Stream temperature change detection for state and private forests in the Oregon Coast Range

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[1] Oregon's forested coastal watersheds support important cold-water fisheries of salmon and steelhead (*Oncorhynchus* spp.) as well as forestry-dependent local economies. Riparian timber harvest restrictions in Oregon and elsewhere are designed to protect stream habitat characteristics while enabling upland timber harvest. We present an assessment of riparian leave tree rule effectiveness at protecting streams from temperature increases in the Oregon Coast Range. We evaluated temperature responses to timber harvest at 33 privately owned and state forest sites with Oregon's water quality temperature antidegradation standard, the Protecting Cold Water (PCW) criterion. At each site we evaluated stream temperature patterns before and after harvest upstream, within, and downstream of harvest units. We developed a method for detecting stream temperature change between years that adhered as closely as possible to Oregon's water quality rule language. The procedure provided an exceedance history across sites that allowed us to quantify background and treatment (timber harvest) PCW exceedance rates. For streams adjacent to harvested areas on privately owned lands, preharvest to postharvest year comparisons exhibited a 40% probability of exceedance. Sites managed according to the more stringent state forest riparian standards did not exhibit exceedance rates that differed from preharvest, control, or downstream rates (5%). These results will inform policy discussion regarding the sufficiency of Oregon's forest practices regulation at protecting stream temperature. The analysis process itself may assist other states and countries in developing and evaluating their forest management and water quality antidegradation regulations.

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1. Introduction

[2] Stream temperature is an important characteristic of water quality, as it affects aquatic system productivity, community composition, and species' developmental rates and fecundity [*Allan and Castillo*, 2007]. For states in the Pacific Northwest, stream temperature is frequently a water quality concern because of the effects of temperature change on salmonid development and survival [*Richter and Kolmes*, 2005]. Anthropogenic factors believed to affect stream temperature regimes in the Pacific Northwest include removal of streamside vegetation associated with timber harvest, agricultural land clearing, livestock grazing, and urban development [*U.S. Environmental Protection Agency*, 2003]. Several previous studies link timber harvest with increases in stream temperature [*Beschta and Taylor*, 1988; *Moore et al.*, 2005, and references therein], and federal endangered species listings of trout and salmon species (*Oncorhynchus* spp.) in the Pacific Northwest cite stream temperature increases due to logging as a limiting factor for population recovery [*Bryant and Lynch*, 1996; *Myers and Bryant*, 1998; *Myers et al.*, 1998].

[3] Stream temperature is a function of multiple energy transfer processes, including direct solar radiation, longwave radiation, conduction, convection, and evaporation. Of these factors, direct solar radiation is the primary contributor to daily maximum summer stream temperature and has the most direct response to forest harvest [*Brown and Krygier*, 1970; *Sinokrot and Stefan*, 1993; *Johnson*, 2004]. Therefore, maintaining shade may serve as an effective tool for minimizing stream temperature heat flux during the summer months when maximum stream temperatures are observed [*Johnson*, 2004]. Oregon, among other states, enacted timber harvest regulations (Oregon Forest Practices Act, or FPA) to maintain stream shade following timber harvest [*Oregon Department of Forestry* (*ODF*), 2007a]. Since removal of shade is strongly associated with stream temperature increases, timber harvest operations are considered in compliance with Oregon Department of Environmental Quality (DEQ) water quality standards if harvest operations comply with the FPA [*DEQ*, 2004]. However, ODF must periodically conduct studies to validate the efficacy of the FPA at meeting state water quality standards [*ODF*, 2007b].

[4] The DEQ developed water quality rules to comply with the U.S. Clean Water Act (U.S. Water Pollution Control Act Amendments of 1972, sections 101(a) and 303(c))

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regulations. The DEQ water quality rules include several stream temperature criteria. Among them is the Protecting Cold Water (PCW) criterion that represents a federally required antidegradation water quality rule component (Code of Federal Regulations, Title 40, section 131.12). The PCW applies to "cold" streams with temperatures below specific temperature thresholds [*DEQ*, 2004]. Anthropogenic activities are not permitted to increase stream temperature by more than 0.3°C above its ambient temperature. In addition, the cumulative amount of anthropogenic temperature increase allowed in streams with temperature total maximum daily loads (TMDLs) is 0.3°C for all sources combined [*DEQ*, 2004; *Sturdevant*, 2008].

[5] Oregon's temperature change criterion bears similarities to water quality rules and guidance in other states and countries. Oregon developed its water quality rules in close agreement with federal water quality guidance for the Pacific Northwest states of Alaska, Washington, Oregon, and Idaho [*U.S. Environmental Protection Agency*, 2003]. Alaska does not permit activities to increase weekly average temperatures by more than 1°C, nor does it allow changes to the amplitude or frequency of normal daily temperature cycles [*Alaska Department of Environmental Conservation,* 2006]. Idaho's water quality standards [*Idaho Department of Environmental Quality*, 2006] contain a variety of permissible temperature change thresholds, including no detectable change, 0.3°C, and 0.5°C. Washington's standards [*Washington Department of Ecology*, 2003] include a 0.3°C change threshold. States outside of the Pacific Northwest include temperature change criteria in their standards (e.g., Oklahoma's is 2.8°C [*Oklahoma Water Resources Board*, 2003]; Michigan's are 1.1°C, 1.7°C, or 2.8°C, depending on water body type [*Michigan Department of Environmental Quality*, 1986]). Guidance for Canada's Alberta province recommends against temperature increases of >3°C [*Alberta Environment*, 1999], while the Australia and New Zealand Conservation Council (ANZECC) guidance recommends a procedure for detecting a shift in temperature values relative to a percentile of temperatures recorded at a reference site [*ANZECC*, 2000].

[6] Detecting changes in stream temperature and attributing them to timber harvest can be difficult because of natural temporal and spatial variability inherent in these systems. Streams generally warm in a downstream direction. The rates of change and relationships between basin size and stream temperature patterns have been noted for larger streams [*Lewis et al.*, 1999; *Caissie*, 2006]. However, some studies note considerable variability in longitudinal stream temperature patterns in larger rivers [*Torgerson et al.*, 1999], smaller streams [*Dent et al.*, 2008; *Johnson*, 2004], or side channels [*Ebersole et al.*, 2003]. For smaller streams, longitudinal patterns may be highly variable in response to a variety of in-stream, microclimatic, and geologic processes [*Brosofske et al.*, 1997; *Hawkins et al.*, 1997; *Kasahara and Wondzell*, 2003]. Stream volumes change seasonally, potentially adjusting the contributing effects of hyporheic and surface flows. Groundwater inflows and outflows also influence stream temperatures [*Mellina et al.*, 2002; *Story et al.*, 2003]. Annual conditions and local hydrological changes can introduce, increase, or diminish surface and subsurface tributary inputs to specific streams, potentially decreasing shade [*Levno and Rothacher*, 1967; *Brown and Krygier*, 1970; *Murray et al.*, 2000] and increasing low streamflows, which would, in turn, influence several of the factors mentioned above [*Poole and Berman*, 2001; *Quinn and Wright-Stow*, 2008].

[7] In 2002 the Oregon Department of Forestry embarked on a manipulative study specifically designed to control for many of the above factors [*Dent et al.*, 2008]. The objectives of the study were to provide information on the effectiveness of riparian rules and strategies at meeting DEQ water quality standards and maintaining shade and large wood recruitment to streams and riparian areas. The study was also developed to quantify riparian area vegetation regeneration and allow an examination of linkages between regeneration and the resulting changes to shade and stream temperature. In this analysis we focus on addressing the effectiveness of riparian rules and strategies at meeting water quality standards, specifically the PCW. The study began with 36 sites (later reduced to 33 because of changes in landowner harvest plans) in Oregon's middle and northern Coast Range. Multiple preharvest and postharvest years of data collection allowed for the determination of within-site variability of stream temperatures across years. Control reaches permitted further evaluation of interannual temperature variability over the entire length of the study. We anticipated that the study's sample size would assist in overcoming a degree of intersite variability in temperature behavior. In this analysis, we compare stream temperatures before and after harvest to evaluate Oregon's antidegradation regulation for lands subject to timber harvest. The expected [*Boyd and Sturdevant*, 1997] mechanism for changing stream temperature following timber harvest was increased direct solar radiation. If riparian buffers adjacent to timber harvest provided insufficient shade, the streams would receive increased amounts of solar radiation, which would increase stream temperatures in excess of the PCW. We expected evidence of insolation to appear as a preharvest to postharvest increase in the temperature difference between the treatment reaches' upstream and downstream probes.

[8] Our primary study objective was to evaluate the effectiveness of private and state forest riparian rules and management strategies at meeting the state water quality stream temperature antidegradation standard in the Oregon Coast Range. A requisite secondary objective was to determine a means for assessing the regulatory criterion with empirical stream temperature data in an analysis that conformed as closely as possible to regulatory language. We constrained the analysis to consider only those site characteristics recognized by water quality and forestry rules and strategies (e.g., main channel water temperature, stream size, and land ownership). The principal results of this study are applicable to the policy issue at hand; the results may directly inform timber management decisions in Oregon and may apply to other timber-harvesting regions with antidegradation or cold-water standards. Our methods and results may assist the assessment and development of antidegradation standards in other states and countries.

2. Methods

2.1. Field Methods

[9] Stream temperature and riparian conditions were measured at 33 streams in the Oregon Coast Range from

2002 to 2008. Surrounding forests were approximately 50–70 years old and primarily managed for timber production [*Spies et al.*, 2002]. Treatment reaches of 16 out of 33 streams were oriented east–west (downstream azimuth between 45° and 135° or 225°–315°; J. D. Groom, unpublished data, 2010). An initial candidate pool of 130 streams was reduced to those that met study criteria. Study criteria included small or medium fish-bearing streams without beaver ponds or debris flows. Although 10 of the 33 sites used in the study were located along non-fish-bearing streams or streams for which fish usage was unknown, timber harvest operators treated all streams as fish-bearing and attempted to leave either minimum private [*ODF*, 2007a] or state (Northwest Oregon State Forest Management Plan (FMP) [*ODF*, 2001]) riparian buffers. Small streams are classified as having average annual flows ≤57 L/s, while medium streams have average annual flows >57 and ≤283 L/s [*ODF*, 2007b]. Even though temperature probes for these analyses were submerged, on a subset of streams they may have been downstream of reaches that some years exhibited spatially intermittent surface flow, a condition that is consistent with type F classification. We required a "control" reach immediately upstream of each harvest unit that would remain unharvested for the life of the study. We also required sites to provide at least 2 years of preharvest data collection. Preharvest and control reach water temperature data were incorporated into the study design to provide the temporal and spatial control necessary to separate treatment effects from site and year effects [*Dent et al.*, 2008]. Assuming selected sites were geographically representative (all available sites that met selection criteria were included in the study), inferential scope included private and state forest lands in Oregon's north and middle Coast Range.

[10] Two study reaches were established on all streams (Figure 1). The unharvested "control" reach was immediately upstream of the "treatment" reach. Treatment reaches were clear-cut or thinned no sooner than 2 years after the study began. Eighteen of the 33 streams also had a "downstream" reach that was immediately downstream of the treatment reach and was not harvested during the study (Figure 1). Control and downstream reaches were continuously forested to a perpendicular slope distance of at least 60 m from the stream's high water level. Reach lengths varied from 137 to 1829 m with means of 276, 684, and 288 m for the control, treatment, and downstream reaches, respectively. Information on reach bankfull and wetted width, gradient, composition, depth, and riparian basal area is given by *Dent et al.* [2008]. Factors affecting reach lengths included harvest unit boundaries in the treatment reach, large changes in valley or channel characteristics, or tributary inputs and junctions.

[11] Eighteen of the 33 sites were on privately owned lands, and the other 15 were on state-managed forest land. Riparian leave-tree buffer dimensions depend in part on fish presence and stream size. Both the FPA (private) and FMP (state) establish no-cut buffers immediately near streams with adjacent limited entry buffers (Table 1). While timber harvest in both state and private forests must comply with the FPA, implementation of the FMP on state sites results in additional leave trees and wider buffers along streams than on private sites managed according to the FPA. Treatment reaches were harvested according to the FPA or FMP and included 26 clear-cuts and 7 partial cuts. All private sites were clear-cut. Seventeen sites were harvested along one stream bank, of which 13 were state forest sites. The remaining 16 sites were harvested along both banks.

[12] Stream temperature data were collected hourly between 1 July and 15 September each year. Continuously recording temperature loggers (Optic Stowaway Temp and HOBO Water Temp Pro (both ±0.2°C accuracy), Onset Computer Corporation, Bourne, Massachusetts) were placed at three to four stations per stream that bracketed the upstream and downstream ends of each reach (Figure 1). The first and second stations (stations 1 and 2) were located at the upstream ends of the control and treatment reach, respectively. Station 3 was located at the downstream end of the treatment reach. Station 4, if present, was downstream of station 3 and represented the end of the downstream reach. We tested probe accuracy and placed probes according to the *Oregon Watershed Enhancement Board* [1999] protocol. Stream temperature probes were placed in shaded locations where streamflow was relatively constant, with reliable summer depth and a well-mixed water column. All temperature data were visually reviewed prior to study inclusion. If data ceased to reflect water temperature patterns from well-mixed water columns (e.g., because of channels going dry), the data were removed. Probe accuracy was checked prior to installation and in the field with National Institute for Standards and Technology thermometers.

[13] We designed the analysis of the temperature data to conform as closely as possible to the regulatory language

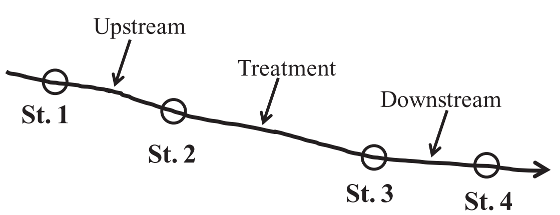

Figure 1. Study site configuration. The 33 sites have three temperature probe stations (St. 1 through St. 3), and 18 sites have a forth (St. 4). We define a reach as the portion of a stream between pairs of probes. Timber harvest occurred in the treatment reach; the upstream and downstream reaches remained unharvested for the duration of the study.

Table 1. Timber Harvest Riparian Buffer Widths Under FPA (Private Forests) Regulations and FMP (State Forest) Management Strategies[a]

Strategy	Stream Size	No Entry	Limited Entry	*n*
FPA	Small	0–6 m	7–15 m	4
FPA	Medium	0–6 m	7–21 m	14
FMP	Small	0–8 m	9–52 m	6
FMP	Medium	0–8 m	9–52 m	9

[a]Slope distances are measured perpendicular from the stream's high water mark. Stream size categories are described in text; no harvest is allowed within no entry distances; specified amounts of timber harvest are permissible within limited entry distance; *n* is the number of study streams per given category. FPA, Oregon Forest Practices Act; FMP, Northwest Oregon State Forest Management Plan.

of the PCW while meeting statistical requirements and accounting for intersite and intrasite variability. We developed the methodology in collaboration with DEQ staff members as we are aware of no formal guidance for PCW criterion evaluation or evaluation procedure for similar antidegradation regulations elsewhere. Our methodology involved two analyses. The first analysis determined whether or not specific years within reaches exceeded the PCW. The second analysis assessed the exceedances to determine whether they reflected a management-related pattern.

2.2. Analysis 1: PCW Exceedance Determination

[14] Analysis 1 identified exceedances using statistically defensible procedures chosen to adhere to PCW regulation. Specifically, we used generalized least squares regression to model "ambient" conditions while accounting for temporal autocorrelation. We examined prediction intervals rather than confidence intervals because rule language indicated that we needed to assess rule exceedance on a daily basis instead of a seasonal basis. We added 0.3°C to the upper predictive interval (PI) limit to incorporate the rule's temperature change threshold. A PCW exceedance occurred if any observed temperature fell above this upper limit.

[15] The structure and data of analysis 1 were influenced by the regulatory language of the PCW. The DEQ PCW requires evaluation of "seven-day-average maximum temperatures" [*DEQ*, 2004, p. 26]. We obtained these values by calculating a 7 day moving mean of daily maximum temperatures and refer to them as 7DAYMAX. The PCW's Air Temperature Exclusion (ATE) [*DEQ*, 2004] provision states that PCW compliance will not be evaluated for days when air temperature at or near a site is unusually warm. We interpreted the ATE to indicate that PCW compliance assessment should occur for every 7DAYMAX temperature. Although we assessed the PCW at every 7DAYMAX temperature, the ATE provision changed our results minimally (changed 2 out of 65 exceedances to nonexceedances).

[16] The PCW regulation [*DEQ*, 2004] considers assessment at the level of individual locations for point and nonpoint source pollution. We therefore structured our PCW assessment to examine reaches separately. Guidance [*Sturdevant*, 2008] recommends controlling for site spatial and temporal variability by assessing temperatures above and below as well as before and after impact. We therefore developed an analysis that modeled the downstream temperature relative to the upstream temperature for each of 2 years and examined changes in that relationship between years. The earlier year (year 1) represented required consideration of ambient conditions [*DEQ*, 2004], and the later year (year 2) represented the test year.

[17] We modeled pairs of years instead of all years within a reach for several related reasons. We obtained between 1 and 4 years of complete preharvest data per reach, which provided us with differing amounts of ambient information per site. The PCW language [*DEQ*, 2004, p. 2] defines ambient stream temperature as "the stream temperature measured at a specific time and place." *Sturdevant* [2008] provided a nonpoint source hypothetical example of testing the PCW that describes either using one preharvest and one postharvest year of data or gathering data postharvest onsite and at a nearby unharvested stream. On the basis of these sources we interpreted the use of only 1 year of preharvest information as mimicking a minimally acceptable timeframe to describe ambient conditions. Therefore, we were able to standardize our assessments by using 1 year of data to represent ambient conditions. Finally, our examination of two preharvest or postharvest years provided us with information on the utility of this analysis approach in situations where no disturbance, such as timber harvest, has taken place between the years in question. As a consequence of using only 2 years of reach data per comparison we expected the variability among ambient years to affect our ability to detect exceedances regardless of harvest effect.

[18] Exceedance rates for the different reaches and year-pair timings provide a variety of information. We obtain PCW exceedance information for control conditions by collectively examining results of preharvest year 1 to preharvest year 2 comparisons in all three reach types as well as all comparisons within the control reach. We anticipated that downstream reach comparisons would perform similarly to control conditions. Although water temperature may have become elevated at station 3 following harvest, we, like the DEQ [*Boyd and Sturdevant*, 1997; *DEQ*, 1995], expected the temperature change relationship between station 3 and 4 to remain stable. Preharvest year 1 to postharvest year 2 comparisons along the treatment reach provided information on the probability of PCW exceedance following timber harvest. Treatment reach postharvest year 1 to postharvest year 2 comparisons offered PCW exceedance information for sites already subject to harvesting.

[19] We used a regression approach to assess whether the relationship between a reach's upstream and downstream temperatures (Figure 1) changed between years. We treated a given reach's downstream and upstream probe 7DAYMAX temperatures, respectively, as dependent and independent variables (Figure 2). During year 1, the 7DAYMAX values at each probe describe stream

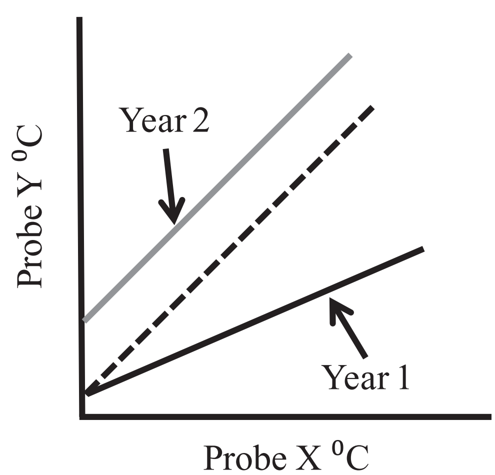

Figure 2. Hypothetical relationship between an upstream probe X and downstream probe Y. Year 1 temperatures are represented by a solid black line, and year 2 temperatures are represented by a gray line. A null model (dashed line) for comparing against year 2 data shares the same intercept as the year 1 relationship but assumes the slope of the year 2 relationship.

temperature under ambient conditions. During year 2, differences in the relationship between the two probes reflect a combination of background conditions and treatment effects on stream temperature. We modeled each year-pair (years 1 and 2) relationship using a generalized least squares regression (GLS; function gls in R (R Development Core Team, http://www.R-project.org) package nlme [*Pinheiro and Bates*, 2000]) as it can account for the substantial residual autocorrelation present in 7DAYMAX.

[20] Temperature relationships between probes were often nonlinear over the course of a season, potentially because of factors such as seasonal changes in solar radiation or streamflows [*Bren*, 1997; *Danehy et al.*, 2005]. For each year-pair comparison we considered 12 GLS models to capture seasonal patterns and autocorrelation behavior in the data. Seasonal patterns were captured in four main effects models, listed here in order of increasing complexity.

[21] Temperature relationship is constant over time (e.g., Figure 2):

$$y \sim \beta_0 + \beta_1 x + \beta_2 I + \beta_3 xI + \varepsilon_t,$$

[22] Temperature relationship changes at a constant rate over the course of the season:

$$y \sim \beta_0 + \beta_1 x + \beta_2 I + \beta_3 xI + \beta_4 \mathrm{day} + \beta_5 \mathrm{day} I + \varepsilon_t.$$

[23] Temperature relationship is parabolic as a function of time:

$$y \sim \beta_0 + \beta_1 x + \beta_2 I + \beta_3 xI + \beta_4 \mathrm{day} + \beta_5 \mathrm{day} I + \beta_6 \mathrm{day}^2 + \beta_7 \mathrm{day}^2 I + \varepsilon_t.$$

[24] Temperature relationship varies in a more complex pattern over time:

$$y \sim \beta_0 + \beta_1 x + \beta_2 I + \beta_3 xI + \beta_4 \mathrm{day} + \beta_5 \mathrm{day} I + \beta_6 \mathrm{day}^2 + \beta_7 \mathrm{day}^2 I + \beta_8 \mathrm{day}^3 + \beta_9 \mathrm{day}^3 I + \varepsilon_t.$$

[25] Here y and x represent the downstream and upstream probe 7DAYMAX temperatures, respectively, and I represents an indicator value (0 or 1) to differentiate year 1 from year 2, respectively. As portrayed in Figure 2, the indicator variable and its interactions allow us to obtain a regression line for each of the two years. The term day refers to the Julian date. The coefficients β_0 through β_9 are estimated by the regression procedure. The error term ε_t was modeled as one of three autoregressive moving average processes (ARMA):

$$\mathrm{AR}(1)\ \varepsilon_t = \phi_1 \varepsilon_{t-1} + a_t,$$

$$\mathrm{ARMA}(1,1)\ \varepsilon_t = \phi_1 \varepsilon_{t-1} + \theta_1\, a_{t-1} + a_t,$$

$$\mathrm{ARMA}(2,1)\ \varepsilon_t = \phi_1 \varepsilon_{t-1} + \phi_2 \varepsilon_{t-2} + \theta_1\, a_{t-1} + a_t,$$

where ϕ_1 and ϕ_2 are the autoregressive parameters at lags of 1 and 2, θ_1 is the moving average parameter at lag 1, and a_t is the homoscedastic (white) noise term centered at 0 [*Pinheiro and Bates*, 2000]. Our limited number of data points per year encouraged us to consider simple autocorrelation models. We selected among the resulting 12 models by visually comparing their residual autocorrelation, normal quantile-quantile, and standardized residual plots. We first selected a preferred ARMA parameterization for each main effects model by selecting the parameterization that reduced autocorrelation with the fewest parameters. We then similarly selected among the four ARMA-corrected main effects models. In no case did all 12 models fail to converge; however, in some cases selected models fit poorly (the 85th percentile of model residual standard errors was 0.331; the 95th percentile was 0.798).

[26] Once we selected a model that described the year-pair comparison, we constructed a null model against which to compare the individual year 2 7DAYMAX values. An increase in stream temperature between probes would result in an elevation of the intercept for the relationship between probes X and Y (Figure 2). Preliminary analyzes revealed that slope beta values from our control comparisons could change significantly ($p < 0.05$) between years under background (nonharvest) conditions. Since changes in slope values were not necessarily indicative of timber harvest effects, we decided to construct the null model from the intercept of year 1 and the slope values from year 2 (Figure 2) and assume that the harvest effect resided entirely in the intercept β_0. This approach allowed the null model slope to align with year 2, with the purpose of minimizing detected exceedances resulting from a change in slope. This assumption minimized our risk of registering false exceedances but increased our risk of failing to identify true exceedances.

[27] To assess 7DAYMAX temperatures on a daily basis and incorporate the PCW temperature increase threshold we generated prediction intervals (PI) around the null model and increased the upper PI endpoints by 0.3°C. Similar to DEQ guidance for evaluating different temperature criteria (biologically based numeric criteria [*DEQ*, 2004; *Sturdevant*, 2008]), we interpreted one or more year 2 7DAYMAX values above the upper PI + 0.3°C as indicating that year 2 exceeded the PCW. Figure 3 provides two examples of year-pair comparisons. We examined all possible year-pair comparisons for each reach in the 33 sites. We tallied year-pair exceedances and nonexceedances for use in analysis 2.

2.3. Analysis 2

[28] The second analysis involved examining the binary year-pair comparison results of analysis 1 (comparisons either exceeded or did not exceed the PCW) for potential explanatory patterns. We developed a priori explanatory hypotheses and expressed them as the set of 23 models described below. Independent variables included the reach (control, treatment, and downstream) and timing (preharvest to preharvest, preharvest to postharvest, and postharvest to postharvest) of each year-pair comparison. Combinations of these variables provided nine year-pair categories (categories). Other variables of interest were stream size (small and medium) and ownership (state and private).

[29] Our 23 specific models are derived from 6 general models (Table 2). The first general model considered the probability of year-pair exceedances as equal across all nine categories (null; no differences). The second general model (reach; control $\neq$ treatment $\neq$ downstream)

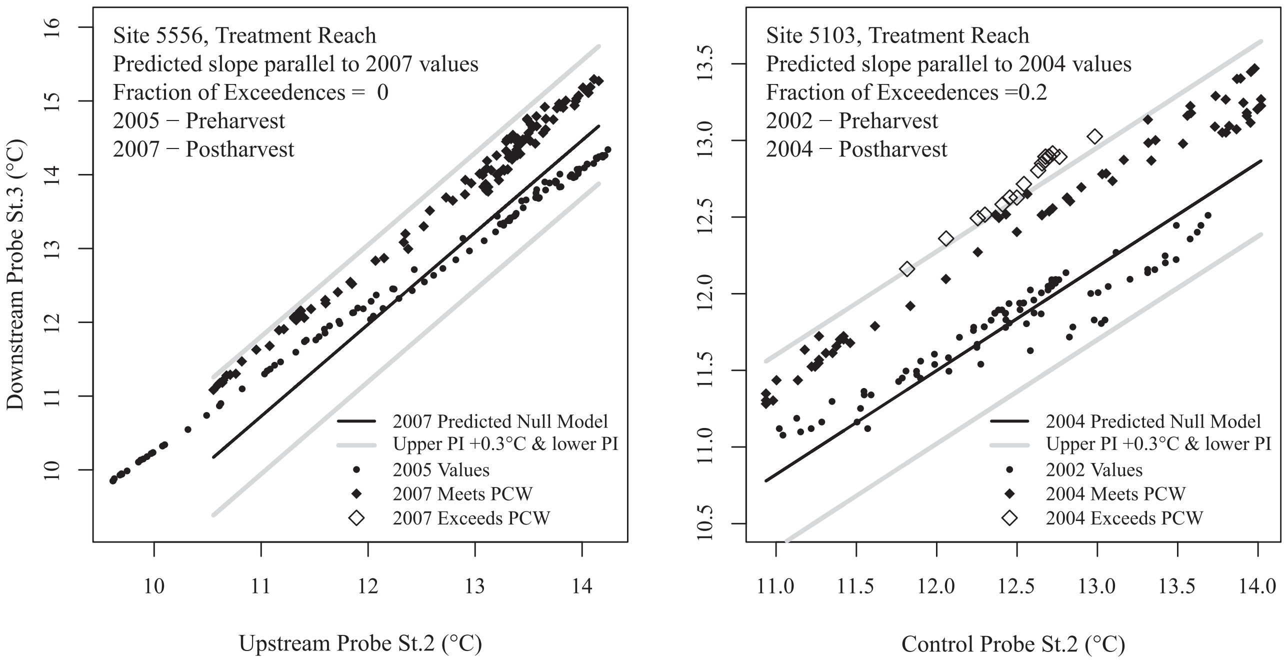

Figure 3. Two examples of year-pair PCW evaluations. Each example is a preharvest to postharvest comparison of a single site's treatment reach using 7DAYMAX stream temperature data from stations 2 (upstream) and 3 (downstream). Black lines represent null model predicted values for the postharvest data. The generalized least squares regression equation used to model both years' data is $y \sim \beta_0 + \beta_1 x + \beta_2 I + \beta_3 xI + \varepsilon_t$, with error term adjustments of ARMA (1,1) for site 5556 and ARMA (2,1) for site 5103 (see text for model and parameter explanations). The bottom and top gray lines represent the predicted null model's lower 95% prediction interval (PI) and an upper 95% PI +0.3°C, respectively. Solid diamonds represent year 2 values that fell below the upper 95% PI +0.3°C limit, and the larger open diamonds represent values above the limit (comparison for site 5103 represents a PCW exceedance).

allowed exceedance rates to differ among reaches. High-elevation, low-order streams predictably change downstream in flow, gradient, width, width/depth ratios, and their substrate [*Allan and Castillo*, 2007]. These characteristics among others potentially altered reach stream temperature. The third general model allowed differences among year-pair timing combinations (timing; preharvest to preharvest ≠ preharvest to postharvest ≠ postharvest to postharvest). Since data recording at all sites began during the preharvest years of 2002 and 2003, any detected exceedances could be attributed to climatic differences among years and would therefore be expected to appear regardless of reach type. In the fourth general model we considered the possibility that timing and reach effects led to differences among all categories (general model: all categories; all nine categories are unequal). The fifth general model (prepost treatment) considered the effects of timber harvest by allowing the preharvest to postharvest treatment reach category to differ from all other categories while treating those other categories as equal.

[30] For the second through fifth general models we considered four refinements (models labeled with b through e in Table 2). State and private forests have different riparian buffer width requirements that may have produced two levels of treatment effect. Model b included the parameterization of its base general model (2a, 3a, 4a, or 5a) but allowed the PCW exceedance rate for state site comparisons to differ from private site comparisons. Model c reflected an expected difference in PCW exceedance rates between small and medium streams. Private medium streams require more leave trees than small streams, potentially resulting in greater stream shading. Additionally, medium stream and channel morphology likely differed from small streams, and their larger flow volumes may have masked temperature gains because of differences in thermal mass relative to small streams [*Poole and Berman*, 2001]. Model d allowed all four combinations of stream size and ownership to be additive and different. Model e included an interaction effect between ownership and stream size, which allowed the size effect to differ for the two ownership categories.

[31] Given the difference in width between state riparian buffers and private buffers, we thought it possible that the wider state buffers would prevent stream warming while private buffers would not. If this were so, we would expect to observe elevated PCW exceedance rates for private treatment reach preharvest to postharvest comparisons, relative to all other comparisons. We named this indicator variable PPPT (private × preharvest versus postharvest × treatment reach). We assumed all other categories to be equal (general model: PPPT; Table 2). As the base general model (model 6a) already considers ownership, the only refinement available is to allow differences by stream size (model 6b).

Table 2. AIC Rankings of Logistic Mixed Effects Models Summarizing PCW Year-Pair Comparison Results[a]

Model	General Model	Additional Parameters	k	AIC	ΔAIC	ω
1	Null	N/A	2	410.48	45.49	0.000
2a	Subreach	N/A	4	392.3	27.31	0.000
2b	Subreach	Owner (S,P)	5	391.99	27	0.000
2c	Subreach	Size (M,S)	5	393.53	28.54	0.000
2d	Subreach	Owner and size	6	392.45	27.46	0.000
2e	Subreach	Owner × size	7	394.45	29.46	0.000
3a	Timing	N/A	4	404.77	39.78	0.000
3b	Timing	Owner (S,P)	5	404.9	39.91	0.000
3c	Timing	Size (M,S)	5	405.5	40.51	0.000
3d	Timing	Owner and size	6	404.72	39.73	0.000
3e	Timing	Owner × size	7	406.72	41.73	0.000
4a	All categories	N/A	6	386.33	21.34	0.000
4b	All categories	Owner (S, P)	7	386.21	21.22	0.000
4c	All categories	Size (M, S)	7	387.48	22.49	0.000
4d	All categories	Owner and size	8	386.58	21.59	0.000
4e	All categories	Owner × size	9	388.57	23.58	0.000
5a	pre-post treatment	N/A	3	372.21	7.22	0.013
5b	pre-post treatment	Owner (S, P)	4	372.06	7.07	0.014
5c	pre-post treatment	Size (M, S)	4	373.36	8.37	0.007
5d	pre-post treatment	Owner and size	5	372.42	7.43	0.012
5e	pre-post treatment	Owner × size	6	374.42	9.43	0.004
6a	PPPT	N/A	3	364.99	0	0.484
6b	PPPT	Size (M, S)	4	365.07	0.08	0.465

[a]See text for definitions of models and general models. Model parameterization includes general model parameters and any accompanying additional parameters; additional parameters included combinations of parameters to indicate land ownership (state (S) or private (P)) and stream size (medium (M) or small (S)); k is the number of estimable model parameters; ΔAIC is the difference between a model Akaike information criterion (AIC) value and the lowest overall AIC value; model weight, ω, is the relative probability that a given model is the best of the set. PPPT, private × preharvest versus postharvest × treatment reach; N/A, not applicable.

[32] We examined the patterns of year-pair exceedance rates for these 23 models with logistic mixed effects regression (function lmer in R package lme4). For each model we fit a random intercept by site to account for dependence among year-pair comparison results observed at the same site. We evaluated random effects variables for normality with the D'Agostino normality test [*Thode*, 2002] and compared relative model performance by examining model Akaike information criterion (AIC) values. We interpreted models with the lowest AIC values as best explaining model variance and two or more models with ΔAIC values (model AIC minus the lowest AIC value) <2 as having essentially equal explanatory power [*Burnham and Anderson*, 2002]. Empirical results from our a priori AIC analysis raised questions regarding impacts of timber harvest on state forestlands. We therefore conducted a post hoc analysis of state site year-pair comparison results with the same mixed effects logistic regression model formulations as described above, with the exception that all models including ownership (state and private) were omitted.

[33] Interpretation of AIC values and model weights (ω; the probability that a model is best given the data and set of models considered [*Burnham and Anderson*, 2002]) for linear mixed effect models is problematic when comparing models with different random effects structures [*Vaida and Blanchard*, 2005; *Greven and Kneib*, 2009]. Because we included random effects for site to account for dependence among observations at the same site, our empirical analysis maintained a constant random effects structure among models. However, the behavior of AIC for generalized linear (e.g., logistic) models has not been well explored [*Greven and Kneib*, 2009]. We therefore analyzed simulated data to ensure that our mixed effects logistic regression model selection inference was not biased. The main issues were to determine (1) if empirical model weights adequately reflected the frequency with which models were ranked as best and (2) the frequency by which our empirically selected top model (lowest AIC value) would be selected as best when data were generated from another model.

[34] To conduct the simulations, we generated data sets of PCW year-pair comparison exceedances similar in structure to the empirical data set used in the mixed effects logistic regression of analysis 2. Each simulated data set had the same number and type (timing and reach) of site year-pair comparisons. We used the β estimates from six of our empirical models (null, 2a, 3a, 4a, 5a, and 6a) along with randomly generated site random effects values to create groups of simulated data sets (null, 2A, 3A, 4A, 5A, and 6A). Each group contained 1000 simulated data sets. The random effects values for each data set's sites were generated using the original empirical model's random effects estimated parameter standard deviation value. We fit all 23 logistic mixed effects regression models to each simulated data set to determine AIC values and model weights (ω). We addressed simulation issues 1 and 2 by examining the distribution of simulation analysis AIC values and model weights.

3. Results

[35] Probe malfunctions and premature harvest schedules reduced the number of preharvest years from 2 to 1 at 12 of 84 reaches (7 of 33 sites). Riparian buffer widths averaged 40.4 m (95% confidence interval (CI) = 35.1 and 45.8 m, n = 33). Buffer widths were greater for state sites than private sites (private mean = 31.0 m (26.7, 35.3); state mean = 51.8 (45.6, 58.0)). A summary of stream temperature probe data is provided in Table 3.

[36] To evaluate performance against the PCW criterion, we performed 614 year-pair comparisons for the three reaches during the three timing categories of interest. We present the raw proportions of comparison exceedances (not taking into account site differences) by category in Figure 4. Each site had between 7 and 45 year-pair comparisons. Twenty-four of our sites exhibited at least one PCW year-pair exceedance. We observed PCW exceedances in all three reach types when comparing only preharvest years. Of the 614 comparisons, 65 (11%) exceeded the PCW.

[37] Table 2 presents results of mixed effects logistic regression model comparisons. The model with the lowest AIC value of our set was model 6a: private treatment reaches during preharvest/postharvest comparisons differ from all other comparisons (ΔAIC = 0; $\omega = 0.48$; observations = 614, groups (sites) = 33; random effects standard deviation (SD) = 1.176; fixed effects: $\beta_{\mathrm{intercept}} = -2.967$ (standard error (SE) = 0.293, $p < 0.001$), $\beta_{\mathrm{PPPT}} = 2.564$ (SE = 0.370, $p < 0.001$)). Model 6b, which is identical to 6a except small stream estimates are allowed to differ from medium stream estimates, received the second lowest yet similar AIC score (ΔAIC = 0.080; $\omega = 0.465$;

Table 3. Summary of Stream Temperature Probe Data[a]

	Station 1			Station 2			Station 3			Station 4		
Temperature Metric	$\overline{X}$	SD	n	$\overline{X}$	SD	n	$\overline{X}$	SD	n	$\overline{X}$	SD	n
7DAYMAX	11.98	1.74	13911	12.21	1.76	12881	12.64	1.74	13888	12.81	1.94	7932
Daily maximum	11.92	1.83	14787	12.14	1.85	13715	12.57	1.84	14752	12.74	2.03	8442
Daily average	11.39	1.69	14787	11.55	1.71	13715	11.85	1.62	14752	11.93	1.75	8442
Daily minimum	10.93	1.65	14787	11.04	1.66	13715	11.25	1.57	14752	11.26	1.64	8442
Daily flux	0.98	0.72	14787	1.10	0.68	13715	1.31	0.9	14752	1.48	0.98	8442

[a]Mean ($\overline{X}$), standard deviation (SD), and sample size (n) of 7DAYMAX, daily maximum, daily average, daily minimum, and daily flux (difference between daily maximum and daily minimum) temperatures at stations 1–4. Temperature data are expressed in Celsius and represent data pooled across 33 sites and all study years (2002–2008). Note that 7DAYMAX values are autocorrelated; standard deviation values should be viewed as summary statistics only and not as estimates of true standard deviation.

observations = 614, groups (sites) = 33; random effects SD = 1.131; fixed effects: $\beta_{\mathrm{intercept}} = -2.425$ (SE = 0.461, $p < 0.001$), $\beta_{\mathrm{PPPT}} = 2.589$ (SE = 0.373, $p < 0.001$), $\beta_{\mathrm{Size}} = -0.780$ (SE = 0.554, $p = 0.159$)). The stream size parameter in 6b improved the model fit over 6a by an amount (1.92) almost equivalent to the AIC parameter penalty (i.e., 2), suggesting that the additional parameter did not substantially improve model 6b over model 6a. The D'Agostino normality test failed to reject the hypothesis of normality of the estimated random effects for model 6a ($\chi^2 = 3.73$, $p = 0.155$) and model 6b ($\chi^2 = 2.30$, $p = 0.316$). Figure 5 presents point estimates of exceedance rates for models 6a and 6b with delta method [*Casella and Berger*, 2001] derived confidence intervals. Models 5a–5e received modest support (combined $\omega = 0.051$). The null model and models 2a–4e received virtually no support (combined $\omega = 0.001$).

[38] When considering the 15 state sites alone, we found that the post hoc model with the most support was model 2a: stream reaches differ from one another (ΔAIC = 0, $\omega = 0.256$; observations = 308, groups (sites) = 15; random effects SD = 1.718; fixed effects: $\beta_{\mathrm{intercept}} = -2.460$ (SE = 0.731, $p < 0.001$), $\beta_{\mathrm{UpstreamReach}} = -1.427$ (SE = 0.723, $p = 0.048$), $\beta_{\mathrm{TreatmentReach}} = -0.211$ (SE = 0.6291, $p = 0.738$); Table 4). Three other models shared similar AIC values (ΔAIC < 2), including post hoc model 5a: State treatment reaches during preharvest/postharvest comparisons differ from all other comparisons (ΔAIC = 1.129; $\omega = 0.146$; observations = 308, groups (sites) = 33; random effects SD = 1.537; fixed effects: $\beta_{\mathrm{intercept}} = -3.252$ (SE = 0.528, $p < 0.001$), $\beta_{\mathrm{PrePostTreatment}} = 0.8848$ (SE = 0.465, $p = 0.057$)). Post hoc model 5a structurally resembled a priori model 6a. Model 5a's PCW exceedance estimate for preharvest to postharvest treatment reach comparisons was 8.57% (95% CI = 2.96%, 22.37%), while the probably of exceedance under all other scenarios was 3.72% (1.35%, 9.82%).

[39] Simulation study results support empirical AIC weights (Table 5). When we examined data groups 6A and 6B (generated from models 6a and 6b, respectively), the null model and models 2a–4e were identified as having the lowest AIC value in less than 1% of simulation runs. Models 5a–5e cumulatively received the lowest AIC scores in 2.3% and 2.1% of the analyses involving groups 6A and 6B, respectively. Analyses for group 6A resulted in correctly selecting model 6a as best for 81.2% of group 6A data sets, while model 6b was selected as best in 15.9% of

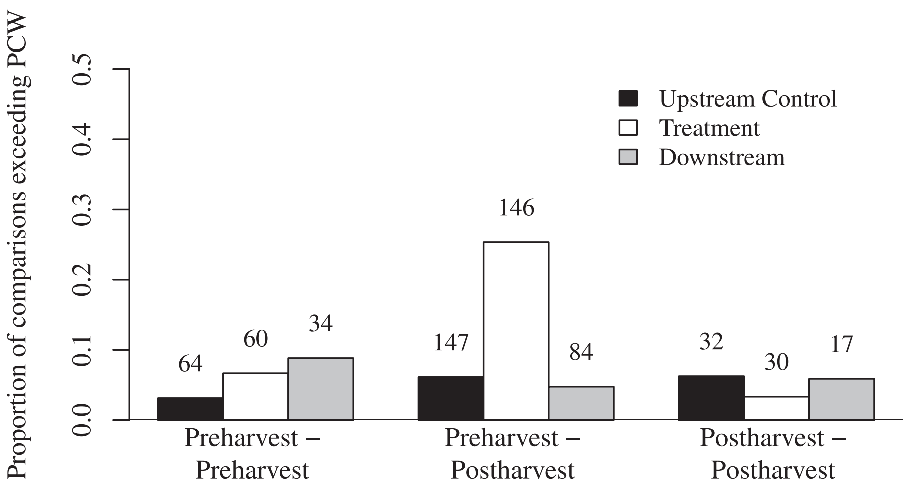

Figure 4. Raw proportion of year-pair comparison exceedances. Binomial data ($n = 614$) were used to estimate the raw proportion of year-pair comparisons that resulted in PCW exceedance by each analysis category. The nine categories are grouped by year-pair comparison timing (preharvest to preharvest, preharvest to postharvest, and postharvest to postharvest year-pair comparisons), and within each group are reach types (control, treatment, and downstream). Numbers above each category indicate the number of year-pair comparisons available for calculating individual proportions.

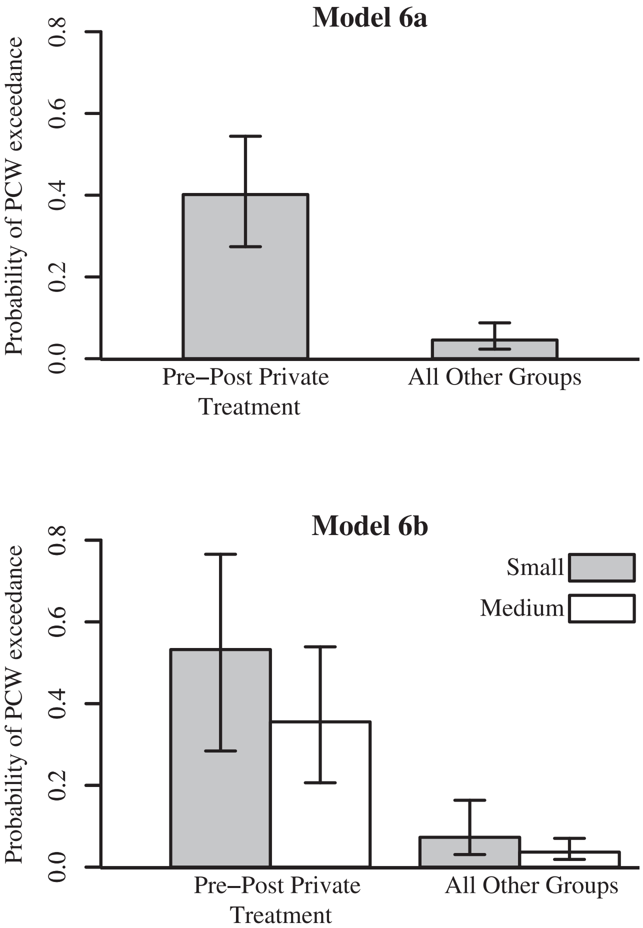

Figure 5. Point estimates for the two best supported mixed effects logistic regression models (±95% CI). Point estimates represent the probability of each comparison type exceeding the PCW. Model 6a allowed preharvest to postharvest year-pair comparisons for private treatment reaches (PPPT) to differ from all other comparison types combined (all other groups). Model 6b is similar in formulation to model 6a except that it additionally allowed small streams to differ from medium streams.

6A data sets. Model 6b was selected as best for 49.3% of group 6B data sets, while model 6a was incorrectly assessed as best in 48.3% of the 6B data sets.

[40] Models 6a and 6b were seldom selected as best when using data generated from the first five general models. Although the null model was correctly selected as the best model in 46.0% of null group data sets, other models were incorrectly selected with some frequency. In 57 of the 80 null group simulation runs where model 6a was selected as best, the null model performed similarly (ΔAIC < 2) In 15 of those 80 instances the AIC value for 6a was >2 from the null model and models 2a–5e, representing a 1.5% probability that model 6a would be selected as best from the null group and have a ΔAIC > 2 from these other models. The probability for model 6b was 1.1%.

4. Discussion

[41] Our primary objective was to evaluate the effectiveness of riparian leave-tree requirements at meeting Oregon's antidegradation Protecting Cold Water criterion, which shares similarities to standards in other states. Our analysis included preharvest and postharvest data from private and state forests in the Oregon Coast Range. Both state and private sites were required to meet the FPA riparian leave-tree standards around streams. State sites had additional harvest restrictions as required in the Northwest Oregon State FMP, which often resulted in wider buffers with more trees. While riparian leave-tree requirements differ, both private and state forests must comply with the Oregon Department of Environmental Quality's PCW criterion.

Table 4. Post Hoc AIC Rankings of Mixed Effects Logistic Regression Models Summarizing PCW Year-Pair Comparisons for State Forest Sites[a]

Model	General Model	Additional Parameters	k	AIC	ΔAIC	ω
1	Null	N/A	2	169.98	2.575	0.071
2a	Reaches differ	N/A	4	167.41	0	0.256
2c	Reaches differ	Size (M,S)	5	168.67	1.264	0.136
3a	Timing differs	N/A	4	171.06	3.653	0.041
3c	Timing differs	Size (M,S)	5	172.18	4.77	0.024
4a	Everything differs	N/A	6	168.34	0.932	0.161
4c	Everything differs	Size (M,S)	7	169.54	2.134	0.088
5a	pre-post treatment	N/A	3	168.54	1.129	0.146
5c	pre-post treatment	Size (M,S)	4	169.77	2.362	0.079

[a]AIC model comparison performed for state forest sites alone ($n = 15$). See text for definitions of models and general models; see Table 2 for definitions of the columns.

[42] Our analysis indicated that timber harvested according to minimum FPA standards along medium or small fish-bearing streams resulted in a 40.1% probability that a preharvest to postharvest comparison of 2 years of data will detect a temperature increase of >0.3°C. The probability of other varieties of comparisons producing a temperature increase was 4.6%. Harvest to state FMP standards resulted in an exceedance probability for treatment reaches preharvest to postharvest (8.6%) that did not statistically differ from all other comparisons (3.7%). The a priori and secondary post hoc multimodel comparisons did not indicate that timber harvest increased the probability of PCW exceedance at state sites.

[43] While these results indicate a probable harvest effect on private sites as measured with the PCW, Oregon's current forest practice regulations have likely reduced the impact of timber harvest on stream temperature increases as compared to historic practices. The 0.3°C change threshold lies 1 or 2 orders of magnitude lower than findings from studies conducted prior to enactment of riparian protection standards [*Levno and Rothacher* 1967; *Brown and Krygier*, 1970]. The testing of forest practices may currently involve controlled, replicated, long-term monitoring programs to detect a relatively small change among background variability. Historically, this level of investment may not have been necessary when riparian areas were entirely cleared, occasionally burned, and subject to increased probabilities of channel scour.

[44] We felt it inappropriate to report the magnitude of 7DAYMAX temperature exceedances because of the constrained and conservative nature of our analysis. Our analysis avoided committing type II errors in analysis 1 by using the slope value from year 2 (the test year) in constructing

Table 5. AIC Model Selection Behavior for Simulated Model Sets[a]

Tested Models		Empirical Results		Simulated Data Results (%)						
Model	*k*	AAIC	CO (%)	Null	2A	3A	4A	5A	6A	6B
Null	2	45.49	0.00	46.0	0.1	3.3	0.0	0.0	0.0	0.0
2a	4	27.31	0.00	4.5	52.0	0.2	2.3	0.0	0.1	0.0
2b	5	27	0.00	3.1	9.3	0.1	1.0	0.1	0.1	0.0
2c	5	28.54	0.00	1.2	10.5	0.0	0.6	0.0	0.0	0.0
2d	6	27.46	0.00	0.5	2.3	0.0	0.0	0.0	0.0	0.0
2e	7	29.46	0.00	1.6	6.2	0.0	0.4	0.0	0.0	0.1
3a	4	39.78	0.00	5.9	0.0	50.0	0.0	0.0	0.0	0.0
3b	5	39.91	0.00	1.8	0.0	11.3	0.0	0.0	0.0	0.0
3c	5	40.51	0.00	2.1	0.0	10.8	0.0	0.0	0.0	0.0
3d	6	39.73	0.00	0.7	0.0	2.9	0.0	0.0	0.0	0.0
3e	7	41.73	0.00	1.8	0.0	4.9	0.0	0.0	0.0	0.0
4a	6	21.34	0.00	0.6	9.2	6.3	46.6	0.9	0.1	0.0
4b	7	21.22	0.00	0.3	1.8	1.6	8.8	0.4	0.3	0.0
4c	7	22.49	0.00	0.1	1.8	1.1	10.0	0.3	0.0	0.0
4d	8	21.59	0.00	0.1	0.3	0.5	2.0	0.2	0.0	0.1
4e	9	23.58	0.00	0.1	0.8	0.9	4.7	0.2	0.0	0.1
5a	3	7.22	1.31	5.8	3.5	2.2	15.4	62.9	0.2	0.3
5b	4	7.07	1.41	5.5	0.3	0.3	2.6	12.9	1.4	0.5
5c	4	8.37	0.74	2.4	0.7	0.6	3.3	11.2	0.1	0.0
5d	5	7.43	1.18	1.1	0.2	0.0	0.6	3.5	0.2	0.7
5e	6	9.43	0.43	3.2	0.7	0.5	1.2	6.6	0.4	0.6
6a	3	0	48.41	8.0	0.2	1.9	0.4	0.6	81.2	48.3
6b	4	0.08	46.51	3.6	0.1	0.6	0.1	0.2	15.9	49.3

[a]Tested models and empirical results are presented and explained in Table 2. Simulated data results indicate the percent of simulated data sets that resulted in tested models receiving the most support (lowest AIC). 1000 sets were generated for each forced "true" model; NULL data sets were generated from the Null model, 2A data sets from model 2a, etc. See text for an explanation of model structure and simulation methodology.

the null (ambient) model. The decision to use the slope from year 2 instead of year 1 reflects risk aversion toward committing spurious regulatory "exceedance" assessments, a decision that might be reversed (null constructed from year 1's slope and intercept) by practitioners more concerned about failing to detect system change when present. If timber harvest generally increases the slope of the relationship between the upstream and downstream probe, our method would likely have underpredicted the true frequency of PCW exceedance and therefore produced underestimates of temperature change. Our additional use of upper prediction intervals inflated by 0.3°C to define our change threshold would reduce apparent change magnitude relative to a standard confidence interval. Finally, in certain instances our prediction intervals were further increased by the inclusion of truncated seasonal data sets (truncation due to probe exposure to air temperatures). As a consequence, those regression models exhibited large estimated variances. Model variability would likely improve in such situations if the analysis included more than 2 years of data.

[45] The strength of our analysis technique lies not with improving change detection methodologies but rather with informing practitioners who are similarly devising, examining, or revising regulatory change thresholds. Other states and countries have structured similar change thresholds; however, only *ANZECC* [2000] appears to provide guidance for testing and interpreting findings for these thresholds. Of Pacific Northwest states, only Idaho includes rule language similar to the ATE (Idaho Administrative Rules 58.01.02, 2007). We found that inclusion of the ATE altered our results slightly but substantially affected our method for testing the PCW. Washington, Alaska, and other municipalities could employ a version of our technique but test for seasonal exceedance on the basis of confidence intervals instead of daily exceedance via prediction intervals. The use of 7DAYMAX, or a 7 day moving average of average daily temperatures, is common Pacific Northwest regulatory data transformation. Although there is a biological rationale for this variety of transformation [*U.S. Environmental Protection Agency*, 2003], these autocorrelated data require statistical manipulation to properly adjust estimated variances. The GLS procedure itself is data hungry as it appropriately models the reduction in effective sample size due to autocorrelation. The 7DAYMAX values may be more appropriate for determining compliance with absolute temperature threshold regulations (e.g., *DEQ* [2004] biologically based numeric criteria) than with change thresholds; one mean for reducing the degree of autocorrelation among measurements could be to use nonaveraged daily temperatures coupled with a greater change threshold. Our regulatory analysis likely benefitted from the regulatory structure [*DEQ*, 2004; *ODF*, 2007b] that allowed us to examine a population of sites rather than relying on individual site assessments of rule compliance. Data collection across 33 sites and over several years of prehaverst and postharvest conditions allowed us to estimate treatment effects as well as a small (~ 5%) but present rate of exceedance under nonharvest conditions. Individual site assessments with limited preharvest and control information would likely produce a certain number of false-positive findings. In addition, the assurance of regulatory compliance offered to timber harvest operators who follow the FPA [*DEQ*, 2004] likely enhanced our ability to obtain access to sites and landowner cooperation.

[46] Our model selection approach in analysis 2 enabled us to evaluate the relative performance of several models that presented alternate scenarios related and unrelated to timber harvest. However, our approach required its own

level of validation. *Greven and Kneib* [2009] demonstrate that use of AIC is appropriate for selecting among linear mixed effects models that differ in their fixed effects parameterization when they share the same random effects parameterization. The authors suspected that a similar pattern of results would hold true for generalized linear mixed effects models, such as the logistic mixed effects procedure we used. Our simulation analysis results for analysis 2 support their suspicion, as our simulation results selected the lowest-AIC models at similar frequencies as predicted by the AIC model weights. Furthermore, the simulation indicated that the observed low AIC scores for models 6a and 6b with wide ($\Delta\mathrm{AIC} > 2$) separation from the null model were unlikely to have arisen from a "true" null model.

[47] This analysis completes an assessment of stream temperature PCW rule compliance for Oregon's state and private forests in the Oregon coast range. The study design was specifically developed to inform the Oregon Board of Forestry about the ability of FPA riparian management regulations and the state forest FMP to meet regulatory temperature requirements. This study's results will likely play a role in informing ODF rule assessment regarding the adequacy of current management practices at protecting stream temperatures. The board is ultimately responsible for policy decisions that alter the Oregon Forest Practices Act. We interpret the results to indicate that antidegradation compliance may be problematic for private lands in Oregon's Coast Range. Our analysis strictly evaluated a regulatory question; as a consequence, it provides limited insight into the severity of temperature increases or their cause. We additionally do not know the biological significance of the rises in temperature to aquatic life in these systems, the expected duration of expected warming, or the persistence of this warming downstream. We therefore recommend that resulting policy discussions about the riparian standards occur after additional information is gathered from a data analysis not constrained by specific regulatory language.

[48] We suspect that our observed changes in stream temperature are linked to decreases in shade that result from narrower, less dense riparian stands than were present prior to harvest. We expect the magnitude of the temperature response to harvest will additionally be affected by factors such as channel gradient [*Subehi et al.*, 2009], aspect [*Gomi et al.*, 2006], treatment length, channel width, elevation [*Arscott et al.*, 2001], channel substrate, wood storage [*Kasahara and Wondzell*, 2003], and subsurface hydrology [*Story et al.*, 2003]. Our next analytical effort will incorporate these parameters, step away from regulatory constraints, and collectively examine temperature responses from multiple sites and years (possibly relying on a mixed effects procedure as promoted by *Tate et al.* [2005]) and summarized either seasonally or at a daily or shorter time period [e.g., *Gomi et al.*, 2006]. Ultimately, we hope to describe stream temperature and large wood recruitment patterns for a full 5 years postharvest, a period when riparian vegetation has an opportunity to exploit increases in sunlight availability [*Quinn and Wright-Stow*, 2008], buffers are subject to windthrow [*Grizzel and Wolff*, 1998], and sediment may move into or out of the channels [*Bruijnzeel*, 2004]. The results from these analyses and others will inform Oregon Board of Forestry policy discussions on current regulations and potentially inform riparian timber harvest policy regulations elsewhere.

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