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RESOURCES**


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## MEMORANDUM

March 12, 2026

**TO:** Cooperative Monitoring, Evaluation, and Research Committee (CMER)

**FROM:**  Lori Clark, Adaptive Management Program Administrator (AMPA)  
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**SUBJECT:** Effectiveness of Experimental Riparian Buffers on Perennial Non-fish-bearing Streams on Competent Lithologies in Western Washington – Phase 3 (Fifteen Years after Harvest) Final Report Dispute Arbitration Panel – Final Decision Memo

### Background

The Effectiveness of Experimental Riparian Buffers on Perennial Non-fish-bearing Streams on Competent Lithologies in Western Washington – Phase 3 (Fifteen Years after Harvest) Final Report (hereafter referred to as the Type N Hard Rock Study Phase 3 report) CMER Guided Decision-Making Process (Dispute Resolution) was initiated on Feb 25, 2025. The Guided Decision-Making Process was followed as outlined in Board Manual, Section 22 (5.3) and further articulated in CMER's Protocols and Standards Manual (PSM)(3.3.3). As a part of Stage 1, an informal meeting was held on March 14<sup>th</sup>, 2025, and on April 17<sup>th</sup> it was determined that it was not possible to achieve a resolution with Stage 1. Continued disagreements warranted elevation to Stage 2, and the disputing parties were invited to provide position papers to articulate continued concerns.

To characterize the dispute and determine the appropriate pathway(s) to resolve the dispute in Stage 2 (CMER PSM, Section 3.3.4), the AMPA and Ash Roorbach, CMER Co-Chair, applied the guidance from the PSM to the 4 position papers submitted by Chris Mendoza, Welles Bretherton, Doug Martin (with Harry Bell, Mark Meleason and Joe Murray (SAG member)), and the study authors regarding the Type N Hardrock Phase 3 Final Report. Doug et al. Dispute Issue 1, concerns 1, 2, 3, 4, 5 and 7, and Dispute Issue 2 met the criteria for a process dispute. Disputants previously approved BACI methods and Phase II conclusions and allowing re-litigation of settled methods undermines the AMP framework. The objections from the disputants represent an attempt to revisit settled methodological decisions, which do not comply with the principles outlined in the CMER PSM, therefore these concerns over these process issues were deemed resolved.

Two concerns were noted as process issues that may have technical aspects as articulated in the Doug et al. position papers: Issue 1, concerns 6 and 8, since which were attributed to differences in scientific interpretation, methodology, and analysis. The dispute was resolved by sending the Type N Hard Rock Phase 3 Report to ISPR with a majority vote to allow for an independent, qualified subject matter expert panel to respond to the CMER standard eight questions providing responses that would address the disputants' concerns. The AMPA also committed to reopening Stage 2 of Dispute Resolution following ISPR approval should the 2 outstanding issues not be addressed adequately during the ISPR process to reach consensus on the report.

CMER did not approve the ISPR Type Np Hard Rock Phase 3 report at their October 28<sup>th</sup> meeting. The disputing parties did not approve the final report with the justification noted that that their technical concerns were not adequately addressed with the ISPR process. The AMPA is responsible for setting up an arbitration panel utilizing the University of Washington on-call arbitration contract for all CMER disputes that involve technical issues. The arbitration panel was given all position papers but were told that the arbitration process will focus only on the AMPA and CMER Co-Chair characterized issues that have technical aspects that remain unresolved (Doug et al. position paper):

**#6** An underlying assumption of the BACI method is that the difference between the reference and treatment observed in the pre-treatment period will persist in the post-treatment period. Also, the difference observed is directly attributed to the treatment effect over the duration of the post-treatment period at the exclusion of other factors that could cause such a change (Manly 2009). Therefore, the initial response at P1 is the most closely associated with the treatment and the response in P3 is more likely to have a treatment effect confounded by other factors (Johnson 2022).

**#8** Phase 3 assumes, based on BACI study design, that the observed changes in amphibian densities are only responding to just the treatment effect.

The concern here is that changes observed in amphibian densities may be responding to more than just the “treatment effect”. In a “true experiment”, subjects are randomly assigned to groups, which are assumed to be equivalent to one another. The difference between the treatment and control groups is the “treatment effect”, which is a direct result of the experimental manipulation and allows a statistical conclusion of cause-and-effect (Sheskin 2011). The issue with the BACI design in ecological studies is that it lacks the controlled environment in “true experiments”. That is, the observed impact may be a “treatment effect”, or it may be confounded by other causes (Manly 2009). It is important in a BACI study to provide a convincing argument (e.g., weight-of-evidence approach) to address the “assumption that nothing else could cause a change of this size” (Manly 2009). To be clear, a confounding variable “is related both to group membership and to the outcome. Its presence makes it hard to establish the outcome as being a direct consequence of group membership (Ramsey and Schafer 2013)”. The underlying mechanisms are the treatment effect and the confounding variables. Given the defined scope of this study (e.g., treatment effect only), the focus is on the patterns of change in mean densities (Table 1), and the overall pattern of this study clearly shows a mixed result across the four taxa and three measurement periods.

## **Dispute Resolution – Arbitration Process**

The arbitration panel was assembled as outlined in the PSM. The Managing Editor requested an extension for the completion of the arbitration outcome due to the holidays and difficulty securing arbiters. The original deadline for completion of the arbitration decision was January 20<sup>th</sup>, 2026, and the revised timeline was February 17<sup>th</sup>. The arbitration team was finalized on January 20<sup>th</sup>, and their initial decision memo was provided to CMER on February 23<sup>rd</sup>. This memo transmits the panel’s initial response to CMER for the committee’s records. A meeting was held between the disputing parties and the arbitration panel on March 3<sup>rd</sup> to ask questions or request clarifications regarding the initial decision memo. The arbitration panel was given until March 12<sup>th</sup> to submit the final decision memo. The AMPA received the arbitration panel’s final decision memo on March 9<sup>th</sup>. The AMPA forwarded the decision by email to disputing parties and to CMER members on March 11<sup>th</sup>. The final decision will also be included in CMER’s March meeting mailing.

Per the PSM (3.3.4.2) the panel’s final decision is binding and ends the dispute over the Type N Hard Rock Study Phase 3 Final Report. The AMPA will forward the final decision to TFW Policy and the Forest Practices Board to inform them of the outcome of this dispute and begin the TFW Policy’s final report review and approval process. Please reach out to the AMPA if you have any questions or need more information.

Attachments:

- Final Decision Memo of the Arbitration Panel

06 March 2026

## **Arbitration of Type Np Hardrock Phase III Dispute**

Brian Gerber, Max Lambert\*, and John Stednick

\*Arbitration lead, authors listed alphabetically

The arbitrators independently reviewed all dispute documents and the Phase 3 report in question. We then met to discuss our independent reviews and unanimously agreed on the conclusions detailed below. Our original document was lightly edited for clarity after meeting with CMER on 3 March 2026.

We focus on two key points highlighted by the disputers' Position Paper (Martin et al., 2025). It was not our mandate to review the quality of the science undergirding the debate. Even so, all research studies have limitations. Such limitations can influence how a study is arbitrated. Indeed, all three arbitrators identified multiple study limitations and difficulties with the report that would influence the study's implementation, interpretation, and conclusions. For instance, the stringent site selection criteria produced few potential sites with which to actually conduct the research. As such, it is unclear how representative the study is to forested streams in the region, or whether this study is only applicable to a relatively narrow set of environmental conditions. In conjunction with non-randomized treatment assignments, such study design decisions may have contributed to the seemingly scattered array of results produced by the experiment. Despite such limitations, CMER has repeatedly agreed on the study's design from the onset and through subsequent phases. Ongoing interpretation of the study and its use for decision-making is contingent on decision-makers' agreement on past decisions around the intrinsic properties of the research including any study limitations.

In general, the study authors' use and interpretation of their study design is valid, although their broad conclusions are perhaps overly simplistic. We do note, however, that we found some other issues raised by Martin et al. have merit, albeit with qualification. *Given our prompt was specific to the two issues below, we did not address other issues raised by Martin et al.* Even so, we emphasize that it is difficult to arbitrate the below issues in isolation of the debate's broader context. Further, this document cannot be interpreted as the arbitrators' agreement with the conclusions from prior ISPR reviews nor all conclusions from the Phase III report in question. Our mandate was not to relitigate prior science reviews and CMER process. There may be issues with the Phase III report's conclusions that are unrelated to the two issues below, but these are beyond the scope of the arbitration.

### **Dispute Position Paper prepared by Douglas Martin, Mark Meleason, Harry Bell, and Joe Murray (May 5, 2025)**

We address two focal issues raised by Martin et al. The first references the before-after control-impact (BACI) study design and the second, somewhat related, argument centers on extraneous factors that may have influenced the results.

Issue 1.6—This issue refers to the assumptions of a BACI design, as employed in the focal study. Our arbitration finds that the study authors' use of the BACI design and results are appropriate. There is no reason for CMER to not continue accepting the study design and results as they are; how CMER uses these results for decision-making is a different issue. Under a null hypothesis, a BACI design assumes that any change in the reference sites pre-to-post impact would be mirrored by a similar change in the treatment sites. Any deviation from this would be attributable to treatment effects. However, demonstrating reference stationarity is essential in all BACI studies, but particularly in this study given the variability in responses and loss of control sites over time.

Any confounding effects on interpreting treatments effects would be a consequence of poor experimental design, site selection, and study establishment and implementation. If confounding factors were occurring, they would be the result of decisions that CMER approved years ago. For instance, these could be due to a systematic bias in intrinsic site conditions for reference and/or treatment sites. This may have occurred due to the small number of study sites, non-random treatment assignments, and stringent site selection criteria. However, if these biases do occur – which we cannot currently discern with the evidence available – CMER agreed (implicitly at least) that these were tolerable in the study design and implementation. As such, CMER has accepted the results of the study design. In general, the BACI design in the study is appropriate.

Issue 1.8– This issue refers to putative extraneous factors that influence response variables beyond the treatments. This problem is exactly why studies need adequate site selection and randomization. If CMER feels confident in site selection since the onset of the study, then CMER can continue feeling confident about the study's conclusions. If CMER is concerned about a systematic bias in the reference and/or treatment sites that was incidentally inserted into the study design, then ideally they would not have approved the study or its subsequent and ongoing phases. Given the evidence available, we have no reason to believe there is a systematic bias. The study design employed very stringent criteria on site selection; this could potentially make the study not generalizable to the region, particularly with non-random treatment assignment. That, however, does not necessarily mean the results are invalidated by an unmeasured factor. No evidence has been provided to suggest that extraneous factors are confounding the experimental design.

It is certainly possible that confounding issues manifest in either the treatment or reference sites but only after sufficient time passes post-treatment, particularly given the potential site selection issues noted above. However, neither the study authors nor Martin et al. provide evidence that some environmental conditions unexpectedly differed between treatment or reference sites as the study's duration proceeded. Without demonstrating some pattern, any discussion of confounding factors is mere speculation.

Further, even if there was a consistent region-wide decline across reference and treatment conditions in a focal species or life stage (i.e., larval tailed frogs), those declines were more pronounced under treatment conditions than reference conditions. This matches expectations from a BACI design and does not signal a confounding variable. Rather, this pattern indicates that some unmeasured factor(s) (e.g., climate) influenced all reference and treatment sites in the same direction and, additionally, treatment effects amplified those effects in the same direction. Regardless of the unmeasured factor, the study still identified an effect of treatments that were not seen in reference conditions.