more or less constant).

- ***Response:*** Agreed. No change required.

23. *Comment:* Page 26: Is this true? Relative humidity is the same. VPD only increases because the saturation vapor pressure increases sharply with temperature. I know there is a relationship between water stress and VPD in trees, but am not certain this holds for water bodies.

- ***Response:*** Revised text. The Penman pan evaporation formula shows that pan evaporation from a free water surface increases unimodally as a function of vapor pressure deficit, all other factors being held equal. Therefore we think this conjecture is reasonable; however, we have revised "this will" to "this may".

24. *Comment:* Page 27: As with humidity, this is based on an empirical relationship with temp and the diurnal range in temperature – could be missing something such as changes in cloudiness/fog.

- ***Response:*** Comment noted, no change made.

25. *Comment:* Page 28: Note: RH, VPD, dew point, Solar Rad are all estimated by VIC based on daily tmin/tmax/precip/wind. Might be misleading to imply that they are products of the downscaling.

- ***Response:*** Revised text. Agreed; we have accepted the proposed changes in the text. See also responses to Comments 13-16 .

26. *Comment:* Page 28: For high elevations only, or for the SFNR as a whole?

- ***Response:*** Revised text. Text should read "higher elevations in the SFNR watershed."

27. *Comment:* Page 28: Nash-Sutcliffe is an error metric that emphasizes absolute biases. As you note below, relative variations in VIC appear to nonetheless be decent, even if the absolute values are biased. I would

complement these numbers with correlations, which will better get at the important question: does VIC accurately represent the sensitivity to warming?

You could easily calculate this by comparing observed and modeled time series of, say, 7-day minimum flows, or July-Aug-Sept runoff.

- ***Response:*** Revised text. The results discussed here are those of Wu et al. We have added their correlation results, which are for daily flows. They do not report correlation coefficients for 7-day minimum flows or July-Sept. runoff separately.

28. ***Comment:*** Page 29: Show numbers for july-aug runoff instead, since that's more relevant?

- ***Response:*** Revised text. A note regarding warm season (April-Sept.) runoff projections has been added. The cited reports do not break out July-August runoff separately.

29. ***Comment:*** Page 29: This could be tested to some extent by looking at the relationship between annual average (or winter average) temperatures and summer runoff. You could look at the relationship obtained from observations, and compare that to the relationship simulated by VIC. For instance, VIC may correlate highly with the observations but underestimate the sensitivity, or slope of the line relating temp and runoff.

- ***Response:*** Comment noted. This is an interesting suggestion, but is beyond the scope of our effort. Our intention is to look at the projected impacts on the TMDL conditional on the VIC projections of future hydrologic conditions. In doing so we note some key issues relative to interpretation of VIC, but are not doing intensive re-evaluation of VIC's ability to predict responses of SFNR hydrology to significant changes in future climate.

30. ***Comment:*** Page 31: Note that the geology of the SFNR may affect baseflow simulations, especially in the upper reaches of the watershed. Are there important upper elevation areas with porous volcanic soil? If so, then VIC may underestimate the importance of baseflow. See, e.g., Wenger et al., 2010 – they evaluate VIC low flow simulations across the PNW.

- ***Response:*** Comment noted. We are not aware of extensive areas porous volcanic soils in the upper part of the SFNR watershed. The area does have serpentine.

31. ***Comment:*** Page 35: Climatic change, 2010?

- ***Response:*** Revised text. Citation corrected.

32. ***Comment:*** Page 39: Is this true? CMIP3 GCMs run from 1950-2099, in most cases.

- ***Response:*** Revised text. There are certainly outputs of GCMs for this period, but the CIG historic analyses run only through 2006. The paragraph that contained this sentence has been deleted for other reasons.

33. ***Comment:*** Page 47: It would be better to calculate dew point using VPD and Temperature – RH is very non-linear and can lead to errors.

- ***Response:*** Revised text. We believe the comment is correct; however, our intention here was to replicate the procedure used in the TMDL analysis, which calculated dew point from observed RH. A note on this point has been added to the text.