The Mass Wasting Effectiveness Monitoring Project: An examination of the landslide response to the December 2007 storm in Southwestern Washington

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Cooperative Monitoring
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The Mass Wasting Effectiveness Monitoring Project: An examination of the landslide response to the December 2007 storm in Southwestern Washington

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Washington State Forest Practices Adaptive Management Program

The Washington State Forest Practices Board (FPB) has established an Adaptive Management Program (AMP) by rule in accordance with the Forests & Fish Report (FFR) and subsequent legislation. The purpose of this program is to:

> *Provide science-based recommendations and technical information to assist the FPB in determining if and when it is necessary or advisable to adjust rules and guidance for aquatic resources to achieve resource goals and objectives. The board may also use this program to adjust other rules and guidance. (Forest Practices Rules, WAC 222-12-045(1)).*

To provide the science needed to support adaptive management, the FPB established the Cooperative Monitoring, Evaluation and Research (CMER) committee as a participant in the program. The FPB empowered CMER to conduct research, effectiveness monitoring, and validation monitoring in accordance with WAC 222-12-045 and Board Manual Section 22.

Report Type and Disclaimer

This technical report contains scientific information from research or monitoring studies that are designed to evaluate the effectiveness of the forest practices rules in achieving one or more of the Forest and Fish performance goals, resource objectives, and/or performance targets. The document was prepared for the Cooperative Monitoring, Evaluation and Research Committee (CMER) and was intended to inform and support the Forest and Fish Adaptive Management program. The project is part of the Mass Wasting Effectiveness Monitoring Program, and was conducted under the oversight of the Upland Processes Scientific Advisory Group (UPSAG).

This document was reviewed by CMER and was assessed through the Adaptive Management Program's independent scientific peer review process. This is a non-consensus CMER report not supported by all CMER members. The minority reports are appended to the report.

The Forest Practices Board, CMER, and all the participants in the Adaptive Management Program hereby expressly disclaim all warranties of accuracy or fitness for any use of this report other than for the Adaptive Management Program. Reliance on the contents of this report by any persons or entities outside of the Adaptive Management Program established by WAC 222-12-045 is solely at the risk of the user.

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Full reference

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The Mass Wasting Effectiveness Monitoring Project: An examination of the landslide response to the December 2007 storm in Southwestern Washington

Executive Summary

Research has shown that forestry-related activities have the potential to increase rates of mass wasting, and that sediment delivered from landslides can negatively affect aquatic resources. The 1999 Forests & Fish Report (FFR), written by federal, state, tribal, environmental and forest-industry representatives, acknowledges the historic relationship between forest practices and mass wasting (U.S.F.W.S. et al., 1999). The Forests & Fish Report included recommendations for reducing landslide sediment delivery (which were adopted into the Washington Forest Practices Rules in 2001) to improve protections for fish habitat and water quality on non-federal forest lands in Washington State.

The current rule strategy for reducing management-triggered landslide impacts is to require a State Environmental Policy Act (SEPA) review of proposed road or harvest activities on certain regulated unstable slopes, termed Rule-Identified Landforms (RIL) that can deliver sediment to public resources (defined as streams and infrastructure). If the SEPA review determines that a proposed activity is likely to have an adverse impact to a public resource, the Forest Practices Rules require that mitigation measures for forest harvest operations be designed to avoid accelerating rates and magnitudes of mass wasting. Because SEPA review is costly and time-consuming, the most common approach is to avoid any logging or road construction within landslide-prone terrain. The strategy for existing substandard roads is to upgrade them to current Forest Practices standards.

The primary objective of this project was to evaluate the effectiveness of current Forest Practices Rules at reducing landslide density and sediment delivery to public resources resulting from a major storm event. A secondary objective was to field identify site-scale management-related contributing factors that might be used to improve unstable slope identification and mitigation efforts.

STUDY DESIGN AND METHODS

The Mass Wasting Effectiveness Monitoring Project, commonly referred to as Post-Mortem, was scoped by the Upland Processes Scientific Advisory Group (UPSAG) in 2005. The study design was reviewed and approved by the Cooperative Monitoring Evaluation and Research (CMER) committee in 2006, and underwent an independent scientific peer review in 2007. In the wake of the December 2007 storm which caused significant flooding and landslide activity in and around the Chehalis River basin, the Forests & Fish Policy Committee (Policy) and the Washington Forest Practices Board authorized initiation of the project.

The study included the following features:

- It examined the landslide response to a single large storm on forest lands in southwest Washington that are subject to Washington Forest Practices Rules;

- Detection methods were designed to find and visit all landslides in the sample areas that delivered to streams, and all road-related landslides. Non-road-related landslides that did not deliver to streams were surveyed when encountered;
- Landslides were identified and characterized in the field to avoid aerial photograph detection bias, and to allow potential management-related contributing factors to be identified while landslides were relatively fresh;
- All landslides were inspected for the presence of site-scale contributing factors such as soil disturbance from logging or road drainage problems;
- A randomized block sampling design was used to minimize the influence of environmental factors that affect landslide occurrence (e.g., storm precipitation, topography, and lithology) in the statistical comparisons.

Forest Practices Rule effectiveness was evaluated through a statistical comparison of landslide response among sets of harvest and road 'treatments' identified at the scale of harvest units and road segments. Treatment determinations were based on past management activities evident from aerial photography and on-site observations. The harvest treatments include the riparian and other forest buffers as well as the harvested areas. This allows for the consideration of both local effects such as reduced rooting strength, and potential off-site effects such as soil moisture increases downslope of harvest. The number of landslides and their volumes for the landslides that delivered to public resources in each treatment, per unit area, served as response variables in a statistical comparison among treatments. Observed differences among treatments were considered the primary indicator of Forest Practices Rule effectiveness.

The treatments were defined as follows, with bold used to highlight treatments deemed critical to the evaluation of rule-effectiveness:

Harvest treatments

***No Buffer* — Harvest units from 0-20 years old with no buffering of RIL, if present;**

Partial Buffer — Harvest units and associated buffers from 0-20 years old in which some but not all RIL are buffered with mature timber;

***Full Buffer* — Harvest units and associated buffers from 0-20 years old in which all RIL, if present, are completely buffered with mature timber;**

Submature — Previously harvested forest stands from 21 to 40 years old;

***Mature* — Previously harvested forest stands greater than 40 years old. Note that virtually the entire study area had been harvested within the previous 100 years.**

Road treatments

***Substandard* — Forest roads that did not meet current Forest Practices Rule standards for construction, maintenance, and design;**

Orphaned — Roads that did not appear to have had any Forest Practices use since 1974 (per Washington Administrative Code 222-24-052 (4)), and were typically in an overgrown and undriveable condition;

***Standard* — Roads that met current Forest Practices Rule standards with respect to water management and tread conditions, but did not qualify as Mitigated, as defined below;**

Abandoned — Roads that had been deconstructed to the extent specified in Washington Administrative Code 222-24-052 (3)), including all culverts removed and vehicle access blocked;

***Mitigated* — Roads that met current Forest Practices Rule standards with evidence of additional mass wasting stability treatments (e.g., sidecast pullback) that indicate the highest level of road improvement effort.**

Descriptive Results

A total of 1147 landslides were found that delivered to public resources (mostly streams) in the 91 square mile study area. The majority (82%) occurred on hillslopes and the rest initiated from roads. The majority of road-related landslides (83%) were characterized as "hillslope road" which means they were not associated with stream-crossings; almost half of these did not deliver to public resources. Most of the stream-crossing road landslides (88%) were reported to have delivered to a public resource. Debris slides were the most common landslide process, followed by debris flow, with the two accounting for 96% of the landslides that delivered to public resources. Although debris flows accounted for 42% of delivering landslides, they are estimated to have delivered 2.3 million out of a total of 3.2 million cubic yards (71%) of sediment to public resources in the study area (Note: delivered volume estimates displayed large observer variability).

Landslide density varied greatly across the study area, and was different between hillslopes and roads. Hillslope landslide density in the four-square-mile blocks ranged from one to 23 per square mile, with an overall density of 11 landslides per square mile. Landslide density along roads ranged from zero to 142 landslides per square mile of road corridor, with an overall density of 33 landslides per square mile of road corridor. Road landslide densities also exhibited much greater spatial variability than hillslope landslides, with four of the 22 sample blocks accounting for 70% of all road failures. The overall landslide density within blocks appeared to be correlated with precipitation intensity.

The field crews identified site-scale contributing factors, such as surface water diversion or logging-related soil disturbance, at few landslide initiation sites. This was true for both landslides initiated on hillslopes (22% had a site-scale contributing factor identified) or along roads (32% had a site-scale contributing factor identified). This finding was largely confirmed in a Quality Assurance/Quality Control (QA/QC) exercise conducted by Washington Department of Natural Resources (WDNR) personnel who visited 143 randomly chosen sites and agreed with the field crew calls 97% of the time.

A sizable proportion of delivering hillslope landslides originated from terrain that did not fit the definition of any named RIL (between 29% and 41% depending on gradient estimates). Landslides that initiated outside of RIL were distributed throughout the study area and block analysis of the relative occurrence of landslides outside of RIL showed that their occurrence did not appear to be correlated with either precipitation intensity or lithology.

Other analyses were conducted on landslides from across the study area. A stand age analysis showed that the distribution of stand age at initiation generally followed the pattern of stand ages across the study area, but that a slightly higher proportion of landslides initiated among 30-50 year trees. Landslides originating in buffers delivered significantly more LWD than landslides outside of buffers, and the probability of LWD delivery for landslides in buffers increased with landslide size. For landslides that initiated outside of a buffer, the probability of LWD delivery increased with landslide initiation size and decreased with increasing gradient.

STATISTICAL COMPARISONS OF TREATMENTS

The descriptive analyses above do not account for the varying effects of precipitation, but the statistical analyses were conducted as part of a randomized block analysis to account for regional differences in precipitation, regional topography, and geology. Landslide counts and sediment yields in each treatment were normalized by area to estimate densities and landslide volumes per unit area. Harvest treatment densities and volumes were then quantitatively adjusted for differences in slope among treatments in a block in the statistical analysis. Statistical tests were non-parametric or were conducted with generalized linear mixed-models.

Comparisons of the three critical harvest treatments indicate that the No Buffer treatment had a significantly higher landslide density (a 65% increase) than Mature, which was used as a baseline for estimating treatment effects. The Full Buffer treatment had a landslide density that was intermediate to Mature and No Buffer (17% more than Mature, 30% less than No Buffer) but not statistically different from either. Furthermore, No Buffer delivered significantly more sediment than either Mature or Full Buffer (347% and 558% increase respectively). In contrast, Full Buffer delivered sediment volumes that were lower than, but were not statistically different from, Mature.

There were no statistically significant differences in landslide density or volume among the critical road treatments (Standard, Substandard, and Mitigated). Abandoned roads generated significantly less sediment than all road treatments other than Mitigated, and it delivered less sediment to public resources than either Standard or Substandard roads. Abandoned roads also had the lowest mean landslide density among the five road treatments, although differences in landslide density were not statistically significant.

DISCUSSION

Evaluating the effectiveness of Washington State's Forest Practices Rules at limiting landslide occurrence is inherently difficult given that rule changes are generally implemented across the landscape at a specific point in time which creates a time-dependence between any set of 'controls' and 'treatments' that might be compared. In addition, because the occurrence of landslide-producing storms cannot be predicted, methods are limited to retrospective time series using air photos (which have issues with detection bias), or to contemporary studies using Light Detection And Ranging (LiDAR) or field identification following individual large storm events.

This study is based on an analysis of landslide occurrence following a single large storm event affecting western Washington. Harvest unit treatments were mapped from aerial photography and some field review based on stand age and the buffering of RIL, while roads were mapped by observed road condition. Harvest unit RIL do not serve as experimental units because they could not be reliably mapped across the entire study area. All buffers, regardless of the reason for which they were left, are included in the treatments, and all delivering landslides, whether initiating in RIL buffers, other buffers or just within the general harvested area or stand, are included in the analyses. This means that RIL buffer effectiveness is not directly quantified, but that total buffer effectiveness, which may include other mitigating processes such as increased LWD delivery and decreased landslide delivery, was tested. The study incorporates spatial replication across a range of precipitation intensities; but it includes no form of temporal replication. Slope normalization was incorporated in the statistical analysis to account for inherent differences in landslide susceptibility among treatments, but the degree to which the slope index fully captures either landslide susceptibility or RIL distribution is unknown. The study was conducted on managed forest lands in southwest Washington with a landslide density of at least four landslides per square mile, and the population to which we make inference is similarly managed forests with similar climatic, geomorphic and land management histories; and a storm intensity that is able to generate a significant population of landslides over a large area.

The study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the density and volume of landslides. This interpretation is based on the comparison of landslide density in harvested areas with fully buffered and unbuffered RIL against areas of Mature forest, which serves as a control. As expected, harvest units in which RIL were clearcut (i.e., No Buffer) had significantly higher landslide densities and volumes than Mature forest, which confirms that harvest without RIL buffers increases resource impacts. In contrast, landslides in the Full Buffer treatment had a smaller overall volume and delivered less sediment than treatments in which the RIL were clearcut harvested. Interestingly, landslides densities in the Full Buffer and No Buffer treatments were not statistically different from each other. However, mean landslide densities in the Full Buffer treatment were closer to those found in Mature forest than the No Buffer treatment. As described in the report, there are several factors including changes in hydrology and root strength that appear to affect slope stability following harvest; and some of these factors create differences in slope stability that appear to vary with stand age. In a non-statistical analysis of densities among the five harvest treatments, in which each treatment is compared against expected density in the absence of buffering, stands with RIL buffers appear to have exhibited smaller increases in landslide density over those observed in mature forest than was observed in stands of similar ages where buffer influence was not included. This finding lends further credence to the hypothesis that RIL buffers reduce landslide density.

Although we conclude that buffers likely reduce landslide density and sediment volume, it is not clear that existing performance targets for hillslope landslides are being met. The performance target for harvest-related landslides under the current Forest Practices Rules indicates that new harvest should result in virtually no additional landslides triggered by harvest on high risk sites. Three findings raise the question of whether the performance targets are, or will, be met. The first is the lack of statistically significant differences in landslide density for the Full Buffer treatment. Based on this finding, it is not clear that the magnitude of the reduction in landslide densities associated with buffering of currently regulated RIL is sufficient for meeting performance targets. Second, it was observed that 47 landslides initiated on named RIL that were harvested under current rules and subsequently delivered to public resources. As discussed in the report, there are a number of possible reasons for recent clearcut harvest of RIL. Finally, it was found that the Partial Harvest treatment, which included some but not complete buffering of RIL, had significantly greater landslide densities than Mature forest when hit with a large storm event 4-7 years after harvest. Further work will be required to determine whether stands with full buffering of RIL can be expected to meet performance targets if hit by a large magnitude storm at a time when hydrologic and root strength effects are expected to elevate instability.

The effect of road treatment on landsliding was largely inconclusive with regards to density, but this is probably caused in part by the fact that 70% of landslides occurred in four blocks which may have reduced the power of the analysis to detect differences among treatments. Still, road abandonment did appear to be effective at reducing landslide volume, and is expected (though not statistically demonstrated) to reduce landslide density as well. Similarly, road instability mitigation work (e.g., sidecast pullback) is shown to reduce landslide volume relative to standard roads without such practices.

Surprisingly, neither contract nor QA/QC field crews found any obvious contributing factors at the majority of landslide initiation sites. Because these calls appear to be sound, we conclude that many road failures were caused by factors inherent to the treatments. The authors find support for concluding that the stability of the road network has been improved by modern construction and abandonment techniques given that road failures previously observed by the authors and others commonly exhibited clear evidence of a contributing maintenance problem or drainage malfunction, and that relatively few road landslides were found outside the portion of the study area that received the greatest precipitation.

Finally, it was noted that a sizable proportion of delivering landslides initiated on terrain that does not meet the current named RIL criteria. Although RIL occupy a relatively small percentage of the landscape yet still account for the majority of landslides, it was generally expected that an even higher percentage of landslides would initiate in RIL given that they are the primary regulated landforms developed during the Forests & Fish Negotiations from watershed analyses and they are defined in part by their high likelihood of delivery. It is worth noting that some of the landslide sites that did not meet a named RIL definition may have met a general description of a potentially unstable slope as provided in the Forest Practices Rules. The fact that the percentage of landslides outside of RIL was not correlated with geology or precipitation intensity undermines the interpretation that landslides outside RIL are limited to marginally unstable terrain that requires an extremely large precipitation event to fail. Further work is needed to identify characteristics of other landforms or areas of the landscape that are sufficiently unstable to justify modifying existing regulations.

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Appendices

Supplemental documents

Mass Wasting Prescription-Scale Effectiveness Monitoring Project Study Design (Dieu et al., 2008)

Field Manual for the Mass Wasting Prescription-Scale Effectiveness Monitoring Project (Phillips et al., 2008)

Mass Wasting Prescription-Scale Effectiveness Monitoring Project Quality Assurance / Quality Control (QA/QC) Report (Miskovic and Powell, 2009)

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SECTION 1: INTRODUCTION

Landslides are a fundamental component of landscape evolution, but landslide occurrence may impart significant socioeconomic and environmental costs (Schuster and Highland, 2001). In the forested environment, the cost associated with landslide occurrence is often evaluated in terms of the ecological impact (Sidle and Ochiai, 2006). Numerous studies conducted in the Pacific Northwest have shown that activities related to forest management have the potential to increase landslide occurrence (Dyrness, 1967; Megahan and Kidd, 1972; Swanson and Dyrness, 1975; Ketcheson and Froehlich, 1978; Amaranthus et al., 1985; Swanson et al., 1987; Robison et al., 1999; Jakob, 2000; Guthrie and Evans, 2004), and that sediment delivered by landslides to surface waters has had an effect on water quality or stream habitat (Everest et al., 1987; Cederholm and Reid, 1987; Geertsema and Pojar, 2007; Restrepo et al., 2009). In response to concerns over the impacts of landsliding, the Washington Forest Practices Board (WFPB) adopted new rules in 2001 that contain specific measures designed to reduce management-related landslide occurrence.

The Mass Wasting Effectiveness Monitoring Project was developed to help evaluate the effectiveness of the 2001 version of the Washington State Forest Practices Rules. The primary goal of the project is to determine whether mass wasting prescriptions and other measures are effective in reducing the number and size of management-related landslides that deliver sediment and debris to public resources. At the broadest level, the project will assess buffer effectiveness in limiting both landslide initiation and landslide delivery and the effectiveness of road practices that are designed to carefully manage drainage and reduce mass wasting potential.

The primary audience for this report is assumed to be stakeholder groups and concerned citizens of Washington State that have a general familiarity with Washington State Forest Practices activities and regulations. We provide definitions for technical terms in the glossary at the end of the report but do not include a detailed description of the process for implementation of the Forest Practices Rules. For further information refer to the Washington State Department of Natural Resources Forest Practices web site.[1]

1.1 Landslides in forested watersheds

Forest landslides are most likely to affect aquatic organisms through scour and sediment deposition along stream corridors (Cederholm and Reid, 1987). While landslides cause direct mortality to inhabitants of reaches in the runout path, changes in sediment transport regimes have the potential to affect stream-dwelling organisms over much longer distances. The very large volumes of sediment delivered to streams through mass wasting can greatly exceed the annual capacity of fluvial transport, and subsequent sedimentation impacts can persist for many years (Dietrich and Dunne, 1978; Benda and Dunne, 1997). Impacts may include sediment deposition in spawning and rearing habitat of salmonids and other aquatic organisms (Everest et al., 1987; Cederholm and Reid, 1987). While excessive sediment delivery is associated with habitat degradation, aquatic habitat can also benefit from

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[1] http://www.dnr.wa.gov/BusinessPermits/Topics/ForestPracticesRules/Pages/fp_rules.aspx

the delivery of gravel and large wood and boulders which form critical components of habitat (Benda et al., 2003; Geertsema and Pojar, 2007; Restrepo et al., 2009). Given this ecological response, a performance target for the Washington State Forest Practices Adaptive Management program is to limit the rate of landslide occurrence in managed forests to the rate associated with 'natural background.'[2]

1.1.1. Natural factors influencing slope stability

There is an extensive body of literature that examines the factors influencing slope stability for shallow, rapid landslides. Much of the literature involves case studies of landslide occurrence on managed forest landscapes, either at the scale of individual landslides or the watershed scale. Most are based on retrospective analyses of landslide occurrence after high-intensity storm events. These case studies seek to identify the factors that contributed to the slope failure. Particularly relevant studies of natural factors affecting slope stability are briefly discussed to help establish the context of this study.

Hillslope hydrology: Landslides commonly occur in response to high-intensity rainstorms and/or snowmelt events that release large volumes of water over a period of days, particularly when relatively heavy rainfall has occurred during the preceding weeks (Campbell, 1975; Starkel, 1979; Caine, 1980; Dai and Lee, 2001; Rahardjo et al., 2001; Jakob and Weatherly, 2003; Godt et al., 2006; Jakob et al., 2006; Crosta and Frattini, 2008; He and Beighley, 2008; Tsai, 2008). Slope stability is substantially reduced when the soil moisture content is at or near saturation because of the added weight of water and the hydrostatic forces exerted on the soil mass that reduce frictional resistance of particles to downslope movement (Iverson, 2000).

Topographic factors: Steep, convergent slopes are associated with some of the highest probabilities of failure. As slope increases, so does the down slope component of the gravitational forces acting upon soil particles. Convergent slopes tend to accumulate soil over time while focusing subsurface flow which increases the likelihood of soil saturation and failure (Montgomery et al., 2000).

Lithology and soil properties: Studies have documented regional differences in landslide rates that appear to be related to differences in lithology and geologic history (e.g., Montgomery et al., 1998; Sarikhan et al., 2008; Thorsen, 1989). Orientation of the bedding and fractures in the bedrock may also influence the specific location of landslides (e.g., Montgomery et al., 1997).

1.1.2. Forest management effects on slope stability

Landslides are a natural occurrence in western Washington but forest practices may alter both physical and biological factors that influence slope stability. The following is a brief summary of the most common forest management effects.

Hydrologic effects: The removal of forest canopy results in increased soil moisture because of reductions in both canopy interception and evapotranspiration (Lewis et al., 2001; Johnson et al., 2007). During storm events, evapotranspiration is generally small compared to the rate of precipitation and

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[2] http://www.dnr.wa.gov/Publications/fp_am_ffrschedulel1.pdf

canopy saturation can occur, but forest cover can still affect landslide occurrence by smoothing the transfer of water to the soil which in turn modulates peak pore pressures (Keim and Skaugset, 2003). The removal of canopy simultaneously enhances snow accumulation and melt which can increase peak soil moisture (Coffin and Harr, 1992; Marks et al., 1998) and result in greater landslide occurrence.

Loss of root strength: Tree roots are believed to contribute significantly to slope stability. When soils are at or near the angle of repose, root systems serve to reinforce soil strength and provide resistance to gravitational forces that tend to pull soil masses downhill (Riestenberg and Sovonick-Dunford, 1983; Schmidt et al., 2001). Timber harvest reduces root reinforcement during the period when harvested timber root systems are decaying and new root systems are expanding (Ziemer, 1981; Sidle and Ochiai, 2006). Total root strength is believed to be at a minimum between approximately 4 and 10 years after timber harvest (Sidle, 1991; Sidle 1992; Schmidt et al., 2001). Simulation studies illustrate that vegetation leave areas can significantly reduce landslide volumes by retaining available root strength in areas prone to failure (Sidle and Ochiai, 2006).

Road construction: Landslide inventories in the Pacific Northwest have established that roads in steep terrain have historically been responsible for a high proportion of landslides in managed forests (Robison et al., 1999). Poor construction techniques and inadequate drainage are believed to be the main causes (Furniss et al., 1991). Landslides associated with forest roads often initiate from sidecast road fill material perched on steep slopes. Road failures can occur when stream crossing or drainage culverts become plugged and excessive runoff is concentrated on unstable slopes. The use of uncompacted fill and the inclusion of organic material (logs) in road fill have also been found to contribute to slope failures (Sidle and Ochiai, 2006). Modern road building techniques include 1) the construction of steeper grades which reduces road mileage and 2) the complete removal of excavated material to lower gradient waste areas. These techniques have significantly reduced road landslide frequency (Sessions et al., 1987), but hydrologic alteration remains difficult to avoid (Montgomery, 1994; Borga et al., 2004).

1.2 Washington's Forest Practices Rules

The Washington Forest Practices Act was enacted in 1974, and the Forest Practices Rules have undergone numerous changes since that time.[3] In 1999, a diverse group of stakeholders which included tribes, forest landowners, state and federal governments, environmental groups, and other interests, wrote the Forests & Fish Report (FFR) which contained strategies for protecting water quality and aquatic and riparian-dependent species on non-Federal forestlands in Washington.[4] In 2001, the Washington State Legislature and the Washington Forest Practices Board (Board) amended the Forest Practices Act and its corresponding Forest Practices Rules to incorporate recommended changes from the report.

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[3] http://www.dnr.wa.gov/Publications/fp_rules_history.pdf

[4] http://www.dnr.wa.gov/Publications/fp_rules_forestsandfish.pdf

The Forest Practices Rules are adopted by the Board, and Washington Administrative Code (WAC) 222-10-030 requires that the Washington Department of Natural Resources (WDNR) develop policies that minimize management-related landslides that could deliver sediment or debris to a public resource or threaten public safety. Public resources are defined as water, fish, and wildlife and in addition shall mean capital improvements of the state or its political subdivisions (WAC 222-16).

Potentially unstable slopes and landforms are defined in WAC 222-16-050 (1(d)) and Section 16 of the Board Manual contains guidelines for identifying unstable slopes and landforms. In the Board Manual, unstable slopes and landforms are referred to collectively as Rule-Identified Landforms (RIL).[5] WAC 222-16-050 requires that road building and timber harvest activities proposed on RIL with the potential to deliver sediment or debris to a public resource, and which has been field verified by WDNR, be classified so that they receive additional environmental review under the State Environmental Policy Act (SEPA) review described by WAC 222-10-030.

WAC 222-24-010 outlines goals for road maintenance and WAC 222-24-050 requires that all forest roads owned by large landowners be improved and maintained to the standards of the WAC by July 1, 2016. To facilitate this, WAC 222-24-051 requires that large landowners submit Road Maintenance and Abandonment Plans (RMAP) and annual accomplishment reports thereafter. The RMAP must prioritize efforts to remove barriers to fish passage, focus on active haul roads that deliver sediment to typed waters, and reduce landslide potential that could adversely affect public resources.

1.2.1. Rule-Identified Landforms

During the FFR negotiations, a review of Washington watershed analyses and other research (e.g., Robison et al., 1999) indicated that a high proportion of shallow, rapid landslides were associated with a particular set of landforms. These landforms were briefly identified in Appendix C of the FFR, and were later incorporated into WAC and the Board Manual.

The RIL, as identified in WAC 222-16-050 (1(d)), are:

> "(A) Inner gorges, convergent headwalls, or bedrock hollows with slopes steeper than 35 degrees (70%);
>
> (B) Toes of deep-seated landslides, with slopes steeper than 33 degrees (65%);
>
> (C) Groundwater recharge areas for glacial deep-seated landslides;
>
> (D) Outer edges of meander bends along valley walls or high terraces of an unconfined meandering stream; or
>
> (E) Any areas containing features indicating the presence of potential slope instability which cumulatively indicate the presence of unstable slopes."

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[5] http://www.dnr.wa.gov/Publications/fp_board_manual_section16.pdf (updated 11/04)

Section 16 of the Board Manual contains illustrated guidelines for identifying each of the RIL. In short, inner gorges are characterized by steep (greater than 70%), straight or concave sideslope walls with at least 10 feet of relief that commonly have a distinctive break-in-slope with more stable terrain above the break. Convergent headwalls are funnel-shaped landforms, broad at the ridgetop and terminating where headwaters converge into a single channel. The upper portion of a convergent headwall is usually formed of numerous bedrock hollows separated by knife-edged ridges. Bedrock hollows are spoon-shaped areas of convergent topography; they are typically 30-300 feet wide, have developed through repeated landslide initiation, and are considered a potentially unstable slope when their gradient is 70% or greater. Toes of deep-seated landslides define the terminus of a landslide deposit, and where these are adjacent to a stream and the slopes are greater than 65%, they are defined as a RIL. Groundwater recharge areas of glacial deep-seated landslides are defined as upslope areas where groundwater in glacial deposits contributes subsurface water to a deep-seated landslide. The outer edge of a meander bend of a stream is an unstable landform where stream undercutting is oversteepening valley walls or high terraces.

In addition to specific landform definitions, other areas may contain features indicating the presence of potentially unstable slopes. Indicators such as hummocky or benched topography; scarps or cracks; fresh debris deposits; displaced or deflected streams; jack-strawed, leaning, pistol-butted, or split trees; water-loving vegetation and others may be used. Individually these observations do not prove that slope movement is imminent, but cumulatively may indicate the presence of potentially unstable slopes.

1.3 The Mass Wasting Effectiveness Monitoring Project (Post-Mortem)

The FFR recommended that the Cooperative Monitoring, Evaluation and Research committee (CMER) evaluate the effectiveness of the 2001 unstable slopes rules as part of the Forest Practices Adaptive Management Program. The Upland Processes Scientific Advisory Group (UPSAG), a subcommittee of CMER, presented scoping documents for three mass wasting effectiveness monitoring projects to CMER in November 2005. One of these recommended a study to examine the landslide response to a single large storm event. Because the study involved a post-facto examination of the landslide response to a large damaging storm, it was nicknamed the Post-Mortem Study.

UPSAG recommended to the FFR Policy committee that the Post-Mortem Study be prioritized for immediate development, because its implementation would require a landslide-producing storm event, the timing of which could not be predicted. UPSAG started working on the study design in 2005 and it was finalized in January, 2008, after going through Independent Scientific Peer Review.

The project is expected to inform other projects listed in the CMER Work Plan including the Mass Wasting Landscape-Scale Effectiveness Monitoring Project and the Testing the Accuracy of Unstable Landform Identification Project (Dieu et al., 2008).[6]

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[6] http://www.dnr.wa.gov/Publications/bc_cmer_workplan.pdf

1.3.1. Research objective

The primary objective of the Mass Wasting Prescription-Scale Effectiveness Monitoring Project (Post-Mortem) is to determine whether mass wasting prescriptions are effective at reducing the size and number of management-related landslides that deliver to public resources, in accordance with the FFR goals. Although the study was initially labeled as a "prescription-scale effectiveness monitoring project," the study was not designed to evaluate individual prescriptions, in part because they are not applied independently of one another. Instead, prescriptions were to be evaluated as they related to a set of conditions found along road segments and in harvest units.

The Mass Wasting Prescription-Scale Effectiveness Monitoring Project Study Design (Dieu et al., 2008) included a set of critical questions to be answered by the study:

- Are the Forest Practices Rules effective in reducing the number of management-related landslides that deliver to public resources?
- Are the Forest Practices Rules effective in reducing the volume of sediment that delivers to public resources as a result of management-induced landslides?
- Which are responsible for the greater proportion of landslides and sediment volume, hillslopes or roads?

Although the study was designed to statistically test for differences among a set of road and a set of harvest unit conditions, a large amount of site-scale data was also collected in the hope that these might reveal patterns of interest. Questions of interest related to the ancillary data include the following:

- Which harvest unit prescriptions or road improvements are performing well? Which are performing poorly?
- What are the site-scale triggering mechanisms for landslides?
- Do those triggering mechanisms differ between harvest or road type?

SECTION 2: STUDY DESIGN

Effectiveness monitoring studies are designed to establish whether management actions produce desired outcomes. To accomplish this, effectiveness monitoring must involve some level of experimentation. Experiments are the manipulation of a system to gather information about the response; they require some level of replication, randomization, and at least a consideration of the need for blocking (Montgomery, 1991).

Controlled experiments are the most powerful method for establishing cause and effect relationships, because the application of treatments is at the complete control of the experimenter. Unfortunately, it is often difficult to conduct controlled experiments in natural systems because the scale over which observation must take place is large, or because there are operational constraints that make the application of manipulative treatments impractical (Quinn and Keough, 2002). Even when those issues can be resolved, there may be political or environmental constraints which prevent the use of prospective manipulative designs in adaptive management research (Sit and Taylor, 1998). It would be socially unacceptable, for example, to initiate a large number of landslides for the sole purpose of evaluating the effectiveness of a management strategy designed to minimize landslide occurrence. An alternative approach is to examine effects in events that have already occurred (Sit and Taylor, 1998).

Retrospective and observational studies are called quasi- 'experiments' (*sensu* Underwood, 1990) when they employ all the components of statistical design (e.g., replication, randomization and blocking) other than the deliberate application of a treatment to a selected set of experimental units. Quasi-experiments require the same level of effort as fully controlled experiments with respect to design and data collection, but they typically offer weaker inference because the application of treatments is outside of the experimenter's control. They may also require the acceptance of additional assumptions, some of which may not be true but whose consequences are hopefully minimal. Despite this, they remain an essential tool for adaptive management research because they offer important insights and predictions for the future events (Sit and Taylor, 1998). As the nickname indicates, the Post-Mortem is a retrospective study and it is treated as a quasi-experiment.

Despite these advantages, a retrospective study of this type is not designed to address the following important aspects of landslide management:

- Characterizing and defining potentially unstable hillslopes;
- Evaluating the site-scale mechanistic causes of landslides;
- Determining the influence of site-scale variables, such as precipitation, stand age, and topography on landslide initiation;
- Characterizing long-term landslide rates over multiple storm events; or
- Quantifying the efficacy of individual Best Management Practices.

Table 2-1: Terms from the Post-Mortem Study Design that were modified in this report.

Post-Mortem Study Design	This report
Strata	Treatments
Clearcut stratum	No Buffer treatment
Partial Harvest stratum	Partial Buffer treatment

However, this study does utilize a combination of conventional and novel approaches to landslide study design and analysis. Thus, reading this chapter carefully will help the reader recognize important differences between this study and other landslide research.

This chapter covers key aspects of the Mass Wasting Prescription-Scale Effectiveness Monitoring Project (Post-Mortem) Study Design including a statement of the problem, a description of the treatments, and the sampling scheme, design limitations and scope of inference. The complete 'Post-Mortem' Study Design (Dieu et al., 2008) is available as a supplement to this report, though it should be noted that several terms employed in the original design have been modified to enhance clarity in this report (Table 2-1).

The steps for designing an experiment include the development of a clear problem statement, the selection of a response variable, and identification of important factor variables. When the first three steps are done correctly, the choice of sampling and analytical design is generally easy (Montgomery, 1991).

2.1 Problem statement, response variable, and experimental factors.

In the Post-Mortem Study, the stated problem is to evaluate the landslide response among a set of treatment conditions representative of Forest Practices Rules, at the scale of individual harvest units or road segments, in response to a high-precipitation, landslide-triggering storm event that affects at least 3 watershed administrative units (~90,000 acres) of forest lands subject to Washington Forest Practices Rules.

The response variable is the number of landslides and the relative size of landslides among a set of experimental factors. The experimental factors for the Post-Mortem Study are a set of five harvest treatments and a set of five road treatments. Each treatment is defined by a set of mutually exclusive characteristics at the scale of an individual harvest unit or forest stand,[7] and road segment.

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[7] As described below, the boundaries of younger harvest treatments delineate as discrete areas of timber harvest (i.e., harvest units), while older treatments are delineated by forest stands of a relatively constant age. We do not consistently make the distinction between harvest units and forest stands in the remainder of the report, but may refer to both as 'harvest units.

2.2 Harvest treatments

Harvest units and forest stands of near uniform age act as experimental units which received treatments that were applied at the time of harvest. Treatments reflect practices carried out under a particular set of Forest Practice Rules. A primary characteristic of the harvest treatments is time since harvest, and it is broken into three age groups: 0-20 years, 21-40 years, and 41+ years. There are three harvest treatments in the 0-20 year group and the secondary characteristic used for differentiating among them is the degree of buffering on *named* RIL.[8] This secondary characteristic critically represents different unstable slope buffering strategies, but also encapsulates different strategies in all buffer types. The three 0-20 year old treatments are:

No Buffer — This treatment is comprised of harvest units with no buffering of RIL, if present. Silvicultural clearcuts meet the definition for this treatment;

Partial Buffer — This treatment is comprised of harvest units with buffering of some, but not all, RIL. This may include thinning on RIL, the cutting of yarding corridors across RIL, or incomplete buffering of one or more RIL regardless of the reason for the partial buffering;

Full Buffer — This treatment is comprised of harvest units with buffering of all RIL, if present.

Stands that were 21 years old or older at the time of the storm are divided into two groups:

Submature — This treatment is comprised of forest stands with a planted age of 21 to 40 years;[9]

Mature — This treatment is comprised of forest stands with stand age greater than 40 years.

Because the study is designed for implementation on managed forest land, the design does not anticipate encountering enough stands older than 60 years to justify another older treatment. Operational constraints affecting treatment delineation are noted in Section 3.4.

The timber age classes of less than 20 years, 21-40 years, and 41+ years are chosen based on an evaluation of literature related both to root strength decline and recovery, and hydrologic recovery (Dieu et al., 2008). Following harvest, root strength declines rapidly reaching a minimum 4-10 years after harvest and the next 10 years allow for significant hydrologic recovery and some limited root strength recovery (Sidle, 1991; Sidle, 1992; Figure 2-1). Thus, the three 0-20 year treatments cover the period of increased landslide hazard which is generally considered to be from 3 to 15 years after harvest (Sidle et al., 2006). Although the three 0-20 year treatments represent different eras of Forest Practices, it is hoped that implementation of Watershed Analysis prescriptions written in the late 1990's

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[8] Named RIL refers to landforms named in WAC 222-16-050(1(d)). WAC 222-16-050(1(d)) also includes an unnamed RIL (category E), which is defined as areas that contain features which indicate the presence of unstable slopes. Because category E has no formal definition, it was not used in this study.

[9] Hereafter, we consistently use the phrase stand age understanding that both our field data estimates and landowner data are not actual tree age, but stand initiation age which means when the trees were planted.

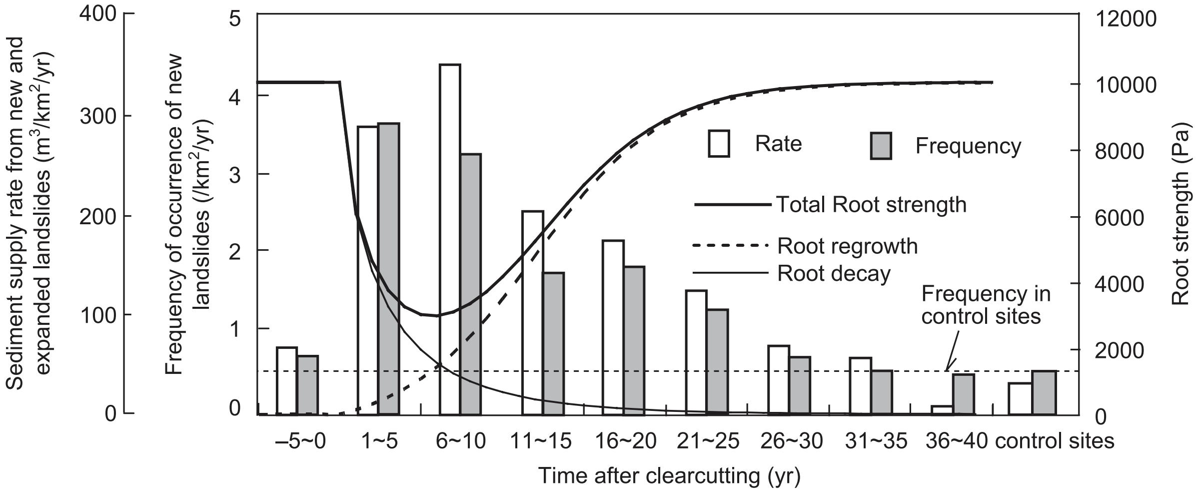

Figure 2-1: Dynamic root strength values from Sidle's (1991, 1992) model (line) superimposed on changes in sediment supply and landslide frequency (bars) as a function of time after clearcutting (from Imaizumi et al., 2008).

will have created older examples of the Full Buffer and Partial Buffer treatments to compare with the No Buffer treatment.

Although the degree of buffering on RIL serves as the secondary characteristic for the delineation of 0-20 year old harvest units, RIL themselves are not mapped and are not directly evaluated as experimental units. It has not been demonstrated that RIL can be reliably identified using remote sensing techniques, and mapping individual RIL is particularly difficult in areas of very dense vegetation (i.e., Submature and Mature stands). During the development of the study design, it was decided that the mapping of individual RIL over the entire study area was infeasible. It is assumed that over a large area, RIL will be evenly distributed among treatments even if they are not found in every harvest unit. That individual RIL are not mapped within the treatments means that the comparison of buffer effectiveness among treatments is really a test of the effectiveness of all buffer types as they limit either landslide initiation or landslide delivery; the effectiveness of unstable slope buffers to limit landslide initiation cannot be separately quantified by this study design.

Three of the five treatments are considered 'critical' with respect to sampling intensity because they serve as a point of reference for the evaluation of Forest Practices Rule effectiveness. No Buffer is considered a critical treatment because it is likely to be comprised primarily of silvicultural clearcuts and therefore may represent a pre-FFR harvest treatment. Full Buffer is considered a critical treatment because it represents full implementation of the current Forest Practices Rules unstable slope buffer strategy. Mature is considered a critical treatment because it serves as a baseline against which other treatments are compared, though it is not presumed to represent old-growth or natural background conditions (Dieu et al., 2008).[10]

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[10] Partial Buffer is considered a non-critical treatment because it was assumed that there would not be enough Partial Buffer harvest units on the landscape to meet the minimum sample size requirements. Submature is considered non-critical because it is an intermediate age that is not considered critical for an interpretation of rule effectiveness.

2.3 Road treatments

Road treatments are classified at the scale of road segments according to their tread, drainage and stability conditions with respect to the Forest Practices Rules and in the context of the RMAP program. Road treatments are applied to road segments that begin and end at road intersections. The road treatments are as follows:

Abandoned — This treatment is comprised of road segments that have been deconstructed to the extent specified in Washington Administrative Code 222-24-052 (3)), including all culverts removed and vehicle access blocked;

Mitigated — This treatment is comprised of road segments that met current Forest Practices Rule standards with evidence of additional mass wasting stability treatments (e.g., sidecast pullback) that indicate the highest level of road improvement effort.

Standard — This treatment is comprised of road segments that are drained and graded in accordance with the Forest Practices Rules, but do not qualify as Mitigated as defined above.

Orphaned — This treatment is comprised of road segments that have not been used for Forest Practices since 1974 (per Washington Administrative Code 222-24-052(4)) and thus are legally exempt from the Forest Practices Rules and RMAP work.[11] They are typically in an overgrown and undriveable condition.

Substandard — This treatment is comprised of road segments that deviate in some substantial aspect from drainage, grading, or construction criteria defined by the Forest Practices Rules or which do not meet current standards for maintenance and design.

Three road treatments are considered critical with regard to sampling intensity. Standard roads are considered a critical treatment because they represent the primary road condition landowners are working towards in their RMAP efforts. Substandard roads are considered critical because they allow for an evaluation of the difference between older practices and modern Forest Practices. Mitigated roads are considered critical because they represents roads where mitigation efforts have been made to reduce landslide potential.[12]

2.4 Sampling and analytical design

The Post-Mortem Study employs multi-stage cluster sampling for data collection. In cluster sampling, the landscape is partitioned into a set of primary units (clusters or blocks), each of which includes a set of secondary units from which samples are collected (Thompson, 2002). Clusters are chosen at random, and all secondary units within each cluster are included in the sample (Thomp-

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[11] The first Forest Practices Act was enacted in 1974. Roads constructed prior to 1974 are not subject to the Forest Practices Rules if they have not been used for purposes defined in the Act since 1974.

[12] While Forests & Fish stakeholders are interested in the relative instability of Orphaned and Abandoned roads, it was assumed that minimum sample sizes could not be met given the variable density of these roads across the landscape.

son, 2002; Quinn and Keough, 2002). The second stage involves augmenting clusters which fail to meet area or length requirements for critical treatments (See Section 2.4.3 for details). Augmented clusters serve as blocks in a randomized block design (Table 2-2 lists key components of the study design).

Blocking is required to control for the effects of precipitation. Precipitation intensity has a significant affect on landslide occurrence (see Section 1.1.1) and observational studies that fail to account for the effect of precipitation are likely to confound treatment and precipitation effects. In this study, it is assumed that precipitation intensities within a randomly selected 4 square mile cluster are relatively similar, and that there is no consistent bias with respect to within-cluster differences in precipitation intensity among treatments. If this assumption is true, precipitation effects are accounted for by blocking treatments so that treatment responses are evaluated relative to the mean block response.

It was decided during the development of the study design that the survey would be field-based because of the detection bias associated with aerial photography inventories (see Section 2.5) and the desire to obtain more data on site-scale triggering mechanisms. The cost associated with the field identification of landslides was reduced by the decision to focus the detection effort on all landslides that delivered to streams because these are the ones upon which Forest Practices focus (WAC 222-16-050), and all landslides that were initiated from roads so that we could evaluate site-scale triggers without bias. In addition, the implementation of cluster sampling, as opposed to blocking individual treatments in close proximity to one another, reduced travel costs by focusing data collection efforts into watershed-scale units.

Table 2-2: Study components

Sampling component	Definition
Population	Forest lands in Washington State that have landslides and are subject for Forest Practices Rules.
Sample frame	Known state and private forest lands with a single-storm-induced landslide density of one landslide per square mile, as identified in aerial photographs.
Experimental unit	Road segments, harvest units, and even-age forest stands.
Response variable	The number (count) and size of landslides that deliver to public resources (e.g., streams).
Treatment	A set of conditions that relate to road and harvest activities carried out under a set of Forest Practices Rules.
Cluster	A randomly chosen block composed of four contiguous public land survey sections.
Cluster frame	Twelve sections surrounding a cluster that are used as part of multi-stage cluster sampling.
Block	Clusters (and associated experimental units from the cluster frame) act as blocks and are used to account for spatial variability in precipitation intensity.

2.4.1. Sample size

The size and number of clusters to be included in the study were determined using data collected for a study that examined landslide occurrence in the Siuslaw National Forest following a 1996 storm (Robison et al., 1999: Miller and Burnett, 2008). Using those data, a power analysis was conducted for a two-factor Analysis of Variance (ANOVA) in which block and stand condition were treated as fixed effects. That analysis indicated that for a similar storm, a minimum of 21 blocks would be required in order to make comparisons among treatments with a power of 0.9 given a significance level of 0.1. Work conducted in conjunction with the power analysis indicated that clusters smaller than 3.8 square miles (10 square kilometers) would have insufficient area in at least one of the five treatments, and that smaller clusters would be likely to yield a large number of zero landslide counts (Dieu et al., 2008). Based on this information, we chose a sample size of 21 blocks with a block size of four-square miles.

It was understood by CMER and ISPR during the development of the study design that a landslide-triggering event sufficient to create the necessary sample size would require a 'big' storm. The potential limitations of this design constraint are discussed in Section 2.5 and in Section 7.2 (Dieu et al., 2008).

2.4.2. Augmenting clusters based on exposure

Assuming a uniform distribution of all the factors affecting landslide initiation, the landslide counts in a given treatment should increase in direct proportion to the size of the treatment area. When a response variable is expected to be strongly linearly correlated (with a zero intercept) with an ancillary variable like area, the use of ratio estimators can lead to a dramatic improvement in precision with negligible bias (Thompson 2002). Unfortunately, while ratio estimators may be unbiased, the variance associated with ratio estimates becomes large as the area of exposure becomes very small (see Appendix A.1 for an example).

In this study, treatment area is not fixed as part of the experimental design because it is the product of historic forest practices whose distribution is initially unknown. To account for the potential effect of treatment area differences, a ratio estimator (e.g., landslides per unit area, or landslide density) is employed in the analysis. To avoid problems with high variance in treatments that were considered critical to the study, a second stage was added to the cluster sampling. If a treatment occupied less than 5% of the initial cluster area (or total road length), then a single PLS section was systematically chosen from the 12 sections surrounding the cluster, called the cluster frame (Figure 2-2). The single frame section was canvassed for underrepresented critical treatments. Harvest units and road segments from the underrepresented treatment were added to the survey in the order they were encountered. Canvassing of block frame sections continued in a counter-clockwise direction until the 5% criteria was reached. Once a frame section was used to augment a cluster, it was no longer available for use in augmenting other adjacent clusters to avoid double sampling. Cluster augmentation using sections from the frame also occurred when significant areas (e.g., 40+ acres) of the cluster were found to be non-timberland or floodplain with little potential for unstable slopes.

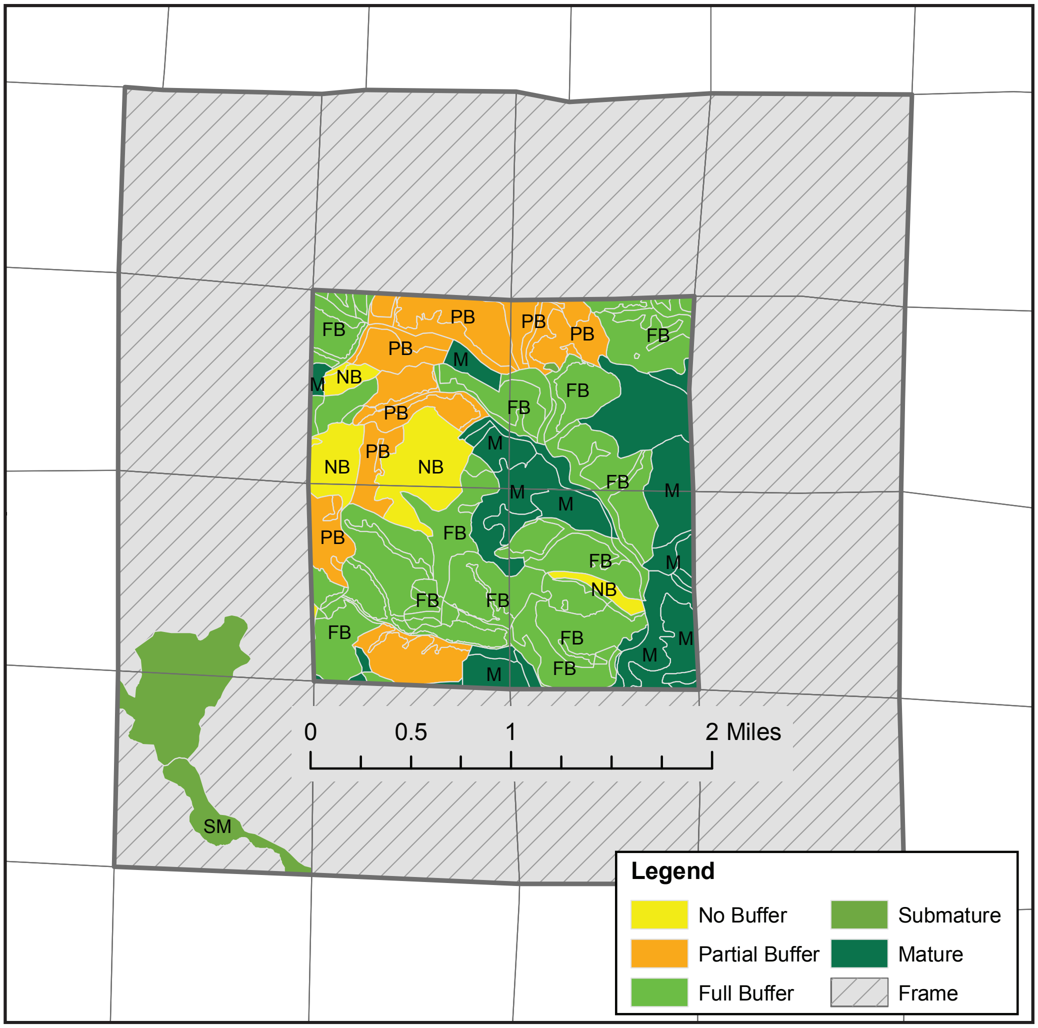

Figure 2-2: Example of harvest stratification in a four-square-mile cluster with under-represented harvest units augmented by sampling sections in the frame (i.e., 12 gray sections surrounding the cluster).
Blocks are composed of the initial cluster and sample units augmented from within the frame. Note that the Submature (SM) polygon in the lower left corner of the frame was added because none of that treatment was present within the cluster boundary.

2.5 Design limitations

There were several constraints identified by CMER and ISPR during the study design phase that may affect the findings. The most significant constraint was the choice to perform a retrospective study rather than a controlled experimental design. This decision was deemed necessary because it was considered infeasible to establish and harvest experimental sites and wait for or artificially initiate a landslide response. It was also considered that accelerating landslide occurrence for the sole purpose of demonstrating the effectiveness of a mitigation strategy was largely unacceptable.

It was decided in the study design phase that the study would examine landslide response to a single large storm rather than landslide surveys from aerial photographs because they are biased against the detection of small landslides and landslides that occur in dense vegetation (Brardinoni et al., 2003; Miller and Burnett, 2007; Turner et al., 2010). As a result, the study was limited to evaluating the spatial density of landslide response to a single large storm rather than the temporal frequency of landslides to storms of varying intensity.

There have been concerns raised that the effect of forest practices on landslide response to large, high-intensity storms may not be representative of small or medium-sized events. Results from recent studies are not in agreement regarding these concerns. Several recent studies evaluating the hydrologic response to forest harvesting support the concern by indicating that pressure head changes related to harvesting were less during large storm events (Dhakal and Sidle, 2004a; Dhakal and Sidle, 2008). Also, Gorsevski et al. (2006) recently modeled landslide susceptibility; their results indicate that while large events cause the greatest spatial instability, the smaller and more frequent events cause the greatest temporal instability. In contrast, a recent field-based study by Turner et al. (2010) found that the effect of stand age (a proxy for forest practices) was greatest at the highest storm intensity.

The magnitude-frequency issue has also been examined in the field of forest hydrology by Alila et al. (2009) who utilized a frequency distribution framework to evaluate the effects of forest practices on hydrology. This type of framework is not feasible for field-based landslide studies, however, because of the long duration between landslide events and the spatial variability in landslide response.

The use of a retrospective study also increased the potential for 'time since harvest' to act as a confounding factor with respect to three 0-20 year old harvest treatments. As noted previously, it is assumed that either Watershed Analysis prescriptions will have resulted in older Full Buffer or Partial Buffer harvest units that overlap in age with No Buffer harvest units; or that the effect will be accounted for through the inclusion of stand age as an auxiliary variable. The retrospective study also eliminated the potential inclusion of temporally variable parameters as auxiliary variables (e.g., local precipitation, snow accumulation, soil saturation) because the timing of the storm event could not be predicted in advance. As noted previously, we assume that those effects are accounted for through the inclusion of block as a random factor in the statistical analysis of landslide response by treatment.

2.6 Statistical inference

Statistical inference is the process of making conclusions based on the response in samples drawn from a larger population. Inference is extended to the population of interest through a set of assumptions attached to a particular study design (Sit and Taylor, 1998). In the Post-Mortem Study, inference related to relative landslide occurrence (i.e., landslides per unit area) can be generalized back to the landscape and conditions of the study area through the random selection of the blocks and the complete enumeration of road landslides and hillslope landslides that deliver to public resources.

SECTION 3: IMPLEMENTATION

In early December 2007, a series of three storms moved through the Pacific Northwest. These storms produced high winds and delivered large quantities of rain that helped trigger thousands of landslides in western Washington (Sarikhan et al., 2008) and led to significant flooding in the Chehalis basin (Mote, 2007). Preliminary reports suggested that the December 2007 storm event had produced a population of landslides over at least 120 square miles of largely commercial forest land subject to Forest Practices Rules. Aerial reconnaissance conducted by the WDNR confirmed that there was a large population of landslides in and around the Chehalis Basin. Based on these reports, UPSAG members secured appropriate permissions from CMER, Forests & Fish Policy, and the Forest Practices Board to proceed with implementation. UPSAG, with the help of other geologists, landowners and WDNR staff, developed a map defining the extent of the potential study area (Figure 3-1).

3.1 Study area

The study area is located in the Environmental Protection Agency's (EPA) level III Coast Range Puget lowlands ecoregions, west of the Coastal Sitka Spruce (*Picea stichensis*) Zone (Figure 3-1). The geology of the area is characterized by a mixture of Eocene and Miocene basalts and marine sedimentary formations (Washington Division of Geology and Earth Resources, 2005). In the Volcanic ecoregion, the topography is steep and the surficial geology is generally associated with Eocene basalt flows and basalt breccias of the Crescent formation. The Willipa Hills ecoregion has low rolling hills and mountains of moderate gradient, and the surficial geology is associated primarily with Eocene and Miocene sandstone, siltstone, and shales (Pater et al., 1998). Most hillslope soils in the study area are formed of colluvium and residuum derived from basalt and basaltic volcanic breccias; deep to moderately deep, well drained loams and cobbly loams are common (Evans and Fibich, 1987). In the study area, only small areas of hillslope soils are formed of sedimentary materials; this is merely a coincidence of storm intensity and distribution of geologic materials. Glacial materials are only present on the broad expanse of the lower Chehalis Valley, and none of the study blocks are located on this nearly level surface. Elevation ranges from near sea-level to 3100 feet.

Forest management practices in the area include clearcut forest harvest, planting, pre-commercial and commercial thinning, aerial fertilization, and chemical control of competing vegetation (Turner et al., 2010). Industrial timberland has almost completely replaced the historic forests (Pater et al., 1998). The landscape is currently dominated by second and third rotation stands of Douglas-fir (*Pseudotzuga menziesii*) with lesser quantities of Western hemlock (*Tsuga heterophylla*) and other species (Turner et al., 2010).

The regional climate is controlled largely by its proximity to the Pacific Ocean. In summer, high pressure in the North Pacific Ocean brings a prevailing westerly and northwesterly flow of comparatively dry, cool and stable air to the Pacific Northwest. During the fall and winter, prevailing southwesterly

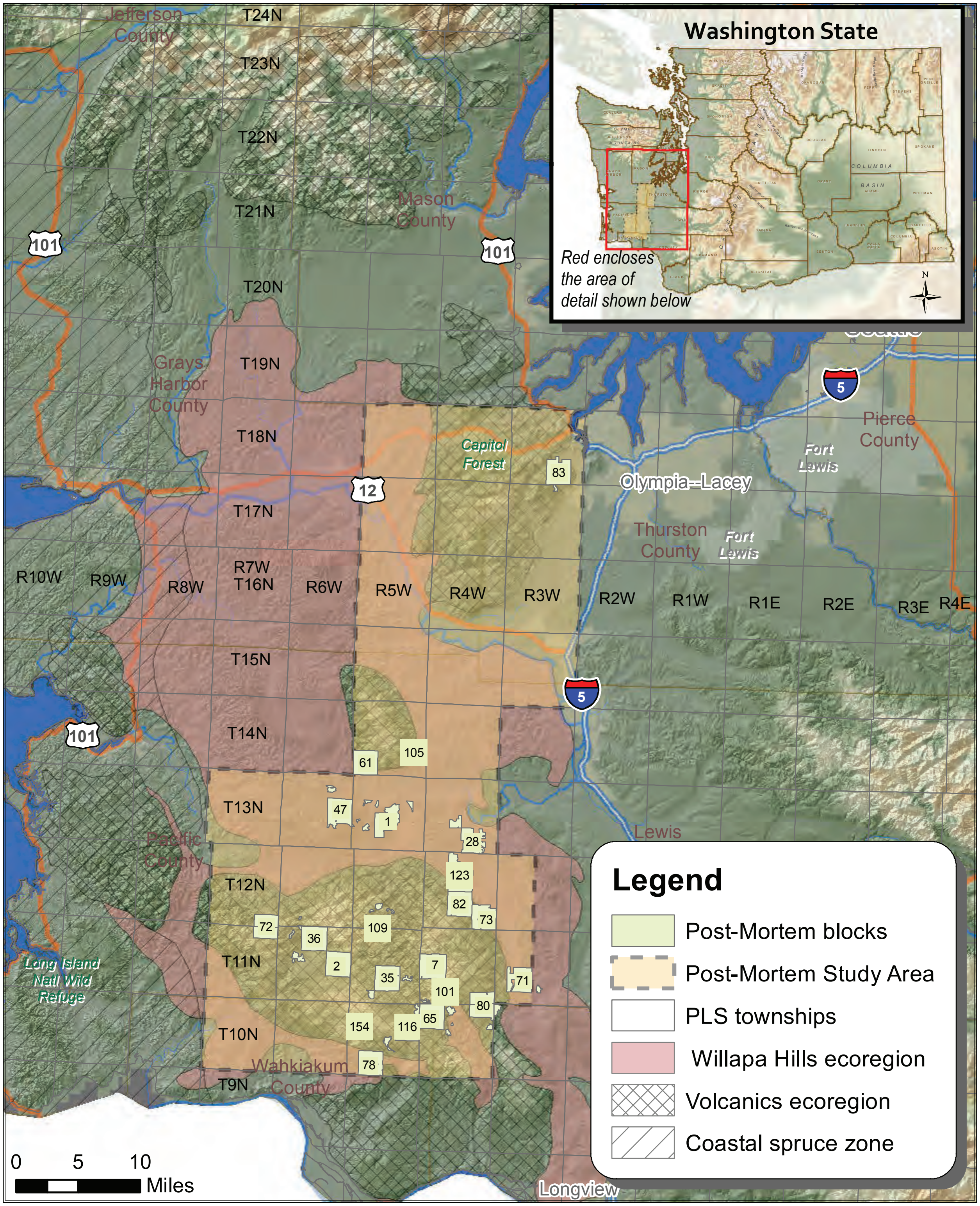

Figure 3-1: Map of the study area showing Level IV ecoregions and the Coastal Sitka Spruce zone (Frankin and Dyrness, 1973; Pater et al., 1998). Block numbers are associated with the random selection process and have no other significance.

and westerly air flow brings moist air which results in a wet season that starts in October, peaks in winter, and then gradually decreases in the spring. During the wet season, rainfall is usually of light to moderate intensity and continuous over a period of time as opposed to heavy downpours for brief periods. In Western Washington, expected maximum rainfall intensities in one out of ten years are: 0.6 to 1.0 inch in one hour; 1.0 to 2.5 inches in three hours; 1.5 to 5.0 inches in six hours; and 2.0 to 7.0 inches in 12 hours (Ruffner, 1985). The Willapa Hills form a continuous ridge from the Chehalis Valley in the north to the Columbia River in the south, which is perpendicular to the axis of flow. As a result, the area receives the full force of storms moving inland from over the ocean, and heavy precipitation and winds of gale force occur frequently during the winter (Ruffner, 1985).

3.2 December 2007 storm

The first of the three storms arrived on December 1st and delivered one to four inches of snow in the Puget lowlands (Mote, 2007). The next day, a second low pressure system moved over the Olympic Peninsula and produced wind gusts of over 80 miles per hour along much of the Washington coast, which caused extensive wind damage. Precipitation to western Washington fell primarily as rainfall. On December 3rd, the third and most significant of the three storms brought moist tropical air and intense rainfall to most of western Washington. Rainfall associated with the third day of the storm ranked among the top 10 on record at several stations, and was among the top 5 at Elma and Aberdeen (Mote et al., 2008). Although local rainfall recording stations do not have lengthy periods of record to allow meaningful ranking, they indicate that the Willapa Hills received exceptionally heavy rainfall (Mote et al., 2008). Daily totals at the four stations within the study boundaries show spatial variability and the rainfall distribution across the four-day storm period (Figure 3-2).

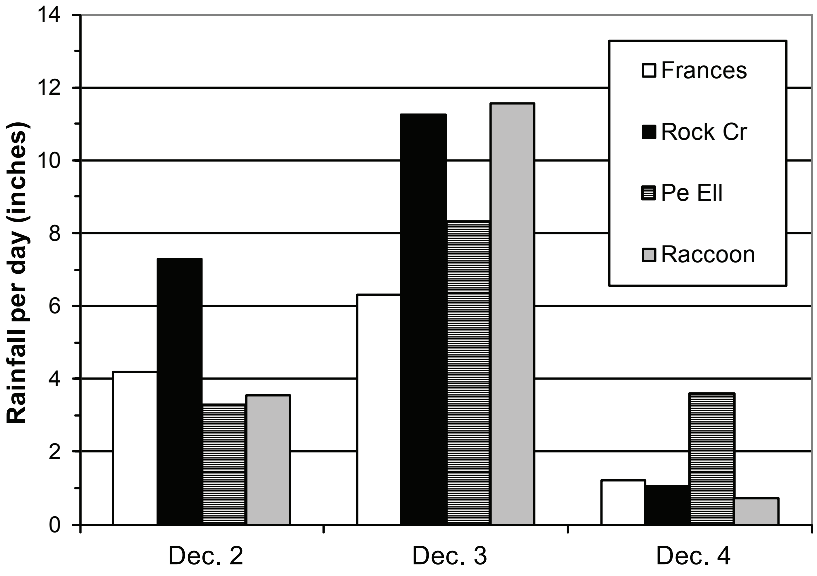

Figure 3-2: Daily (24-hour) precipitation amounts for December 2, 3 and 4, 2007 at the four weather stations in the study area that are available from the Office of Washington State Climatologist. Totals were determined at midnight except for Pe Ell, which was not specified. See Figure 5-5 for station locations.

3.3 Site selection and logistics

In February 2008, the general area for the study was finalized and contracts were signed with the Washington Department of Transportation (WDOT) for the acquisition of aerial photography. During April and May of 2008, 1:12,000 aerial photography was flown, providing stereo coverage with minimal parallax for the identification of landslides. Aerial photography was delivered to WDNR in batches in June and July of 2008.

Simultaneous to the acquisition of aerial photography, a Request for Qualifications and Quotations was issued by the WDNR. In June 2008, Matt O'Connor the principle investigator for O'Connor Environmental Inc. who is a licensed geologist in both Washington and California, was chosen to implement the project.

Post-Mortem clusters were randomly selected from public land survey sections. If a randomly selected cluster overlapped a portion of a previously selected cluster, the latter cluster was rejected and a new random selection was made. The result was a random selection of non-overlapping clusters. Randomly selected clusters were screened, and rejected if they did not meet the required minimum landslide density of four landslides per cluster as identified in the aerial photography.

In June 2008, the project manager sent letters requesting access permission to landowners in potential study clusters. The letters described the project and access requirements. The contractor was responsible for follow-up contacts and securing access to private land. A few small landowners denied access on a few, relatively small parcels, but all large landowners granted access.

3.4 Harvest treatment delineation

Delineation of harvest units and forest stands was accomplished by interpretation of 1:12,000 aerial photographs, National Agriculture Imagery Program (NAIP) 2006 orthophotos, and WDNR base map information including contours, streams, roads, and section lines. All of the landbase in a block was assigned to one of the five treatments based on a set of defining characteristics (Table 3-1 and Figure 3-3) except large areas of flat land (e.g., the Chehalis River floodplain) and non-forest land (e.g., the City of Pe Ell).

Table 3-1: Primary and secondary characteristics for the delineation of harvest treatments.

Treatment	Primary characteristic	Secondary characteristic	Critical
No Buffer	0-20 years old	No (or very limited) buffering of RIL	Yes
Partial Buffer	0-20 years old	Some (but not complete) buffering of RIL	No
Full Buffer	0-20 years old	Complete buffering of RIL	Yes
Submature	21-40 years old	n/a	No
Mature	Greater than 40 years old	n/a	Yes

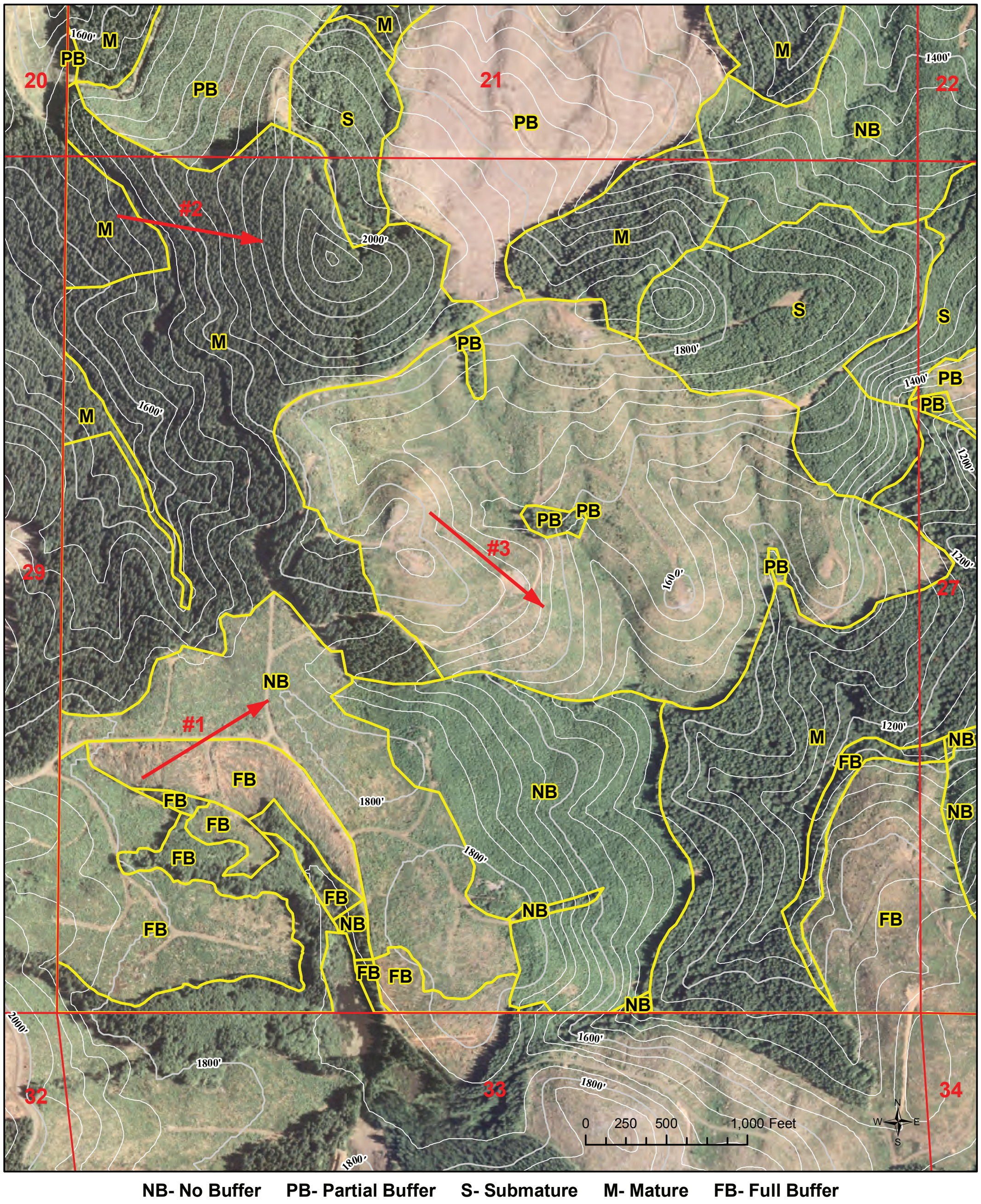

Figure 3-3: An example of harvest treatment delineation over ortho-photography and 10-m DEM topography from Cluster 82 (Township 12 N, Range 4 W, Section 28), showing the spatial scale of treatment polygons and distribution of relevant landforms. Although buffers and leave areas within 0-20-year-old harvest treatments (NB, PB and FB) are outlined, each was incorporated into the adjacent treatment polygon for analysis. In Mature and Sub-Mature polygons, streams and RIL that would be buffered are also present and were similarly included with the stand, though are not delineated on this map. Numbered red arrows point to broadly convex, undissected areas with no apparent RIL; these areas were observed within polygons of all treatments. Because photography was taken in 2006 before the Post-Mortem storm, no landslides are evident. Work was done to evaluate stand age as a covariate (Section 6.1.1).

Large landowners provided stand-age maps which confirmed the broad age categories of 0-20 years, 21-40 years, and 41+ years and helped with the polygon delineation. Where stand-age maps were not available, field crews were able to establish the treatment using tree age estimates in the field. Where harvest unit edges were not obvious, treatment boundaries were assumed to coincide with streams, roads, ownership boundaries, and ridge lines.

Harvest unit boundaries for stands older than 20 years old were inferred simply as a function of stand age, since unstable slope or riparian buffering was seldom done in that era. However, harvest units less than 20 years old typically include a mix of operational (e.g., logging access, tree merchantability), wildlife, riparian and RIL buffers, and frequently have trees of significantly different ages within their boundaries. Buffers that could be reasonably associated with a harvest unit were included in its delineation. An additional set of operational characteristics were defined to ensure consistent delineation of treatments in a complex landscape. For example, the No Buffer treatment could have limited unstable slope buffering along the lowest extent of an inner gorge on Type F water (i.e., pre-FFR riparian buffer). Also, yarding corridors created by suspension yarding through the upper canopy without the cutting of corridors (but perhaps with the cutting of a couple of safety trees) did not prevent a harvest unit from being classified as Full Buffer.

Distinguishing among the three 0-20 year harvest treatments (No Buffer, Partial Buffer, and Full Buffer) required careful evaluation for the presence of RIL and their level of buffering and these decisions were made by interpretation of aerial photography coupled with the use of 10-meter digital elevation models (DEM) and the WDNR slope stability model SlpStab (Shaw and Vaugeois, 1999). In some situations, it was difficult to verify the presence or absence of RIL using aerial photography (e.g., snow, shadows, etc.) and other tools (e.g., inherent limitations of determining exact slope gradient from 10-meter DEM which tend to underestimate the true gradient of small, steep landforms). When the remote determination was questionable, field crew were dispatched to harvest units to answer specific questions (e.g., is this concave feature really a 70% bedrock hollow?). This is consistent with Forest Practices which require field verification of potentially unstable slopes. In the first several blocks, treatment assignment was performed by the contractor's field coordinator. Later, much of the harvest treatment classification task was transferred to WDNR staff.

Although the treatments were designed to be mutually exclusive, harvest units were found that appeared to meet the definitions for either the Full Buffer or No Buffer treatments. Typically, these were harvested units with or without riparian buffers, but with no RIL and therefore no clear category of RIL buffering. Harvest units with stands between 0-20 years old (but typically less than 10 years old) without RIL were assigned to the Full Buffer treatment if they were planted after 2001 (i.e., post-FFR), and were assigned to the No Buffer treatment if they were prior to 2001 (i.e., pre-FFR – see NB polygon on Figure 3.2 at Arrow #1 which does not appear to have any RIL). The implementation decision to assign harvest units without RIL into No Buffer and Full Buffer by age of harvest is consistent with the study design because it was assumed that unstable landforms would be quasi-randomly distributed, and therefore roughly equally distributed, among the treatments after the individual harvest units and forest stands were aggregated within a block. No Buffer and Full

Table 3-2: Primary characteristics for the delineation of road treatments.

Treatment	Primary characteristic	Critical
Abandoned	Removed from use according to a formal abandonment process or equivalent	No
Mitigated	Improved for the purpose of reducing mass wasting potential	Yes
Standard	Drained and graded in accordance with Forest Practices Rules	Yes
Orphaned	Orphaned in 1974 and not used since that time.	No
Substandard	Fails to meet current road standards	Yes

Buffer, both critical treatments, represent two different eras of Forest Practices whose relative effectiveness at limiting landslide rates is what this study is evaluating.

Assuming a quasi-random distribution of RIL, individual polygons of Mature and Submature may also be devoid of RIL. Therefore, individual polygons of No Buffer, Full Buffer, Submature and Mature may not contain RIL, and we have observed significant areas within many harvest units or forest stands that do not contain RIL (Arrow #2 points to such an area within a Mature stand). By virtue of its definition, each Partial Buffer harvest unit did contain one or more RIL, but those too may contain significant areas without RIL (Arrow #3). Further discussion about the implications to study results is provided in Section 7.2.3.

3.5 Road segment treatment delineation

Road segments were assigned to one of five treatments (Table 3-2). Assigning road segments to treatments was accomplished primarily through direct observations by field crews. With abandonment, one or more of the following typically occur: road fill is removed from stream crossings; road tread, ditches and cross-drain culverts are re-graded to restore natural drainage patterns; numerous deep waterbars may cross the former road tread; and perched fill is removed. Management leading to abandonment is designed to best limit any future surface erosion or mass wasting from the road. Mitigated roads are defined by a lack of perched sidecast on steep sideslopes with delivery potential, as well as a well-designed and maintained drainage system (e.g., over-sized stream-crossing culverts) that is beyond what would be expected for a Standard road segment. Standard roads display long-term active maintenance such as grading, road surfacing, ditch maintenance, and appropriately sized, located, and maintained stream-crossing and cross-drain culverts. Standard roads may be non-driveable because of the growth of brush or younger trees, so long as the vegetation does not act to impair the drainage function. Orphaned roads have not been in use since 1974 and are typically undrivable. They may be difficult to locate because of understory vegetation and trees growing in the road prism. Substandard roads are defined by evidence of inadequate drainage such as non-functioning culverts and few or non-functional ditch relief structures.

Road treatments were designed to be mutually exclusive, and if a road segment was Abandoned, Mitigated or Orphaned, the assignment of a treatment was usually simple. However, mitigation measures such as sidecast pullback that had occurred several years prior were difficult to detect in the field. If a road segment was not clearly one of the first three types, then general road conditions and the number and effectiveness of drainage structures were assessed and the road segment was designated Standard or Substandard as appropriate.

Paper maps and GIS data were used by the field crew to locate roads mapped in the WDNR GIS database. When unmapped roads that were constructed prior to the December 2007 storm were found in the field, the road segments were mapped. Roads constructed in spring and summer of 2008 were occasionally encountered and excluded from the study because they were not subjected to the December 2007 storm.

3.6 Augmentation of critical treatments

To ensure that representative samples could be obtained, the study design provided minimum area and length criteria for the critical treatments. When sufficient area or length of a critical treatment could not be found within the cluster (e.g., initial four sections), sampling was augmented through use of the frame. The frame was defined as the 12 sections surrounding a cluster.

The overlap of frames, and the overlap of a cluster onto another's frame, was permissible; however, to avoid repeated sampling, once field data had been collected in a section of the frame, that section was no longer available for sampling to augment another cluster's critical treatments. Frame sections were numbered from 1 to 12 beginning with the northeastern corner and continuing in a counter-clockwise direction to determine the sampling order. When the frame was used, the randomly assigned cluster number was divided by 12 and the frame section with the same number as the remainder was sampled first. Using Cluster 70 as an example, 70 divided by 12 is 5 with a remainder of 10, so the frame sampling would begin with the section numbered 10 (which is southeast of the southeastern section of the cluster).

Within a given frame section, searches were conducted from the southeast corner diagonally towards the northwest corner, and the first-encountered road segment or harvest unit/timber stand was used. If a frame section did not yield sufficient additional length or acres of an under-represented treatment, the sampling of frame sections would proceed by searching frame sections sequentially in a counter-clockwise direction. Where two frames overlapped, frame use for the first randomly selected cluster had to be completed before frame use for the next randomly selected cluster could begin. In no instance was it permissible for two clusters to sample from the same frame section.

The twelve-square-mile frame was also used to augment acres removed from a four-square-mile cluster (Figure 2-2) for the following reasons: 1) a small portion of the cluster lay on the Chehalis River Floodplain or other 40+ acre area of flat land; 2) the landowner denied access; and 3) the land was not forest land (e.g., large borrow pits, agriculture, incorporated land, etc.). The removal of acres from a four-square-mile cluster happened only occasionally.

3.7 Field personnel training and data collection

Field crews conducted landslide surveys and road indices surveys, attributed road segment treatments, and reviewed preliminary harvest unit treatment determinations. Crew members each had previous training in geology, geomorphology, hydrology, forestry, and/or forest engineering; but may or may not have had previous experience with WA Forest Practices Rules. Field protocols for the landslide surveys included walking or driving all roads and walking all streams in search of all road-related landslides and all landslides that delivered to streams.

UPSAG hosted a four-day training for the field crew between June 30 and July 3, 2008. The training covered field data collection protocols and the definitions of harvest unit and road segment treatments. Ten of the sixteen field crew members attended this first training. A second training was conducted for the additional six field crew and for an observer variability team for during the first week of September 2008. Because it was possible for the new members of the crew to observe field collection methods with experienced field crew, the second training was shortened to one day in the classroom followed by time in the field working with previously trained crew members.

Field crews were provided maps for each cluster showing roads, 1:100,000-scale bedrock geology, 40-foot elevation contours, streams, stand age (where available), section lines, and probable landslides from aerial photo interpretation. Crew members were required to have in their possession when collecting data the following items: set of paper maps described above; field manual; field forms; field computer (with GPS and camera); materials for plant species identification; laser range finder; 100' tape measure; string box; DBH tape; compass; clinometer; and flagging.

Fieldwork began July 7, 2008. Field crews followed data collection procedures described in the Post-Mortem Field Manual (Phillips et al., 2008). The Post-Mortem Field Manual contains instructions for data collection methods organized into several data tables. The primary table contains data collected at every landslide surveyed. An additional data table was completed for each landslide based on landslide initiation location: 1) Hillslope (No Road), 2) Hillslope Road, or 3) Stream-Crossing Road. Field crew were instructed to identify landslides as Hillslope (No Road) if the failure initiated outside of a road prism (i.e., not within the cutslope, tread or fillslope), as Hillslope Road if the failure initiated within a road prism but not within a stream-crossing fill, and as Stream-Crossing Road if a stream-crossing fill had completely or partially failed. Table 3-3 lists the set of parameters associated with each table. The Field Manual (Phillips et al., 2008) contains a complete description of each parameter, a list of possible choices, and instructions for making a choice.

Data were collected primarily for debris flows, debris slides and debris avalanches, which are the rapidly moving earthen failures. Data were collected for dam-break floods events and deep-seated, rotational landslides only if it was apparent that the failure initiated during the storm (see Section 5.2 for definitions). If multiple landslides coalesced into one, the largest of the multiple slides was considered to include the entire length of the landslide while the smaller adjacent slide(s) ended at the point of convergence. The minimum landslide initiation volume where data were collected was five cubic yards if the initiation did not evolve into a larger event.

Table 3-3: Landslide data parameters from the Post-Mortem Field Manual (Phillips et al., 2008).
The manual is available as a supplement to this report and contains a complete description of each parameter, a list of possible choices, and instructions for making a choice.

Primary table			
Landslide ID	GPS location	Aspect	Slope form horizontal
Slope form vertical	Failure length	Failure width	Failure average depth
Failure maximum depth	Rule-identified landform	Mapped / observed geology	Harvest unit planted age (yrs)
Understory plant characteristics	Landslide process	Delivery to typed waters	Sediment and debris delivery volume (cubic feet)
Event location	Photo number	Comments	

Hillslope (no road) table			
Landslide ID	Overstory tree composition	Average tree diameter	Density
Buffered	Stand age of buffer (yrs)	Pre-storm blowdown	LWD delivery
Landform comment	Contributing factors	Photo number	Comments

Hillslope road table			
Landslide ID	Failure location	Natural ground gradient (%)	Road surface geometry
Tread condition	Ditch depth (in.)	Ditch flow	Sidecast width (ft)
Drainage	Upslope road distance	Photo number	Comments

Stream-crossing road table			
Landslide ID	Inlet stream angle	Structure type	Structure material
Structure diameter	Culvert gradient (%)	Culvert condition	Flume
Culvert blockage	Upstream bankfull width (ft)	Sediment type	Organic debris load
Stream gradient downstream (%)	Upslope road distance draining to site	Pirated water	Fill quality
Total fill depth at outlet (ft)	Failure description	Photo number	Comments

Landslide information was entered directly into electronic field forms loaded onto handheld computers equipped with GPS and GIS capabilities (Trimble Nomad XT). The handheld computers contained map layers with polygons depicting landslides identified from aerial photo interpretation, WDNR roads, WDNR streams, geology, harvest treatments, section lines and topographic contours. If the field crew was unsatisfied with the accuracy of the position recorded by the GPS device (e.g., when GPS signal coverage was poor) then the point could be moved or the location could be selected using the touch pad screen; the map layers were helpful for verifying landslide location.

Paper field forms were used if a handheld computer was not functioning properly or was unavailable. Field crews also kept a field notebook in which they entered landslide ID numbers, data forms used, and a drawing of landslides that had not been identified through the aerial photo inventory. Additional information was commonly noted, and the estimates of delivered volumes were calculated and explained. Field notes were collected and retained by the contractor.

As the field season neared completion, the contractor reviewed compiled cluster data to search for data gaps (i.e., an underrepresented treatment) and to guide completion of field surveys. If necessary, the field crew, under direction of the contractor and the field coordinator, returned to previously visited clusters to locate and survey landslides from road and harvest treatments to ensure that the minimum sample size requirements (5% minimum representation of any critical treatment in any cluster) were met. As of November 22, 2008, field data collection was completed and minimum sample size requirements were determined by the contractor and coordinator to have been met.

3.8 Quality assurance and quality control (QA/QC)

Data collected during the study went through two data Quality Assurance and Quality Control (QA/QC) reviews and an assessment of observer variability that was performed as a quality assurance exercise.

The first QA/QC review was conducted by the contractor during the period of field data collection. This review focused primarily on the identification of data entry errors. Refinement and additional data collection occurred as errors or omissions were identified. As a result of an informal UPSAG quality control check on the assignment of road segment treatments near the end of the field season, all road segments were subsequently reviewed by two of the field survey staff who received additional field training from an UPSAG member. The original road treatment determinations were revised only when demonstrably in error.

The second QA/QC exercise occurred when stand age data were made available by landowners for all harvest units in the study area.[13] At that time, stand age was added to the GIS layer for harvest treatments. All harvest treatment determinations were reviewed as new GIS line work was added to

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[13] For a small number of private landowners, stand age was estimated through allometric regression against tree height. WDNR staff estimated stand heights using BAE SOCET SET photogrammetric system software in a 3D stereo, WA State Plane NAD83/91 South Zone projection, units U.S. Survey Feet.

delineate buffers and even-age portions of harvest units. If, during the review, it was noted that the stand age was inconsistent with the treatment assignment, the treatment assignment was changed to be consistent with the stand age. If the classification for a 0-20 year old treatment appeared incorrect with respect to the buffering of RIL, a qualified expert for unstable slope identification in Washington State reviewed the unit and made the final determination as to the assignment of treatment. The original treatment determinations were not altered unless there was clear evidence of an error.

3.8.1. Quality Assurance and observer variability

A third QA exercise was conducted to assess observer variability among the field crew. This assessment was carried out by an observer variability team composed primarily of WDNR staff (Miskovic and Powell, 2009). In this third exercise, two or more members of the team visited landslide initiation sites that had previously been visited by the field crews. At each landslide site, the team made independent measurements and evaluated the field crew determinations. Results were not used to modify the Post-Mortem dataset, but serve as an independent assessment of observer variability. Key findings from this exercise are discussed in Section 5.1, and a full copy of the Miskovic and Powell (2009) report is available as a supplement to this document.

SECTION 4: DATA ANALYSIS

This section describes the data analysis techniques employed in the study including the software used, data quality control procedures, and the choice of statistical models.

4.1 Software

Data analysis was conducted with ArcGIS 9.3, Access 2007, Excel 2007, JMP 8.0.2.2, SAS 9.2 and the open source software package R (version 2.11.1, R Development Core Team, 2008) with the following packages: agricolae (Mendiburu, 2010), gplots (Warnes et al., 2009), lme4 (Bates and Maechler, 2010), multcomp (Hothorn, et al., 2008), pscl (Jackman, 2010; Zeileis et al., 2008), and SuppDists (Wheeler, 2009).

4.2 Analysis of harvest landslide count by treatment

The statistical analysis used to examine differences in landslide count among treatments was conducted using a Generalized Linear Mixed Model (GLMM). GLMM combine the properties of two widely used statistical frameworks: 1) linear mixed models, which incorporate random effects; and 2) generalized linear models, which handle non-normal data through the use of link functions from the exponential family. GLMM are the best tool for analyzing non-normal data that involve random effects, including count observations (which may be non-normally distributed) collected from experimental units within randomly selected blocks, where some or all of the observations may be correlated (Bolker et al., 2009; SAS Institute Inc., 2006).

Landslide counts were modeled with a Poisson distribution and log link and a model of the form:

$$\mathrm{Ln}(E(y_{ij}|\alpha)) = \mu + \alpha_i + \beta_j + \theta A_{ij} + \gamma X_{ij} \tag{1}$$

where: y_{ij} is the log (Ln) of the expected (E) landslide count for each block (i) and treatment (j),
μ is the grand mean for all blocks and treatments,
α_i is the random effect for each block,
β_j is the fixed effect of for each treatment,
A_{ij} is a variable representing area or exposure in each cell (θ is fixed at 1), and
γ is the regression coefficient for the fixed effect of an auxiliary variable (X) across treatments and blocks.

In this model, cell treatment responses are the additive result of treatment, block, and auxiliary variable effects and cell values are calculated at the scale of treatments within blocks (Appendix A.2 provides the justification for block-scale pooling). Area is used as a weighting factor since the variance in density (count per unit area) increases as the area becomes small.

Auxiliary variables were included in a series of models that were evaluated based on Akaike's information criterion (AIC) scores produced through likelihood ratio tests. Modeling was performed with the glmer function in the lme4 package of R. Glmer is a procedure for fitting GLMM and it produces fitted models that can be analyzed using ANOVA. Models producing the lowest AIC scores are preferred.

Final model fitting was performed with the SAS GLIMMIX procedure in SAS 9.2. Model parameters were estimated using Gauss-Hermite quadrature estimation, which is more accurate than other estimation methods and is appropriate for data limited to 2-3 random effects (Bolker et al., 2008). P-values for comparisons among treatments were adjusted using Tukey-Kramer step-down adjustments. Tukey-Kramer is appropriate when data are unbalanced and the step-down procedure increases the power of multiple comparisons using a Holm's adjustment (SAS Institute Inc., 2009). The method employs the Royen (1989) extension in such a way that the resulting p-values are conservative.

4.3 Analysis of road landslide count by treatment

Road landslide counts are analyzed using the non-parametric Friedman test from the agicolae package in R. The Friedman test is a rank-sum method for analyzing unreplicated complete block designs (i.e., there is exactly one observation for each combination of treatment and block) where the distributional assumptions of parametric statistics cannot be met. The Friedman test is an extension of the sign test and only requires that blocks are mutually independent. The null hypothesis is that the ranking of the random variable within each block is equally likely indicating that the treatments have identical effects (Conover, 1980). The Friedman test does not allow for factors other than block (i.e., no covariates), but early analyses conducted with linear models and log-transformed road landslide densities failed to identify important covariates.[14]

4.4 Analysis of road and harvest unit landslide density and sediment yield

A number of previous studies have compared landslide density and sediment yield per unit area for roads and harvest units independently. In this study, harvest unit polygons include the area occupied by roads. Where road and harvest unit densities are reported together (e.g., Section 5.4), they are based on landslides, roads, and harvest unit delineations in the core of each block. Within the core area, road area is subtracted from harvest unit area for the purpose of estimating density and sediment yield per unit area.[15] These density estimates are described in tables and figures as core area densities.

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[14] Two different slope parameters were evaluated as predictors for road landslides: 1) 10-meter DEM slope extracted along road lines, 2) 10-m DEM slope extracted using a 5-meter offset from the road center-line and extending an additional 5 meters onto the hillslope. Preliminary analysis based on an ANCOVA model with road landslide density as response and block, treatment, and slope metric as predictors; indicated that neither slope parameter was significant and that both should be dropped from the road landslide model.

[15] As described in Section 3, critical road and harvest treatments were sometimes augmented by using areas of the frame that surround the core of each cluster. By restricting the analysis to the core of each block, it becomes possible to estimate the area occupied by road corridors and subtract it from harvest unit area calculations. Since road area was not measured as part of the study, a fixed road width of 60 feet was used to estimate the size of the road corridor, which is similar to the 20 meters used by Swanson and Dyrness (1974) and Guthrie (2002) for estimating road area.

4.5 Analysis of landslide size

Landslide size at initiation was calculated as the product of the length, width, and averaged depth of the landslide initiation area. If a landslide delivered to a public resource (e.g., typed water), field crews estimated the volume of sediment delivered. Delivered volumes were estimated as the initial volume plus the volume of observed scour minus the volume of observed deposition along the landslide track (this is explained more fully in Section 5.2). Sediment volume is sometimes reported in terms of mass density (i.e., tons per hectare), and an early reviewer of this study requested that all landslide volumes be presented in those terms.

4.5.1. Unit conversion to mass density

The conversion from volume to mass density requires an estimate of soil bulk density and area. Soil bulk density was not measured as part of this study so a single bulk density value of 125 pounds per cubic foot was used for all conversions. This value was chosen because Montgomery et al., (1998) reported 125 pounds per cubic foot for a site in coastal Oregon; and Shaw and Vaugeois (1999) used this value for all bulk densities in the WDNR SlpStab model. As a result of using a single bulk density, estimates of landslide volume and mass density are directly proportional.

4.5.2. Analysis of harvest landslide mass density by treatment

Mass density does not meet the distributional assumption of Poisson or Gaussian regression, so values were normalized by fitting a Box-Cox transformation using the PROC TRANSREG procedure in SAS. Treatment was included as a class variable in the analysis. The TRANSREG procedure tries a range of power parameters and maximum likelihood to pick the parameter (λ) that provides the best transformation. Given the transformation parmater λ, original values are transformed such that:

$$y_{\text{transformed}}=(y_{\text{original}}{}^{\lambda}-1)/\ \lambda) \tag{2}$$

Box-Cox transformations require that all data be non-zero, so 1xE-6 was added to all original mass densities prior to analysis. Power parameters (λ) that were tested ranged from -2 to 2 in increments of 0.01.

Using Box-Cox transformed values, differences in mass density by treatment were evaluated with a GLMM of the form:

$$(y_{ij}|\alpha) = \mu + \alpha_i + \beta_j + \gamma X_{ij} \tag{3}$$

where: y_{ij} is the expected transformed mass density for each block (i) and treatment (j),
μ is the grand mean for all blocks and treatments,
α_i is the random effect for each block,
β_j is the fixed effect for each treatment, and
γ is the regression coefficient for the fixed effect of a auxiliary variable (X_{ij}) across treatments and blocks.

Model parameters were calculated with SAS GLIMMIX using Gauss-Hermite quadrature estimation. The distribution was specified as Gaussian with an identity link. Multiple-comparison tests were conducted with conservative stepdown Tukey-Kramer adjustments.

4.5.3. Analysis of road landslide mass density by treatment

Differences in road landslide mass density were evaluated using the non-parametric Friedman test in the agicolae package in R. The Friedman test is a rank-sum method for analyzing unreplicated complete block designs where the distributional assumptions of parametric statistics cannot be met. The null hypothesis is that the ranking of the random variable within each block is equally likely.

4.6 Pooling auxiliary variables at the scale of blocks

The analysis of landslide counts allows for the inclusion of covariates calculated at the scale of treatments within blocks. For landslide counts, pooling is performed by summing over all experimental units in the same treatment group and block. For auxiliary variables, block-scale values are calculated as an area-weighted mean:

$$X_{ij} = \frac{\left(X_{ijk} \cdot area_{ijk}\right)}{\sum_k area_{ijk}} \tag{4}$$

where k is number of experimental units within a given block and treatment.

4.7 Other analysis

Other analyses were conducted using the JMP and R software packages. Most graphs were produced with R and exploratory multivariate regression were largely performed with JMP.

SECTION 5: DESCRIPTIVE RESULTS

This section of the report includes descriptions of landslide and landscape attributes collected from the 22 randomly selected blocks in the Willapa Hills province in southwest Washington. As noted earlier, field data collection efforts were focused on identifying all road-related landslides and all hillslope landslides that delivered to WDNR typed waters. Hillslope landslides that did not deliver were surveyed opportunistically and therefore results that include hillslope landslides that did not deliver may have an unknown amount of bias associated. Descriptions of data parameters from which no inference can be drawn are relegated to presented in Appendix A. Although this section characterizes the landslides and landslide processes in this study, it does not contain the statistical analyses, which are covered in Section 6.

Within the study area, over 58,000 acres (91 square miles) of forested uplands and 555 miles of forest roads were field surveyed for landslides and assigned to one of five harvest or road treatments, respectively (see Section 2.2). Field crews located 1,133 hillslope landslides, 938 of which delivered to public resources, and 347 road-related landslides, 209 of which delivered to public resources (Table 5-1).[16] The majority of road-related landslides (289 of the 347) were characterized as "hillslope road" which means they were not associated with stream-crossings; almost half of these did not deliver to public resources. Most of the stream-crossing road landslides (59 of 67) were reported to have delivered to a public resource. The eight stream-crossing road landslides reported as not delivering may be because the streams are untyped water,[17] or may simply be errors on the part of the field crew.

Table 5-1: Number of landslides by event location and whether it delivered to a public resource.

Landslide event location	Delivery	
	No	Yes
Hillslope (No-Road)	194	938
Hillslope road	129	150
Stream-crossing road	8*	59

Notes: Field crews failed to note delivery status for one hillslope and one road-related landslide so those landslides are not incorporated in this table.

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[16] Public resources in this context generally refers to waters typed by WDNR.

[17] Small, non-fish-bearing streams that do not connect to the rest of the channel network are classified as "untyped" and are not considered public resources under Forest Practices Rules.

5.1 Observer variability

In order to evaluate the accuracy and consistency of the data collected by the field crews, the WDNR provided an 'observer variability team' with experience in unstable slopes and Forest Practices Rules to independently evaluate field crew data calls. Members of the observer variability team (typically a pair) visited approximately 10% of the landslides that had been surveyed by the field crews. At each site, the observer variability team compared their responses with the field crew responses and decided whether the field crew assessments were reasonably in agreement with theirs. 'Agreement' was tallied where the observer variability team's observations were the same or sufficiently similar to the original field crew observations. The report that summarizes their findings, Miskovic and Powell (2009), also defines the methods for determining agreement for specific parameters. Results were not used to alter the data collected by field crews because there was no determination of which group was in error and disagreement may result from a high degree of subjectivity in the parameter itself. The observer variability metrics that relate to results presented in this report are included in Table 5.2 and some particularly important metrics are briefly discussed here.

The observer variability team agreed with the assessments of the field crews the majority of the time, but certain parameters were subject to greater disagreement than others (Table 5-2). For example, the observer variability team found that gradient at the initiation site was difficult to measure consistently. They report that the average absolute difference between the two groups was 11.5%, with the observer variability team generally reporting higher gradients on shallow slopes, and lower gradients on steeper slopes than the field crew (Figure 5-1). There are several reasons why the two groups might record different slopes. First, it is impossible to measure the slope gradient of interest (i.e. , the

Table 5-2: Percent agreement between field crews and observer variability team for a select set of Post-Mortem parameters.

Parameter	Landslides examined (N)	Agreement (%)	Parameter	Landslides examined (N)	Agreement (%)
Event location	144	99%	Delivery to typed waters	129	90%
Landslide process	144	94%	Gradient at failure site	144	88%
Scarp length (ft)	144	94%	Scarp width (ft)	144	92%
Average depth (ft)	144	88%	Delivered sediment volume	144	77%
Contributing factors	143	97%	Initiation in a RIL	144	85%
Harvest unit age	144	77%	Buffer presence	110	77%
LWD delivery*	108	85%			

Source: Miscovic and Powell, 2009

* The observer variability team report that they had low confidence in their assessment of this metric, citing differences in the interpretation of Large Wood Debris (LWD) as the cause.

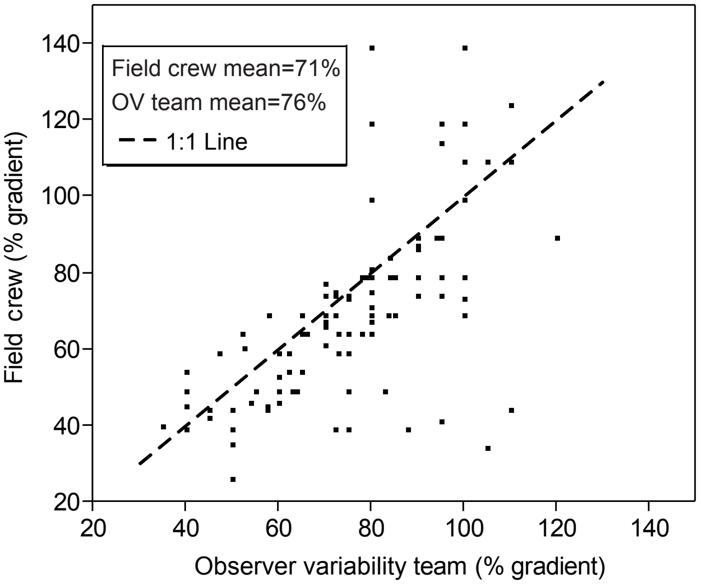

Figure 5-1: Gradient at initiation site for the observer variability team vs. the field crew. Points below the 1:1 line indicate landslides where the observer variability team recorded steeper slopes than the field crew, while points above the line show where the field crew recorded steeper slopes.

steepness of the slope before the landslide) at a site that has already failed. As shown in Figure 5-2, the gradient of the hillslope adjacent to the landslide was measured because it is the best approximation of the pre-landslide slope gradient. The observer variability team discovered that even this seemingly straightforward measurement of gradient adjacent to the landslide proved difficult to reproduce under certain field conditions (Miskovic and Powell, 2009). With the non-uniform gradients of natural hillslopes, gradient measurements vary depending on exactly where the observer is located. On average, the observer variability team reported gradients that were 5% steeper than the gradients reported by the field crews on average (Miskovic and Powell, 2009).

The observer variability team reported they disagreed with 15% of the call as to whether a landslide initiated in a RIL. In most cases where there was disagreement, the observer variability team identified the landslide as having originated in a RIL where the field team did not (Miskovic and Powell, 2009). Given the importance of slope gradient in RIL definition, it appears likely that many of the differences in RIL determination between the field crew and observer variability team may have been predicated on their differences in slope measurement.

The observer variability team found that they generally agreed with measurements of landslide scarp width and length, though irregularly shaped landslide scarps resulted in some disagreement between the two groups. The observer variability team found that maximum depth calls were fairly easy to agree with, but that there was greater disagreement on mean depth. Where there was a disagreement on mean depth, the field team generally recorded slightly deeper values than the observer variability team (Miskovic and Powell, 2009).

Figure 5-2: Example of measuring hillslope gradient at the failure initiation site (Photo: Julie Dieu)
As shown in this photo, field crew were trained to take a hillslope gradient measurement adjacent to the landslide initiation site that best approximated the hillslope gradient at the failure site.

While the observer variability team often agreed with field crews on initial headscarp dimensions, there was much more subjectivity in determining the total amount of sediment delivered by a landslide. The volume of sediment delivered to a stream is based on estimates of initial headscarp volume (product of length, width and average depth), minus deposition, plus scour. Each measurement requires some level of judgment by the observer and differences are compounded over the length of the runout. The observer variability team found that the field crew assessments were reasonable 77% of the time, which they felt was surprisingly high, given the number of factors and the variability in the estimate of each one. They note that differences, where present, were associated with either: 1) the field crew's failure to include scour associated with a debris flow; or 2) different determinations of delivery to a WDNR typed stream (Miskovic and Powell, 2009). An additional observer variability exercise, described in Appendix A3, compared delivered sediment volume estimates from 10 independent observers from the field crew and LiDAR cut-and-fill estimates for 9 large landslides. In this exercise, individual estimates of delivered volume varied greatly – by up to two orders of magnitude in one case (average coefficient of variation of 0.84). However, these landslides were among the largest landslides in the study, and would be expected to be among the most complex to measure.

The observer variability team agreed with 77% of the field crew calls on buffer presence. They note in the report that the instructions for data collection on this metric proved problematic for both the field crews and the observer variability team. The primary source of the problem appeared to be how to treat stands greater than 20 years old. According to the Field Manual, these stands should have had buffer recorded as 'Null' meaning that buffers do not exist in older forest stands and the question cannot be answered, but in 20% of the cases the field crews reported that there was 'no buffer.' When the analysis was restricted to stands less than or equal to 20 years old, there was 96% agreement (Miskovic and Powell, 2009).

5.2 Landslide process, volumes and sediment delivery

Field crews were asked to determine the landslide process that best described all or most of the length of the landslide, its initial size, and the volume of sediment that delivered to public resources. For guidance on determining landslide process, the field manual provided detailed descriptions (Phillips et al., 2008). Table 5-3 includes a summary of landslide process definitions. Field crews were allowed to pick 'Other' if the landslide did not match any of the provided descriptions. With respect to volume, crews were asked to record initial scarp dimensions (Table 5-4). In addition, they were asked to calculate delivered sediment volume which is the product of the initial dimensions (i.e., the initial failure volume) plus any observed channel scour as the landslide traveled downslope minus any observed deposition on the hillslope or floodplain (Table 5-5). The delivered sediment volume is the sediment that the landslide transported to the channel where the landslide stopped; this is important to understand because researchers who study landslides or conduct sediment budgets view delivery in a variety of ways. For this study, delivered volume includes sediment that had already reached a small stream channel (through a variety of processes) and then was remobilized as a debris flow scoured down the small stream channel and "delivered" to the lower gradient channel network. The calculations of delivered sediment do not include any component of fluvial entrainment or transport after the landslide process was finished.

Table 5-3: A summary of the landslide processes described in the Post-Mortem Field Manual (Phillips et al., 2008).

Landslide process	Definition
Debris slide	Aggregations of coarse soil, rock, and vegetation that lack significant water and move at speeds ranging from very slow to rapid down slope by sliding or rolling forward. Debris slides typically travel short distances and tend to form hummocky, poorly sorted deposits.
Debris flow	Rapid flow of slurries composed of sediment, water, vegetation, and other debris. Debris flows typically initiate on steep, saturated slopes and travel down convergent channelized pathways (Sidle and Ochiai, 2006).
Debris avalanche	Partially or fully saturated rapid landslides similar in process and material to debris flows but not channelized over most of their length. They tend to behave morphologically similar to snow avalanches that splay across the slope (Hungr et al., 2001).
Deep-seated landslide	Typically large rotational slides that occur because of weakness or changes in bedrock geology or mechanical properties of unconsolidated materials. The slide plane of deep-seated landslides is generally well below the maximum rooting depth of forest trees.
Dam-break flood	Catastrophic flood events formed predominately of water. Most often, they are secondary events that initiate after a landslide deposit dams a confined but low gradient channel and a pond builds up behind the dam. They also can be triggered by the breaching of small manmade dams, beaver dams and road stream-crossing fills.

Table 5-4: Landslide initiation dimensions and slope gradients as a function of event location and delivery status.

Location			Gradient (%)*	Length (ft)			Width (ft)			Ave. Depth† (ft)		
Delivery	Process	N	Mean	Min	Mean	Max	Min	Mean	Max	Min	Mean	Max
Hillslope												
No	Debris avalanche	16	72	14	89.7	315	10	31.0	120	1	4.1	15
	Debris flow	38	71	15	81.6	600	6	31.0	110	1	3.7	8
	Debris slide	128	63	3	63.3	900	5	41.4	200	1	3.7	20
	Deep-seated	7	50	5	48.1	207	24	70.3	123	1	3.6	6
	Other	2	53	10	27.5	45	38	39.0	40	4	4.0	4
Yes	Debris avalanche	22	68	15	118.2	340	20	91.7	280	2	7.3	20
	Debris flow	374	68	7	127.5	3170	5	45.7	300	1	6.0	120
	Debris slide	520	78	2	60.7	1824	8	53.4	600	1	4.8	50
	Deep-seated	3	57	60	120.0	150	100	150.0	200	8	16.0	20
	Other	5	84	5	27.0	80	30	92.0	160	2	3.8	5
Hillslope road												
No	Debris avalanche	8	72	20	71.8	180	15	73.8	200	2	6.5	22
	Debris flow	27	76	10	78.0	660	11	37.1	90	1	4.7	12
	Debris slide	85	75	3	39.1	130	8	40.0	250	1	4.1	12
	Deep-seated	3	48	25	46.0	80	80	96.7	120	4	8.0	12
	Other	6	100	12	58.5	200	10	63.2	150	1	3.7	10
Yes	Debris avalanche	5	73	25	76.4	159	60	297.8	730	3	12.2	20
	Debris flow	73	73	12	85.2	550	7	61.0	210	1	9.9	40
	Debris slide	68	74	12	63.8	230	12	63.5	600	1	5.9	25
	Deep-seated	1	35	21	21.0	21	42	42.0	42	5	5.0	5
	Other	1	73	34	34.0	34	10	10.0	10	4	4.0	4
Stream-crossing road												
No	Debris avalanche	1	50	85	85	85	30	30	30	5	5	5
	Debris flow	3	63	15	101	180	24	31.7	36	3	5	6
	Debris slide	4	59	39	61.5	93	27	31.5	39	4	11	15
Yes	Dam break flood	3	31	36	86.3	123	50	62.7	81	20	30.3	46
	Debris flow	34	62	20	104.9	1100	10	45.6	105	2	11.8	30
	Debris slide	20	62	10	41.3	196	15	41.65	120	2	9.5	30
	Deep-seated	1	25	63	63	63	69	69	69	8	8	8
	Other	1	90	15	15	15	54	54	54	5	5	5

Notes: Field crews failed to record a landslide process for 21 landslides (16 of which were determined to have delivered to public resources), so those are not incorporated into the count. Surveyed deep-seated landslides were those that appeared to be new initiations, not previously existing features.

*Field estimate of natural hillslope gradient at initiation site.

†Average depth of the landslide scarp used to calculate initial failure volume.

Table 5-5: Landslide count and initial failure volume by whether it delivered to a public resource, and proportion of initial sediment volume that delivered to public resources.

		Delivery				
		No		Yes		Percentage of initial sediment volume that delivered
Event Location	Landslide process	Count (N)	Mean Initial Volume (yd^3)	Count (N)	Mean Initial Volume (yd^3)	
Hillslope (No road)	Debris avalanche	16	746	22	5891	71%
“	Debris flow	38	398	374	4724	101%
“	Debris slide	128	576	520	2081	60%
“	Deep-seated	7	978	3	13556	4%
“	Other	2	156	5	249	71%
Hillslope road	Debris avalanche	8	4230	5	8369	92%
“	Debris flow	27	659	73	4815	117%
“	Debris slide	85	315	68	1219	115%
“	Deep-seated	3	1450	1	163	5%
“	Other	6	1926	1	50	66%
Stream-crossing road	Dam break flood	0	—	3	7526	215%
	Debris avalanche	1	472	0	—	—
“	Debris flow	3	548	34	3667	90%
“	Debris slide	4	770	20	861	103%
“	Deep-seated	0	—	1	1288	100%
“	Other	0	—	1	150	32%

Notes: Field crews failed to record landslide process for 21 landslides so those are not incorporated into the table. Surveyed deep-seated landslides were those that appeared to be new initiations, not previously existing features.

The most common landslide process across the study area was debris slide, followed by debris flow. These two processes accounted for 96.3% of the landslides that delivered to public resources. Debris flows, which account for only 42% of delivering landslides, are estimated to have delivered 2.3 million out of a total of 3.2 million cubic yards (71%) of sediment to public resources in the study area.[18] Debris avalanches, debris flows, and debris slides that delivered to public resources tended to be larger than those that did not deliver. This is not surprising because runout distance increases with the size of a landslide (Rickenmann, 1999), and large landslides which travel further are more likely to ultimately intersect a stream or other public resource.

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[18] As a point of comparison, the total initial failure volume for landslides that delivered to public resources, excluding deep-seated and dam break floods, was 3.6 million cubic yards.

The percentage of initial sediment volume that delivered to public resources was related to landslide type in predictable ways as well. Debris avalanches are shallow flows that, by definition, lack confinement. Without confinement, sediment splays over the hillside, thereby dissipating momentum and reducing the volume of sediment delivered to the channel downstream. In contrast, the data provided by the field crews indicates that channelized dam break floods and debris flows delivered more than the initial volume. As debris flows move through the first and second-order channel network, they can significantly increase in volume through scour of the channel bed (Benda and Cundy, 1990). Since debris flows grow as they move through the channel network, total volume of sediment delivered to the low gradient channel network is larger than the initial volume and may be strongly correlated with runout distance (May, 2002). The relatively small volumes attributed to debris flows may be a function of the topography of the Willapa Hills, where hillslope length rarely exceeds 1,000 feet resulting in short scour paths to low gradient, relatively unconfined channels where deposition occurs.

5.3 Landslide density and sediment yield per unit area

The delivering road landslide density for active roads in core areas was 32.7 landslides per square mile of road corridor, compared with 11.2 delivering landslides per square mile for managed forests (Table 5-9). This is equivalent to a landslide density ratio of approximately 3:1 between road corridors and managed forest, and is significantly smaller than the order of magnitude difference reported in most studies (Sidle and Ochai, 2006). Although roads have higher landslide density per affected area than managed forests, the majority of landslides that delivered to streams (81%) were not from roads. This is because road corridors occupy a much smaller proportion of the landscape in the study area than managed forests (7% and 93% respectively).

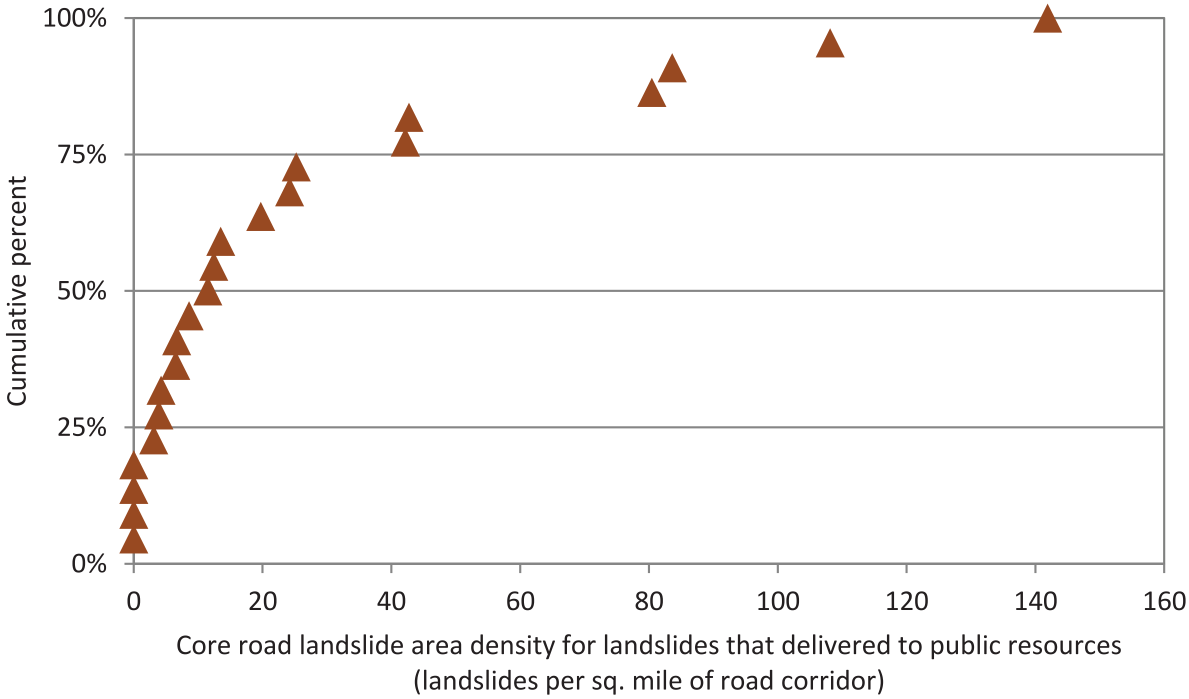
Landslide density varies spatially as a function of rainfall intensity, topography, and other factors affecting landslide occurrence. Figure 5-3 shows the distribution of landslide densities in core areas for roads and hillslopes, respectively. We see that the distribution is not uniform for either, but that road landslide density is more variable than hillslope landslide density. With road landslides, densities range from zero to142 delivering landslides per sq. mile and four of the blocks have much higher landslide densities than the rest. By contrast, the distribution of landslide density on hillslopes has a smaller range and increases fairly linearly from 1.1 to 22.9 delivering landslides per square mile. As illustrated by the bar chart shown in Figure 5-4, the blocks that exhibited the highest density of hillslope landslides also had a high density of road landslides, though the range of road landslide densities was more extreme.

For analyses other than the core area analysis, harvest unit areas include road areas and it is assumed that this has a negligible effect on estimated densities. As shown in Table 5-10, the area occupied by roads is relatively small and there is a similar road density among the harvest treatments.

Table 5-6: Core area analysis of landslide density and sediment yield for landslides that delivered to public resources.

Metrics	Active road	Inactive road	Harvest < 20 y.o.*	Harvest > 20 y.o.*	Roads	Hillslope
Count of delivering landslides	164	44	422	446	208	868
Sediment delivered (million yd^3)	14.6	4.9	15.1	51.2	19.5	66.3
Study area (mi^2)*	5.0	0.8	36.2	41.5	5.8	77.7
Landslide density (#/mi^2)	32.7	52.9	11.7	10.8	35.6	11.2
Delivered sediment (tons/acre)	284	577	41	121	326	83

Note: Active roads include Standard, Substandard, and Mitigated; Inactive roads includes Orphaned and Abandoned; Harvest less than 20 y.o. includes Full Buffer, Partial Buffer, and No Buffer; and Harvest > 20 y.o. includes Submature and Mature treatments.



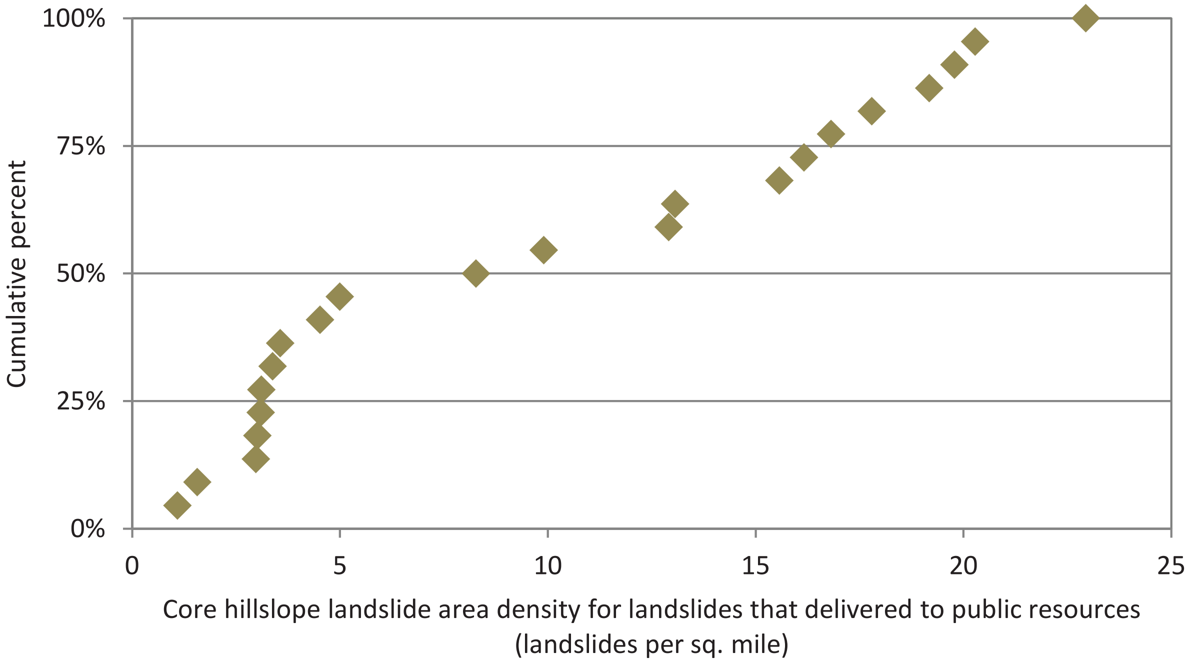


Figure 5-3: Cumulative distribution of landslide area density by block (n=22) for landslides that delivered to public resources as a function of whether the initiation point was associated with a road (top) or not (bottom). Symbol labels indicate the random block associated with each density estimate.

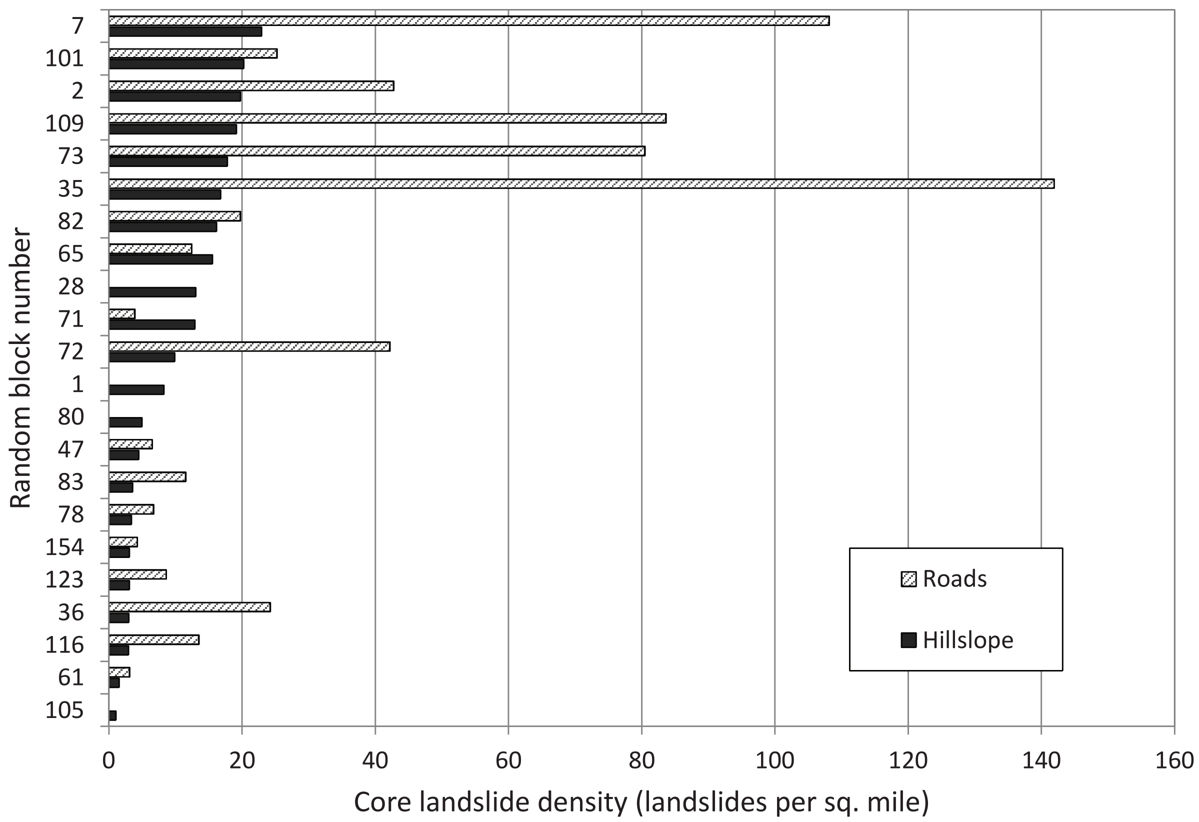


Figure 5-4: Core area landslide density (delivered) for road and hillslope landslides by block. Block numbers have no particular significance outside of the random draw.

Table 5-7: Proportion of each harvest treatment occupied by road treatments in the core area.

Road treatment	No Buffer	Partial Buffer	Full Buffer	SubMature	Mature
Not-road	92.7%	92.3%	92.1%	91.6%	95.5%
Substandard	0.61%	0.22%	0.55%	1.1%	0.10%
Orphaned	0.43%	0.59%	0.64%	0.74%	0.51%
Standard	0.14%	0.58%	0.25%	0.51%	0.72%
Mitigated	4.8%	5.6%	6.0%	4.1%	2.5%
Abandoned	1.3%	0.69%	0.48%	1.9%	0.74%
Total	100%	100%	100%	100%	100%

5.3.1. Relationship between landslide density and precipitation intensity

Landslides commonly occur in response to high-intensity rainstorms and/or snowmelt events that release large volumes of water over a period of days, particularly when relatively heavy rainfall has occurred during the preceding weeks (Campbell, 1975; Starkel, 1979; Caine, 1980; Dai and Lee; 2001; Rahardjo et al., 2001; Jakob and Weatherly, 2003; Godt et al., 2006; Jakob et al., 2006; Crosta and Frattini, 2008; He and Beighley, 2008; Tsai, 2008). Slope stability is substantially reduced when the soil moisture content is at or near saturation because of the added weight of water and the hydrostatic forces within the saturated soil mass reduce frictional resistance of particles to downslope movement (Iverson, 2000).

Figure 5-5 shows of the location of each sample block within the study area along with the total landslide count and density. These are shown against the backdrop of estimated 24-hour precipitation intensity interpolated from rain gage stations.[19] Precipitation data were measured at public and private weather stations in the study area vicinity that documented the central area of peak rainfall. The 24-hour values used are also reflective of differences in four-day storm totals, as precipitation totals for the two durations are closely correlated among stations ($r^2=0.84$). The interpolated precipitation map is informed by 12 stations, five of which were within the study area. Although the spatial density of available precipitation monitoring stations is relatively good for unpopulated forest lands, interpolated rainfall amounts cannot be expected to precisely reflect the actual precipitation at individual sample blocks or experimental units within blocks. Despite this, they provide an estimate of the spatial variation in precipitation intensity within the study area.

As might be expected, blocks with the highest landslide densities are near the zones of highest estimated precipitation, and landslide density is correlated with maximum daily precipitation (Figure 5-6).[20] The shape of precipitation isohyets (precipitation contour lines) are strongly influenced by the spatial distribution of the precipitation measurement stations, especially where strong gradients between stations exist (Minder et al., 2009). Based on observed landslide density, it is possible that the actual zone of maximum precipitation was located somewhat southeast of what is indicated by the mapped isohyets, and if so, the correlation coefficient would be greater.

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[19] Precipitation gage data for the 2007 storm event were obtained through the Office of the Washington State Climatologist. http://www.climate.washington.edu/events/dec2007floods. Interpolation was performed using natural neighbor method in ArcGIS 9.3. The method finds the closest subset of input samples to a query point and applies weights to them based on proportionate areas in order to interpolate a value.

[20] In the Post-Mortem study area, the required landslide density of approximately one per square mile as observed on aerial photos roughly coincided with four inches of precipitation in 24-hr as is apparent on Figure 5-5. For context, on the west coast of Vancouver Island, Jakob and Weatherly (2003) noted a significant increase in landslide rates when the 24-hr precipitation equals or exceeds 100 mm (approximately 4 inches).

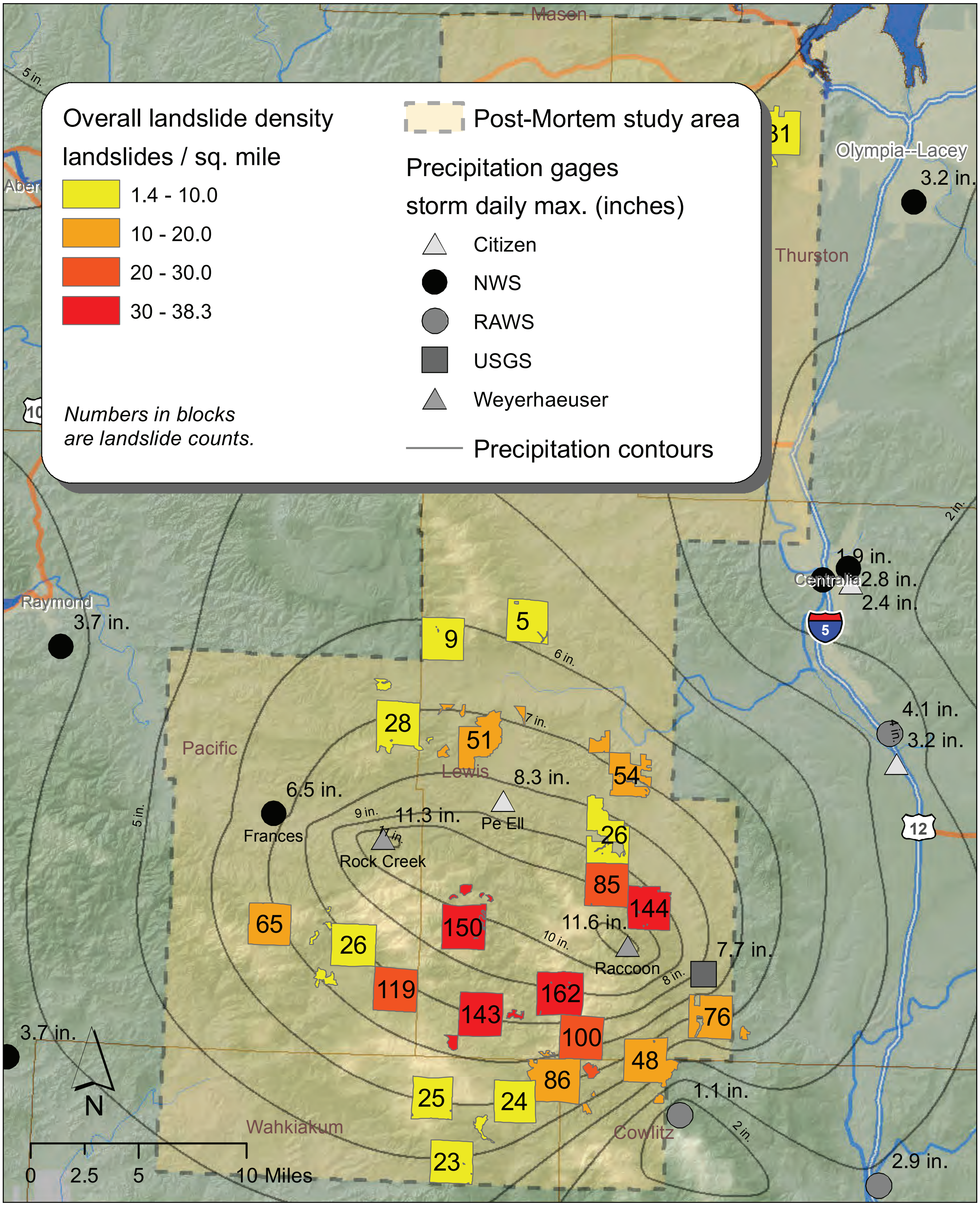

Figure 5-5: Landslide density and count using all landslides identified in the study. Colors denote landslide density while the number within each block indicates the landslide count. Precipitation contours are based on a nearest neighbor interpolation of gage station readings.

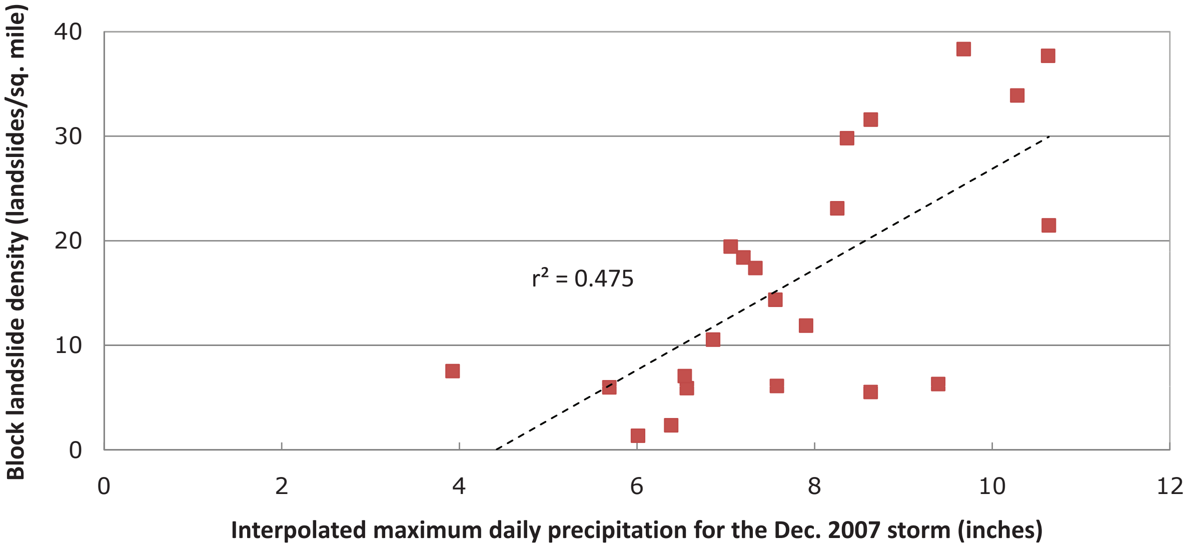

Figure 5-6: Landslide density based on all identified landslides as a function of estimated maximum daily precipitation for the December 2007 storm event.

5.4 Contributing factors

Field crews were asked to look for and identify site-scale management-related factors that may have contributed to landslide occurrence. These contributing factors did not include the basic forest practices activities that were used to define the treatments (road building or harvesting) because they were evaluated in the statistical analyses among treatments. Field crews were provided a list of possible site-scale management-related factors identified in the field manual (Phillips et al., 2008) and asked to identify whether each factor was present or absent at each landslide; and if present, whether the factor appeared to contribute to slope failure. Crew members were also able to identify 'other' factors that may have contributed to landslide occurrence. Management-related factors that crews were asked to look for included: 1) yarding corridors; 2) silvicultural activities including thinning and brushing; and 3) water diversions.

Field crews found no evidence of any of the listed management-related contributing factors at 717 of the 919 hillslope landslides that delivered to public resources (Table 5-8). 'Other' was the most commonly cited factor and the majority of those (69 of 112) contained text entries which indicated that stream bank erosion was the contributing factor.[21] Although an interesting observation, stream

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[21] The majority (59 of the 69) of landslides for which stream bank erosion was cited as a contributing factor occurred as debris slides in inner gorges in the 21+ year treatments.

Table 5-8: Hillslope road failure count (and percentage) by presence of factors involving road drainage.

Ditchout	Silt-trap	Water-bar	Pirated water	Cross-drain	Outsloped road	Count	Percent of total
No	No	No	No	No	No	189	68%
No	No	No	No	No	Yes	38	14%
No	No	No	No	Yes	No	20	7%
No	No	No	No	Yes	Yes	4	1%
No	No	No	Yes	No	No	9	3%
No	No	No	Yes	No	Yes	3	1%
No	No	No	Yes	Yes	No	3	1%
No	No	No	Yes	Yes	Yes	1	0%
No	No	Yes	No	No	No	4	1%
No	No	Yes	No	No	Yes	2	1%
No	Yes	No	No	Yes	No	2	1%
Yes	No	No	No	No	No	1	0%

Note: Field crews did not enter information related to triggers for four of the 280 non-stream-crossing road failures, so those four landslides are not included in this table.

bank erosion is not a management-related contributing factor, leaving only 14% of landslides having documented contributing factors related to management. Yarding corridors were cited as likely contributing to the initiation of only two landslides. Silvicultural activities and water diversion were never cited as factors which were present and likely to have contributed to landslide occurrence. The WDNR observer variability team agreed with field crews for 97% of the landslides with respect to identification of the listed contributing factors (Miskovic and Powell, 2009).

Field crews were also asked to evaluate whether various types of road drainage contributed to landslide occurrence at hillslope road failures. Potential contributing factors included: 1) water contributions from ditchouts (e.g., water diversion from ditchline to hillslope); 2) the presence of a silt-trap or other water retaining feature on the fillslope or outer edge of a road; 3) water contributions from waterbars; 4) pirated water from nearby channels; 5) cross-drain culverts leading to the site; and 6) water focused by an outsloped road (Phillips et al., 2008). Crews were asked to enter yes or no as to whether each contributing factor was present. The responses were not limited to one factor per landslide. Table 5-9 shows the responses for road drainage contributing factors for hillslope road failures. The most common potential contributing road drainage factor was outsloping of the road (14%), followed by water diverted through cross-drain culverts (7%). Field crews identified no contributing factor at 68% of the hillslope road failures, which may suggest that the presence of over-steepened road fill itself, accounted for in the treatment definitions, is the most common road landslide contributing factor.

Table 5-9: Landslide count and percentage by contributing factor for hillslope landslides that delivered to public resources.

Yarding Corridor	Silviculture	Water Diversion	Other	Count	Percent of total
Absent	Absent	Absent	Absent	717	78.0%
Absent	Absent	Absent	Other factor	112	12.2%
Absent	Absent	Present	Absent	16	1.7%
Absent	Absent	Present	Other factor	8	0.9%
Absent	Not likely a factor	Absent	Absent	34	3.7%
Absent	Not likely a factor	Absent	Other factor	4	0.4%
Absent	Not likely a factor	Present	Absent	1	0.1%
Not likely a factor	Absent	Absent	Absent	19	2.1%
Not likely a factor	Absent	Absent	Other factor	2	0.2%
Not likely a factor	Absent	Present	Other factor	1	0.1%
Not likely a factor	Not likely a factor	Absent	Absent	3	0.3%
Likely a factor	Absent	Absent	Absent	2	0.2%

Note; Field crews made no record for triggering mechanism at 19 delivering landslides so they are not incorporated in these totals.

For stream-crossing landslides, field crews were asked to identify the failure type from a list that included: 1) plugged pipe followed by fill collapse as a debris slide which leaves some of the road prism intact; 2) a plugged pipe followed by ponding of water and fluvial erosion of the fill (i.e., washout); 3) plugged pipe followed by debris flow initiation; or 4) collapse of the fill edge at the outlet with no evidence of a plugged pipe (Phillip et al., 2008).

Crews identified plugged pipes as contributing to 68% of the stream crossing failures and fill edge collapse without plugging in 11% of the failures. For 21% of the failures, the field crews could not determine the failure conditions (Table 5-10). This is partly because data collection occurred 8-11 months after the storm event, such that evidence had been obscured by vegetation growth and some crossings had already been rebuilt.

Table 5-10: Stream-crossing road failure count and percentage by failure type.

Failure description	Count	Percent of total
Plugged pipe – fill edge collapse	20	32%
Plugged pipe – washout	14	22%
Plugged pipe – debris flow	9	14%
Fill edge collapse – no plugging	7	11%
Unknown	13	21%

Note: Field crews did not record failure type for four of the 67 stream-crossing road landslides, so those four are not included in this table.

5.5 Landslides outside of rule-identified landforms

Because the study design anticipated that a portion of the landslides would occur outside RIL (Dieu et al., 2008), field crews determined whether each landslide initiated within or outside of a named RIL.[22] The fraction of landslides in RIL illuminates how completely the existing RIL criteria describe terrain that fails and delivers, at least for the Post-Mortem study area. Of the 1135 delivering landslides (complete census), 45% occurred outside of a RIL, with delivering road landslides being more likely to have occurred outside of RIL (65% outside RIL) when compared with delivering hillslope landslides (41% outside of RIL)(Table 5-11).

Existing RIL consist predominantly of landforms that are located adjacent to streams (e.g., inner gorges) and channel heads (e.g,, bedrock hollows), and this makes it very likely that landslides initiating in a RIL will deliver to a stream. In the road network, where we have a complete sample, landslides that initiated outside of a RIL delivered 50% of the time, while those that initiated within a RIL delivered 94% of the time. We cannot perform that same analysis for hillslope landslides because an unknown number of non-delivering hillslope landslides were not inventoried (both within and outside of RIL).

The fact that an unknown number of non-delivering landslides were not inventoried also makes it impossible to determine the total percentage of hillslope landslides that occurred outside of RIL. When the incomplete sample of non-delivering landslides was added to the census of delivering landslides, 55% of the total occurred outside an RIL. This is likely to be an underestimate, however,

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[22] Individual RIL were not mapped for this study because of problems associated with identifying RIL in older regrowth, submature and mature timber; and because of the overall field effort that would have been required. Crews were asked to determine whether individual landslide initiation sites were located in terrain that met the definition for a named RIL.

Table 5-11: Landslide count by type of rule-identified landform, whether it delivered to a public resource, and event location.

	Delivery					
	Hillslope (No road)		Hillslope road		Stream-crossing road	
Rule-identified landform	No	Yes	No	Yes	No	Yes
Null	152	380	124	93	8	41
Inner gorge	16	406	2	25	0	12
Bedrock hollow	17	105	3	18	0	4
Outer edge of meander bend	0	22	0	8	0	0
Convergent headwall	4	9	0	3	0	1
Toe of deep-seated landslide	3	6	0	1	0	1

Notes: Field crews failed to record RIL status, delivery status or event location for 16 landslides, so those are not incorporated into this table.

because hillslope landslides outside of RIL are more likely to have been missed given their lower delivery potential. These data indicate that RIL are more useful in identifying landslide-prone terrain in harvest units and less applicable in predicting road failures. Also, the regulatory focus on landforms with effective routing mechanisms has benefits in limiting the amount of sediment reaching streams.

During the analysis, numerous hypotheses were explored to explain the 380 hillslope landslides that initiated outside of a RIL and delivered to public resources. Slope is a defining criterion for RIL and one of the first hypotheses proposed was that the field crews had misapplied the slope criteria when determining whether a RIL was present. As noted in Section 5.1, field crews measured the slope adjacent to the landslide as an approximation of the hillslope gradient before the landslide. As shown in Figure 5-7, slope measurements made by the field crews are generally consistent with their RIL determination based on the 70% slope criteria for inner gorges, bedrock hollows, and convergent headwalls; landforms which account for 94% of the landslides within a RIL that delivered. During the WDNR observer variability exercise, however, it was noted that the observer variability team recorded slopes that were on average 5% steeper than the field crews (Miskovic and Powell, 2009). If the field crews recorded slopes that were 5% less than the 'actual slope,' they could have inflated the percentage of landslides occurring outside of RIL by as much as 12%.[23]

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[23] As a result, we report the percentage of delivering hillslope landslides initiating outside of RIL with an estimated range of 29-41%.

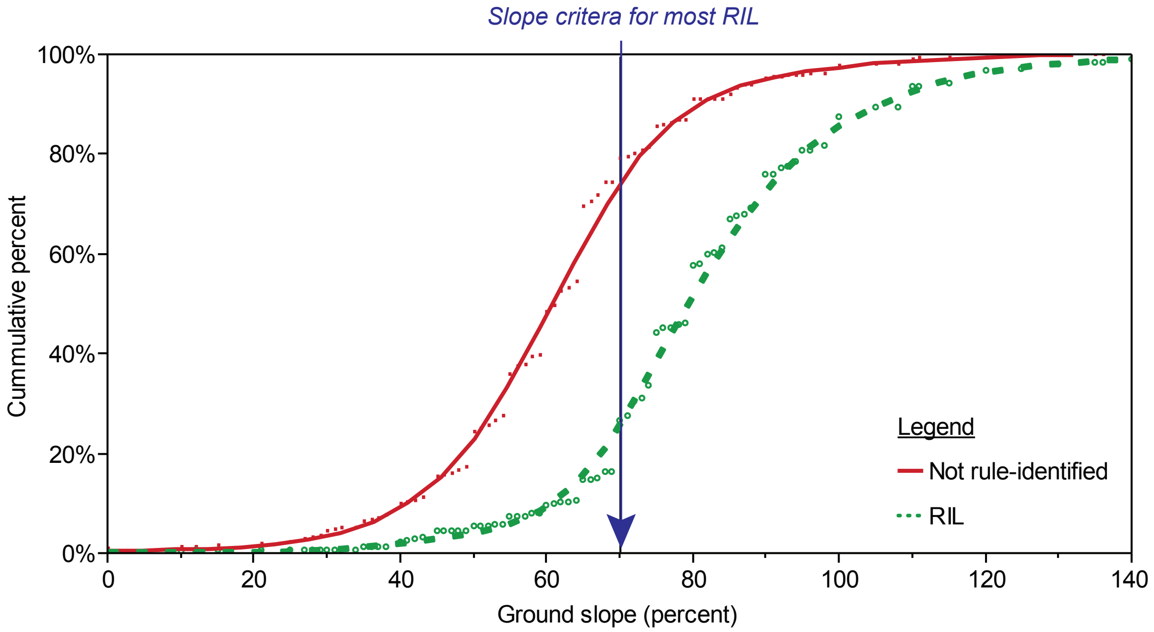

Figure 5-7: Slope gradient (percent) at initiation site for hillslope landslides that delivered as a function of whether they initiated inside or outside of an RIL (n=547 and 380 respectively).

Other working hypotheses were applied in an attempt to explain the percentage of landslides outside of RIL as a function of precipitation, slope, or geology. A common hypothesis was that moderately unstable ground might fail at very high levels of precipitation and thus the percentage of landslides outside of RIL might be positively correlated with precipitation intensity above some threshold. Figure 5-8 shows the percentage of landslides originating outside of RIL as a function of estimated daily maximum precipitation for the storm. The data do not support the hypothesis that the proportion of landslides occurring outside of RIL is correlated with precipitation intensity. In addition, we note no apparent correlation with landslide density (likely to be a better predictor of actual precipitation intensity than interpolated precipitation values) or average slope (Figure 5-8).

Finally, it was proposed that the percentage of landslides outside of RIL might be related to lithology. This was difficult to test because lithology is inherently confounded with precipitation intensity in this study. Study blocks in Crescent Formation Basalts for example, generally have the highest landslide densities, but they also received the greatest amounts of precipitation (Figure 5-9). Given the design, it is difficult to separate the effects of precipitation and lithology. However, no relationship between the percent age of landslides initiating outside of RIL and lithology was noted.

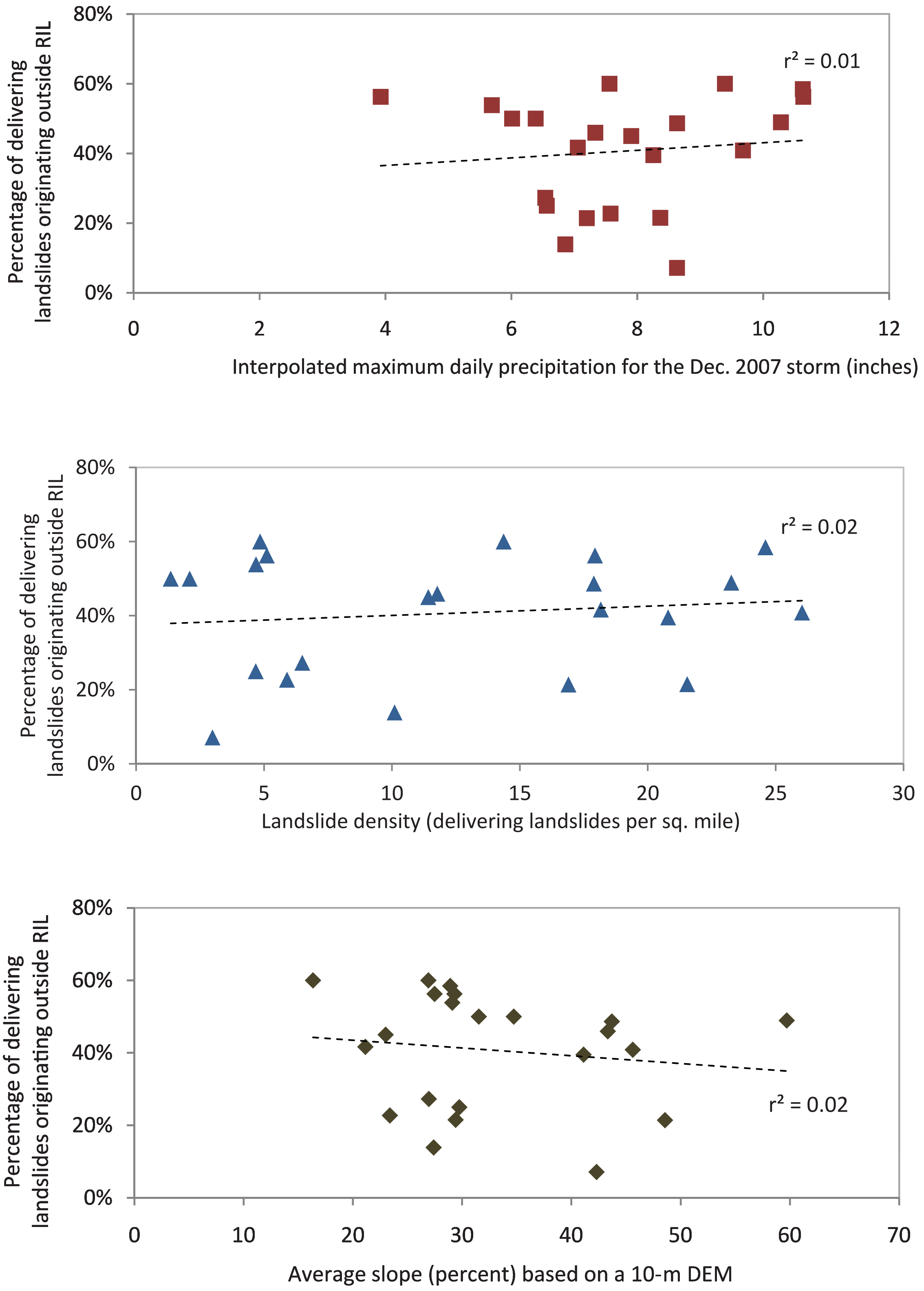

Figure 5-8: Percentage of delivering hillslope landslides originating outside RIL for each block as a function of estimated maximum daily precipitation (top), landslide density (middle) and average DEM slope (bottom).

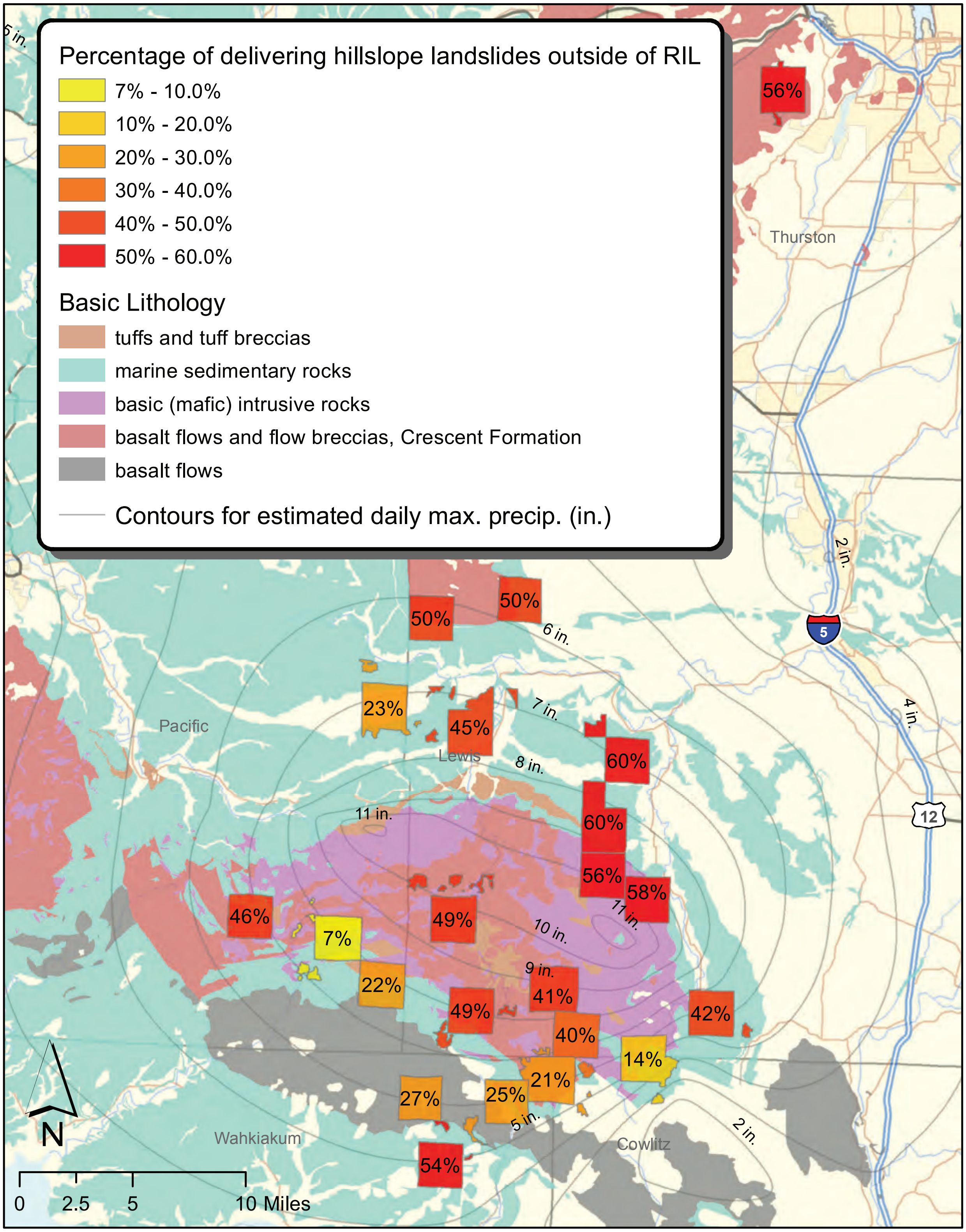

Figure 5-9: Map showing the percentage of delivering hillslope landslides originating outside of rule-identified landforms for landslides that delivered to public resources (n=1135).

5.6 Stand age at landslide initiation sites

The distribution of stand ages for harvest units and buffers across the study area was provided by landowners. A GIS analysis was completed to determine the stand age at landslide initiation sites. Figure 5-10 is a count histogram of delivering hillslope landslides as a function of stand age at the initiation site. Landslide counts by stand age class exhibit a bimodal distribution with peaks in the 0-10 and 30-50 age brackets. The figure would appear to suggest a decreasing trend of landslide occurrence with increasing stand age from the 0-30 years (a hypothesis that is supported by published literature) and increased landsliding in the 30-55 year stand age. However, large landslide counts in a given stand age class might simply reflect a high abundance of that age class, as opposed to increased landslide occurrence. Essentially, the data presented in Figure 5-10 cannot be interpreted without additional analysis

Figure 5-11 shows the distribution of stand ages throughout the entire study area. By comparing Figure 5-10 to 5-11, it can be seen that the distribution of landslide counts by stand age generally

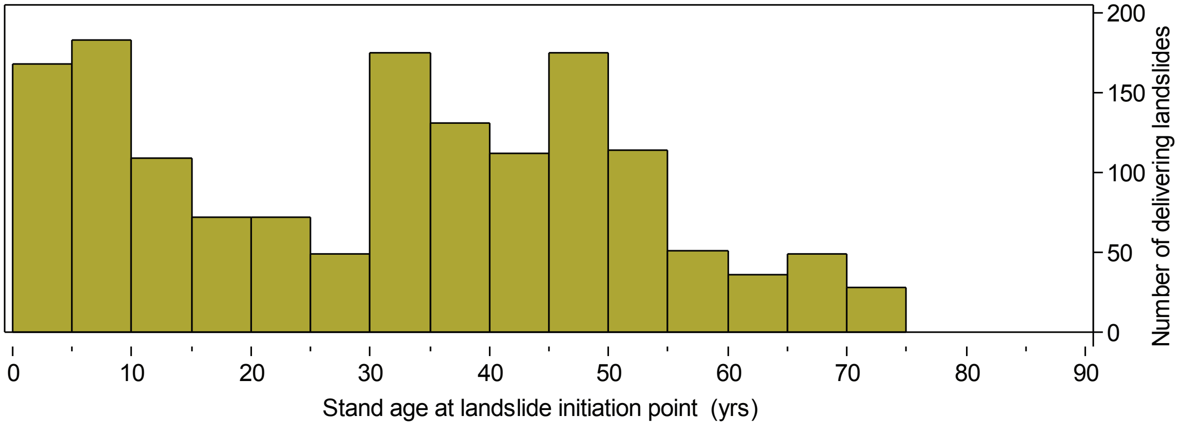

Figure 5-10: Histogram showing the number of hillslope landslides that delivered to public resources as a function of stand age at initiation point

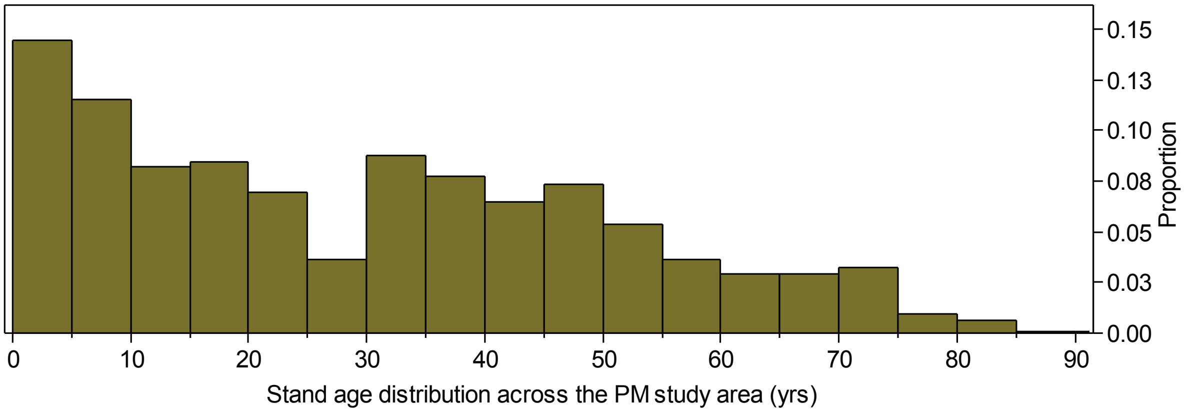

Figure 5-11: Histogram showing the proportion of the Post-Mortem study area occupied by stands of different ages.

follows the distribution of ages across the study area. When we divide the number of landslides by the amount of area in each stand age category, we can display the proportional landslide density as a function of stand age and we see that stands between 30-50 years have the highest landslide density (Figure 5-12). The histogram shown in Figure 5-12 does not account for potential differences in topography, precipitation, management history, or any other factor likely to affect landslide occurrence; and given that current Forest Practices Rules require no-cut buffers on unstable slopes (e.g, RIL buffers), a higher landslide density for hillslopes with stand ages from 30-50 years should not be surprising.

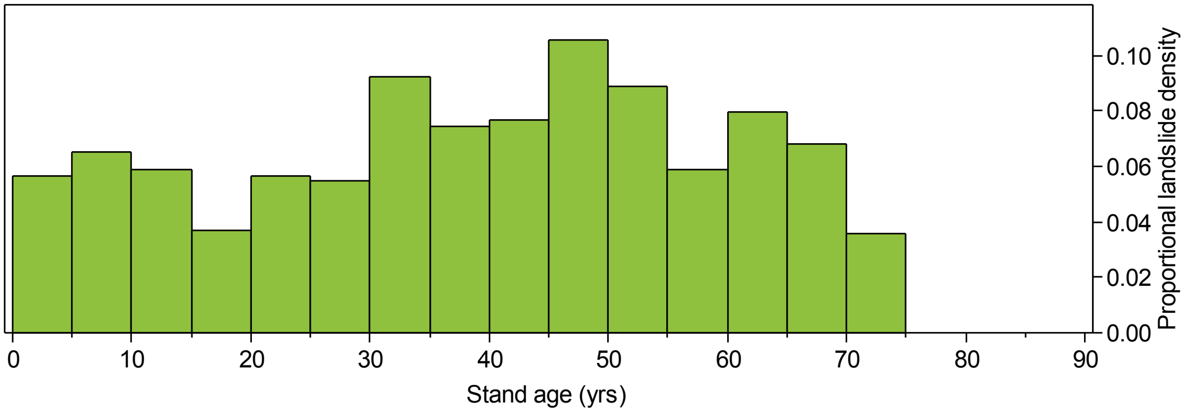

Figure 5-12: Histogram showing the relative density of landslides in different stand age categories.

5.7 Landslides originating in buffers

Our initial analysis of landslide size among the less than 20 year old treatments (FB, PB, and NB) indicated that landslides initiating in buffers are smaller than those initiating outside of buffers, but additional analysis showed that this is was likely an artifact of the study's focus on assessing landslides that delivered to streams. A multivariate mixed-model regression analysis shows that delivered volume is correlated with both initial volume and distance to stream, and that there is a significant interaction between delivery and initial volume. The analysis shows that initial and delivered volume decreases with increasing distance to stream, such that landslides initiating far from the stream (i.e., more likely to be outside of a buffer) have to be proportionately larger in order to deliver.

When the analysis is restricted to delivered volume for all landslides in the Partial Buffer treatment, the only treatment to contain buffered and unbuffered RIL, and the data are analyzed as a function of buffer presence at initiation point (left side of Figure 5-13), the results indicate that landslides in buffers are significantly smaller than those initiating outside of buffers. But, when the analysis is further restricted to landslides that initiated from within buffered (n=84) and unbuffered (n=38) RIL we find no difference in the size of landslides as a function of buffer presence (right side of Figure 5-13).[24] As a result, we interpret the significant differences in delivery volume shown on the left side

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[24] We restrict the analysis to landslides initiating in RIL because RIL are defined in part by their high potential for delivery.

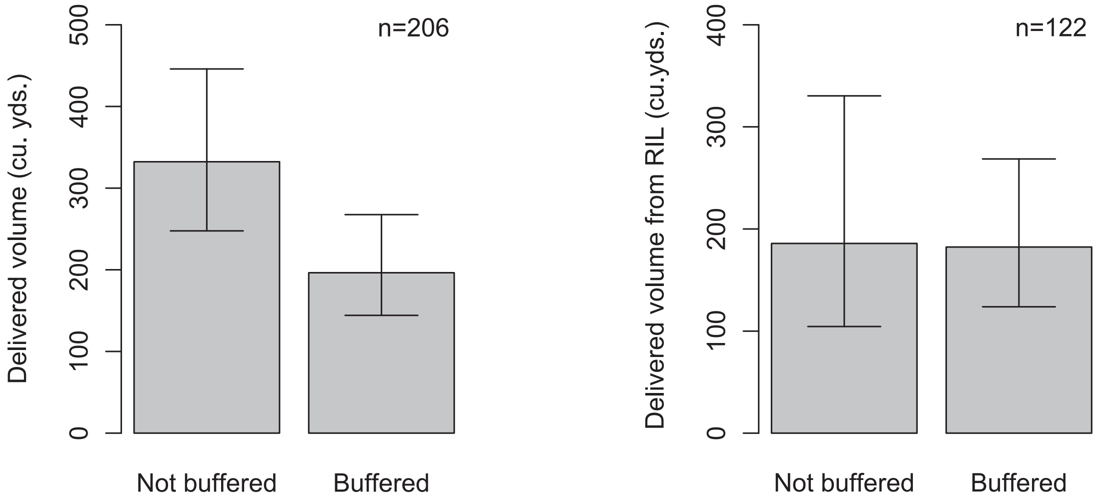

Figure 5-13: Barplot of average delivered sediment volumes as a function of buffer presence at initiation site for all delivering shallow-rapid landslides from the PB treatment. The left plot includes all shallow-rapid landslides from PB that delivered, while the right side is restricted to landslides that initiated within RIL. Confidence intervals on 90% based on the inverse of a log-linked glm. The significant difference in volume shown in the left graph is the result of landslides that initiated outside of buffers (small landslides outside of buffers have a lower probability of delivery and therefore a lower probability of being counted).

of Figure 5-13 as potentially being an artifact of unequal sampling in which small landslides outside of buffers were not counted. While it may be true that buffers composed of mature trees can reduce the size of landslides, this descriptive analysis which does not account for the effects of precipitation or other factors that vary across blocks, cannot be used to support this argument.

Landslides provide an important transport mechanism for delivering large wood to stream channels where it serves as an important component of aquatic habitat (Bilby and Bisson, 1998; Bigelow et al., 2007). To evaluate whether buffers affected the proportion of landslides that delivered large wood to channels, the subset of delivering landslides was selected for analysis.[25] These data are plotted as a function of buffer presence and large woody debris (LWD) delivery in Figure 5-14.

A nominal logistic regression that incorporated LWD delivery as a dependent variable and initial volume, gradient and buffer presence as significant predictor variables, indicated that there was a difference in LWD delivery by buffer presence and that the effect of gradient was crossed with buffer presence. Factor profiling revealed that landslides delivering LWD was significantly greater for landslides originating in buffers ($p<0.001$) and that there was a small additional increase in the probability of LWD delivery with increased landslide size (no gradient effect). For landslides that initiated outside of a buffer, the probability of LWD delivery increased with landslide initiation size and decreased with increasing gradient.

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[25] Data were limited to delivering landslides in treatments expected to have unstable slope buffers (PB and FB) where buffer presence was specified from field observations. The analysis was further restricted to debris avalanches, debris flows, and debris slides which account for 96% of all delivering landslides. There were 268 landslides that met the listed criteria, 53% of which originated in a buffer and 47% that did not.

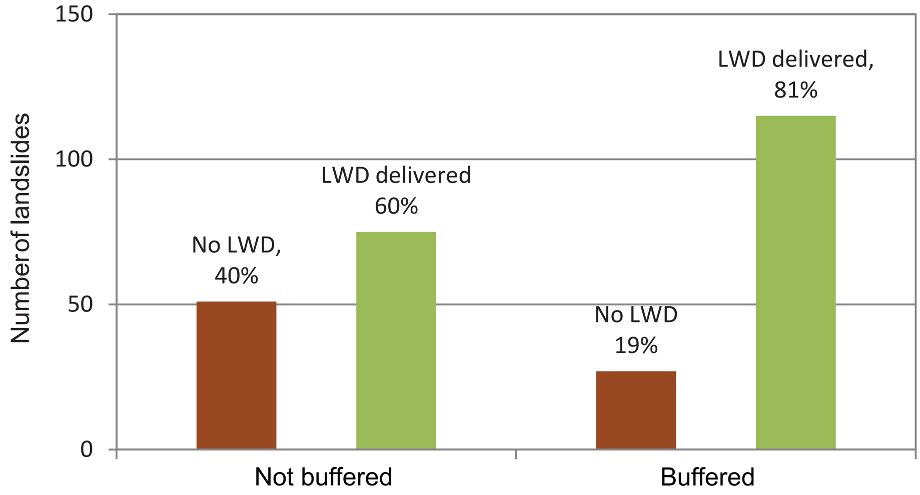

Figure 5-14: Barplot of count and proportion of large woody debris (LWD) delivery as a function of buffer presence.

5.8 Treatments

As discussed in Section 2.2, every road segment and harvest unit or forest stand in the study area was assigned to one of five road or harvest treatments based on the characteristics of the experimental unit. For each treatment, the number and size of landslides were normalized by area of the treatment.[26] The data were organized in a one-way layout and the differential landslide response among treatments was used for evaluating Forest Practices Rule effectiveness.

5.8.1. Harvest treatments

There were 938 delivering hillslope landslides in the five harvest treatments. All five treatments contained landslides, though the number of landslides was not directly proportional to the area sampled (Figure 5-15). Density is better than count for making comparisons among groups with unequal sampling intensity or sample area. Hillslope landslide density (landslides per square mile) for delivering landslides varied greatly by block (Figure 5-16), most likely as a result of differences in precipitation intensity and perhaps influenced by other factors (Figure 5-3, Figure 5-5). Treatment landslide densities varied less than block densities (Figure 5-17) with the Mature, Submature, and Full Buffer treatments exhibiting lower mean densities for delivering landslides than either the Partial Buffer or No Buffer treatments (Table 5-12 and Figure 5-18).

Field crews estimated two different landslide volumes for each landslide in each treatment: 1) a volume based on initial failure site dimensions; and 2) the volume of sediment that delivered to public resources. Both volumes were summed over the treatments within blocks and then converted to mass

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[26] Treatments are pooled at the scale of blocks so that there is effectively one treatment per block.

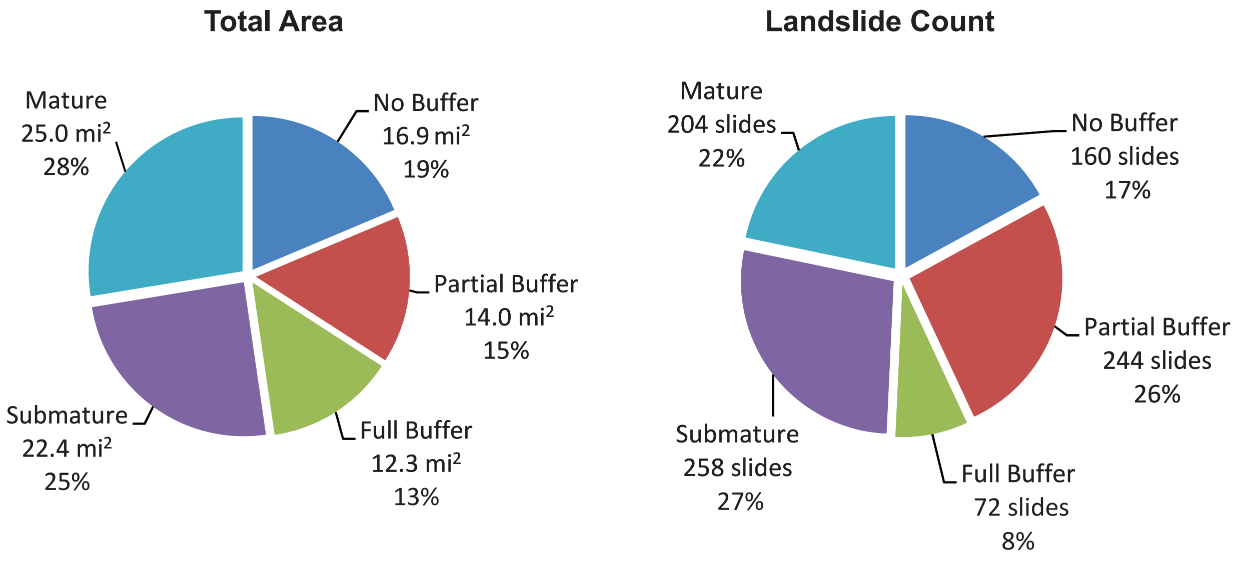

Figure 5-15: Pie charts of total land area (left) and count of landslides that delivered to public resources (right) for each harvest treatment.

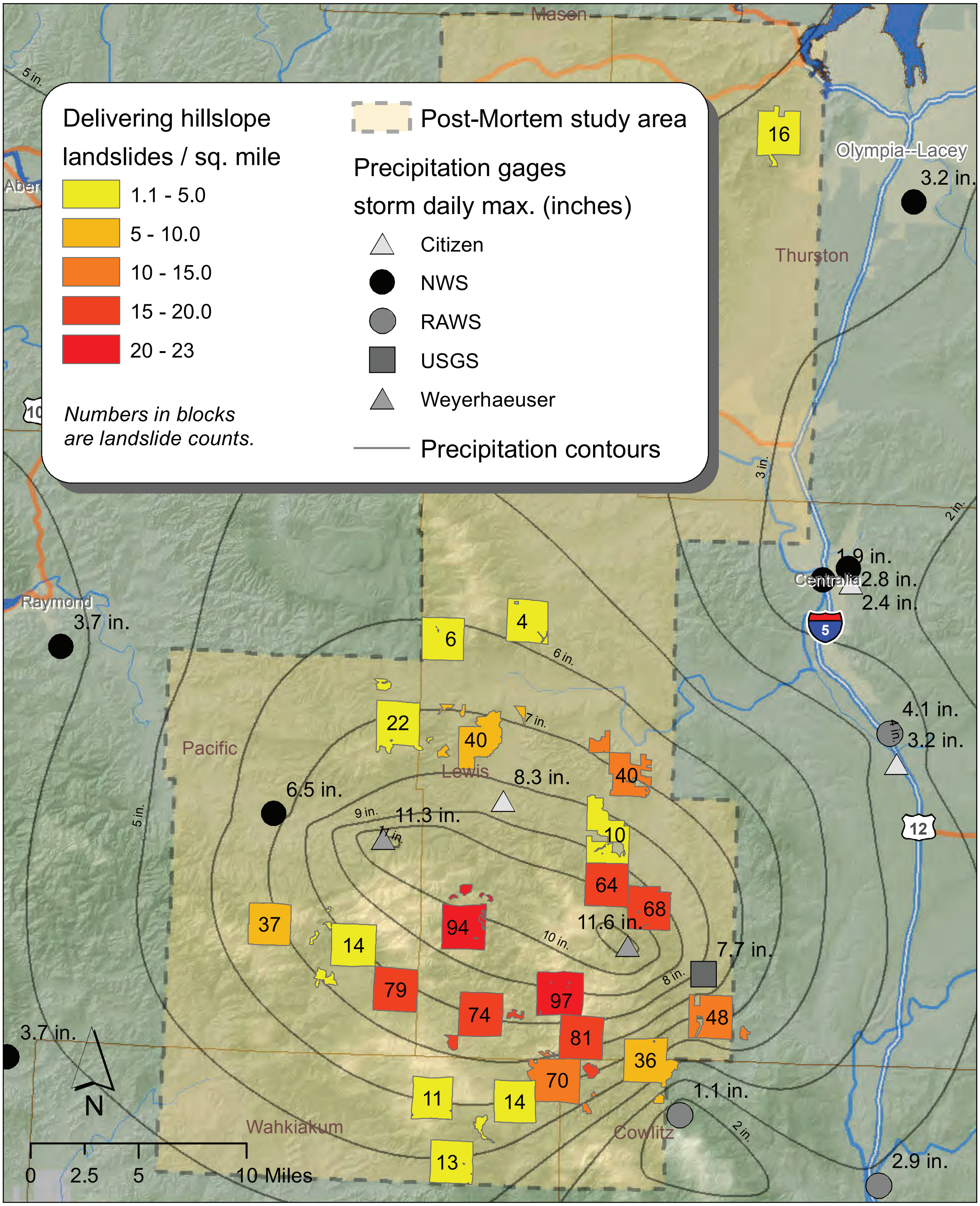

Figure 5-16: Landslide density and count for hillslope landslides that delivered to public resources. Colors denote density, and the numbers within each block are landslide counts. Precipitation contours are based on a nearest neighbor interpolation of gage station readings.

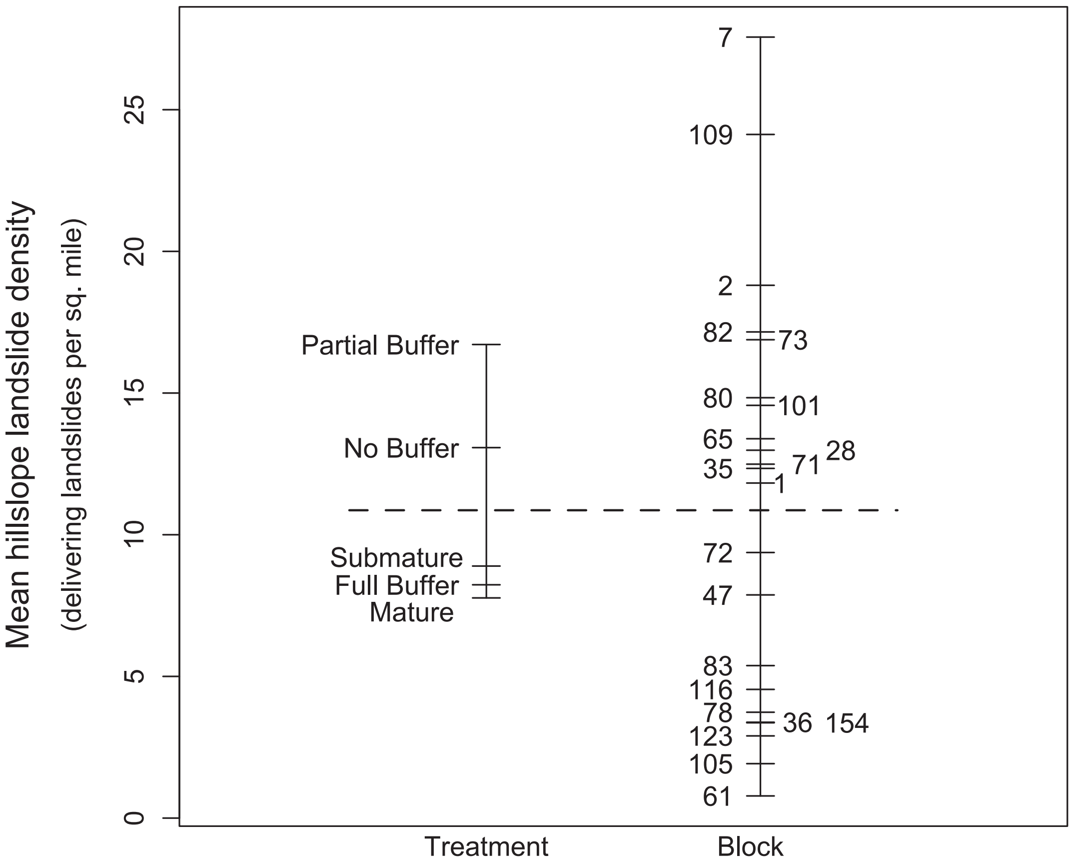

Figure 5-17: Cell means plot for delivering hillslope landslide density (one cell for each treatment within each block). Short horizontal lines indicate the mean for each treatment (n=5) and block (n=20-22) with the grand mean (10.3 landslides per square mile) indicated by the dashed line.

Table 5-12: Statistics for harvest treatment landslide density.

Treatment	n*	Mean density (slides / sq. mi.)	Std. Dev.	Min.	Median	Max.	CV
No Buffer	20	13.1	12.9	0	9.4	50.0	1.0
Partial Buffer	21	16.7	13.8	0	13.2	48.9	0.8
Full Buffer	22	8.2	10.0	0	2.2	30.5	1.2
Submature	21	8.9	9.4	0	6.7	29.1	1.1
Mature	22	7.8	6.5	0	6.0	19.1	0.8

Note: Statistics are calculated at the block scale.
*Although every effort was made to create a balanced dataset, treatments are missing from some blocks.

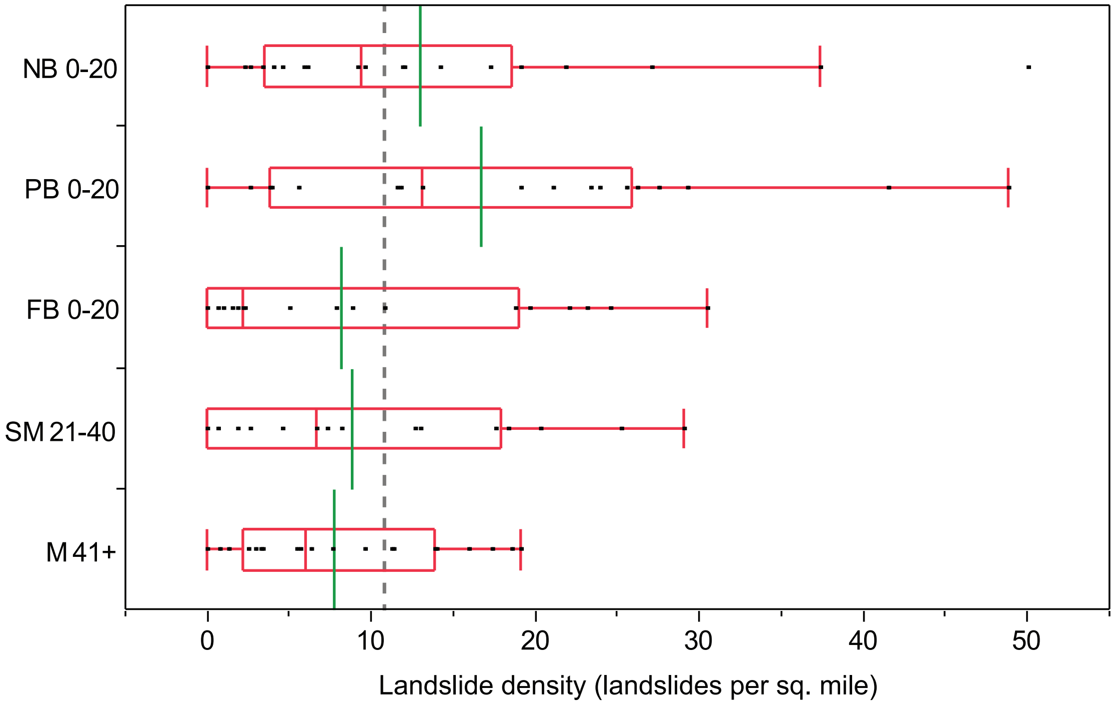

Figure 5-18: Box and whisker plot of landslide density for delivering hillslope landslides by treatment. Treatments are arranged by the predicted landslide density in the Post-Mortem Study Design (highest predicted to lowest predicted). The box outlines the upper and lower quartiles of observations (25th and 75th percentiles). The red line within the box indicates the median value and the green line extending through the box indicates the mean value for each treatment. The grand mean is shown with a gray dashed line. Cell values are shown with dots.

density (tons per acre) for reporting purposes.[27] Table 5-13 and Figure 5-19 show total initial landslide mass density by block for landslides that delivered to public resources. Initial sediment yields were heavily skewed, with means greater than median and coefficients of variation (i.e., ratio of standard deviation to the mean) greater than one for all treatments. On average, the No Buffer treatment had the greatest initial sediment yield and Full Buffer had the smallest.

Estimates of the total amount of sediment that delivered into the channel network (per unit area) are presented in Table 5-14 and Figure 5-20. Median delivered yields are very similar to initial yields and delivered yields have similarly high coefficients of variation, but the quantity of sediment that delivered was generally estimated to be less than the quantity of sediment in the initial failure. On average, the No Buffer treatment had the greatest delivered sediment volume and Full Buffer had the smallest.

With respect to landslide occurrence, stand age is an important characteristic because numerous field-based landslide studies have identified a link between stand age, or time since harvest, and

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[27] A constant bulk density was used so that tons per acre is proportionally equivalent to volume per unit area.

Table 5-13: Statistics for initial sediment yield from hillslope landslides that delivered to public resources.

Treatment	n	Mean initial sediment yield (tons per acre)*	Std. Dev.	Min.	Median	Max.	CV
No Buffer	20	120	313	0	32	1425	2.6
Partial Buffer	21	63	89	0	21	295	1.4
Full Buffer	22	31	54	0	4	203	1.7
Submature	21	53	127	0	2	531	2.4
Mature	22	57	199	0	7	943	3.5

Note: Statistics calculated at the block scale/
*Based on estimates of initial landslide volume for landslides that delivered to stream channels.

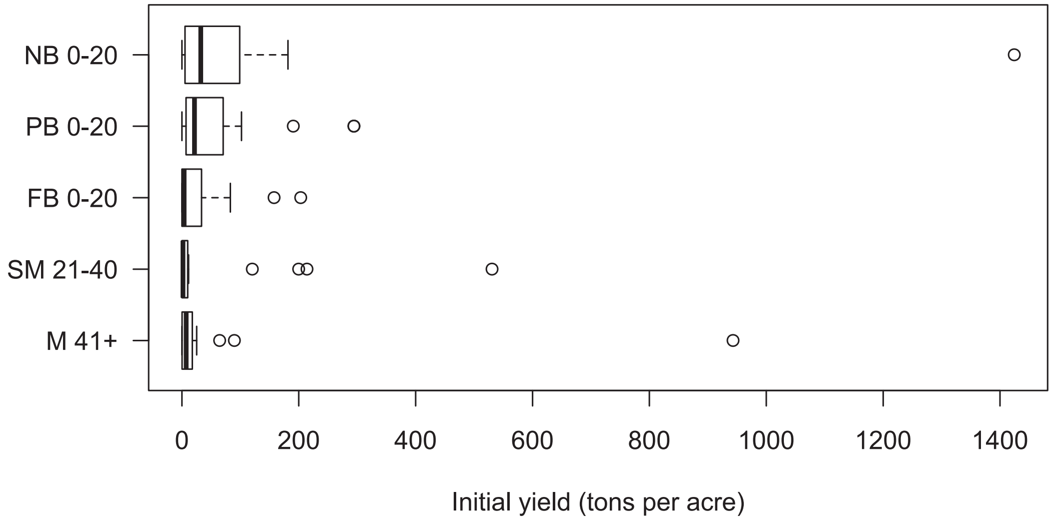


Figure 5-19: Boxplot of initial sediment yields within blocks by harvest treatment.

Table 5-14: Statistics for delivered sediment from shallow rapid hillslope landslides that delivered to public resources.

Treatment	n	Average yield delivered* (tons per acre)	Std. Dev.	Min.	Median	Max.	CV
No Buffer	20	66	155	0	22	703	2.3
Partial Buffer	21	55	85	0	26	356	1.5
Full Buffer	22	16	29	0	3	120	1.7
Submature	21	60	126	0	2	440	2.1
Mature	22	61	173	0	5	754	2.8

Note: Statistics calculated as the sum at the block scale.
*Shallow rapid defined as debris avalanches, debris slides, and debris flows.

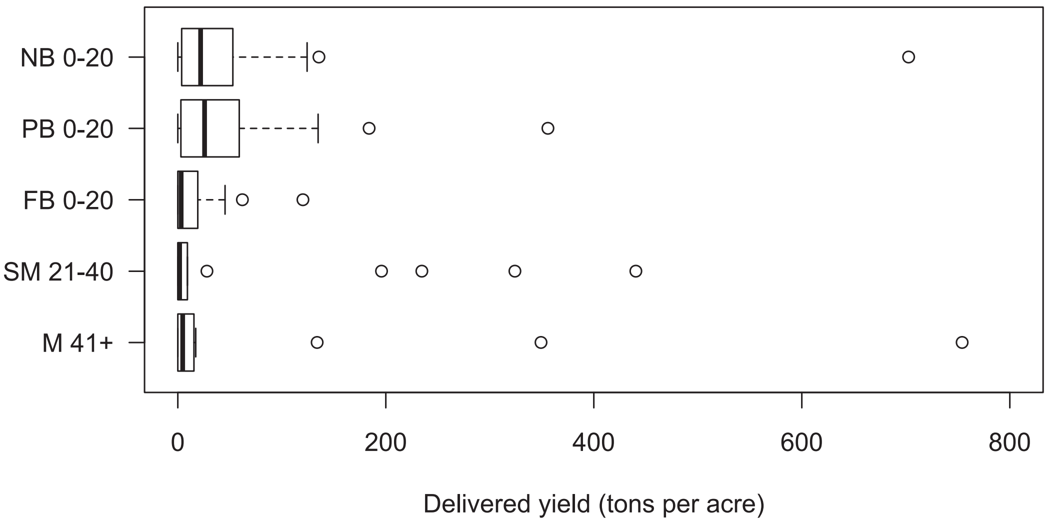


Figure 5-20: Boxplot of total delivered sediment yield by harvest treatment.

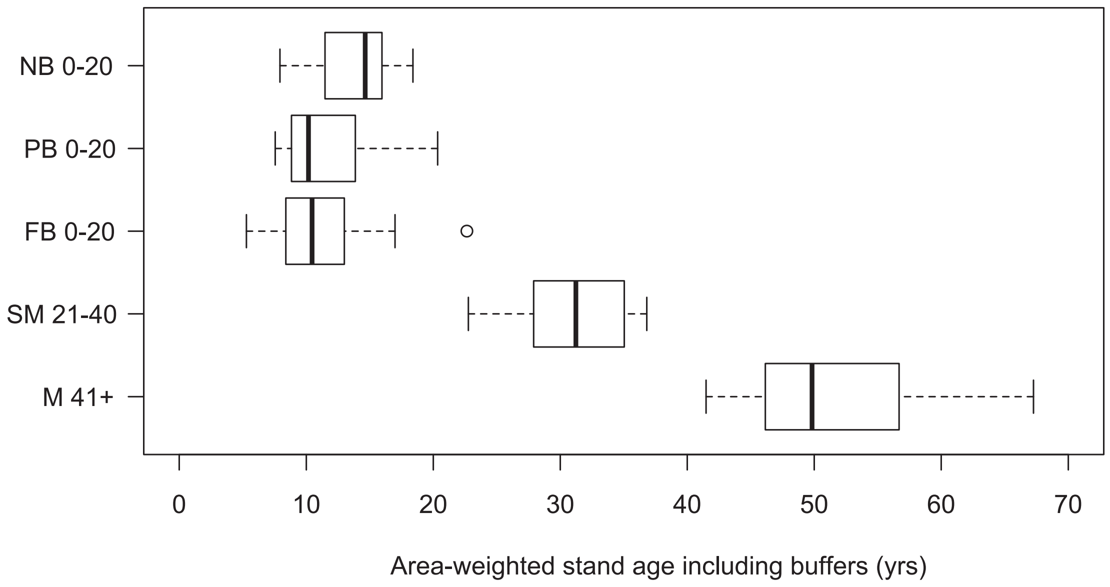
landslide response. As discussed in Section 5.6, stand age data were obtained from landowners for the entire study area. Table 5-15 and Figure 5-21 show the range of area weighted mean stand ages for each block and treatment, with and without the area occupied by buffers. The data show that Full Buffer units are the youngest on average, and that buffers composed of older standing timber greatly inflate the mean age for the 0-20 year old treatments. Based on Sidle (1991, 1992) estimates of root strength and Imaizumi's estimates of landslide occurrence as a function of time since harvest (e.g., stand age excluding buffers), one might expect Full Buffer to have the highest landslide rates if buffers had no affect on landslide occurrence (see Figure 2.1).

Hillslope gradient is also clearly related to landsliding because, as slope increases, the down-slope component of gravitational force also increases and landslides become more likely. Field reports indicated that there were potential differences in slope gradients associated with different harvest

Table 5-15: Statistics for area-weighted stand age with and without areas of forest buffer.

		Area-weighted stand age with buffer			Area-weighted stand age excluding buffers*		
Treatment	n	Min	Mean	Max.	Min.	Mean	Max
No Buffer	20	7.9	13.8	18.4	5.2	12.7	18.4
Partial Buffer	21	7.6	11.9	20.3	1.8	5.8	18.0
Full Buffer	22	5.3	11.3	22.7	0.7	3.3	11.1
Submature	21	22.8	31.0	36.8	22.8	31.0	36.8
Mature	22	41.5	52.8	75.0	41.5	52.8	75.0

Note: Statistics are calculated at the block scale.
*Approximately equal to time since harvest.



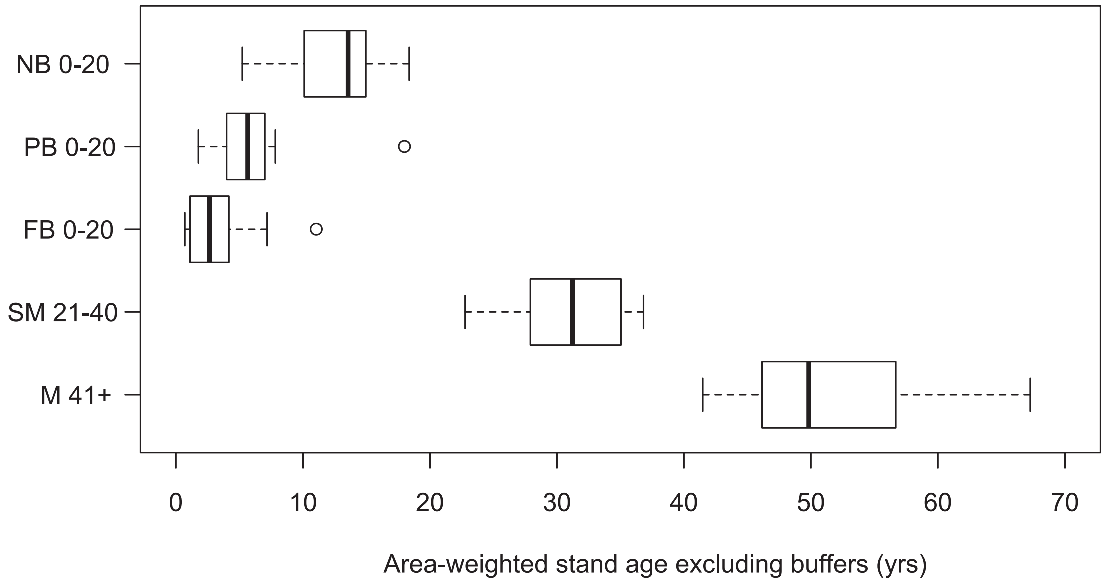


Figure 5-21: Box and whisker plots of area-weighted stand age by treatment and block with (top) and without buffers. The lower plot reflects time since harvest by block for each treatment. Buffers inflate area-weighted values for PB and FB because, even though they occupy a small area, the age of the buffer trees is much greater than the rest of the unit, while the inclusion of buffer tree ages (including riparian) has very little affect on the other treatments.

treatments; in particular, harvest units without RIL that were divided between No Buffer and Full Buffer treatments appear to have lower slope gradients. Table 5-16 and Figure 5-22 show the range of mean slope calculated from a 10-meter DEM for each treatment and block. The data indicate that the Full Buffer treatment (including buffers) is associated with lower gradient slopes than any of the other four treatments, which have very similar mean slope values. It is therefore possible that differences in gradient help explain differences in the landslide response among treatments. As noted in Section 3.4, the Partial Buffer treatment is the only treatment that each harvest unit must, by definition, contain RIL. While it is possible that this could result in the Partial Buffer treatment containing more RIL and a higher inherent risk of failure (discussed further in Section 7.3.2), the finding that the mean and median slope gradient is similar among the treatments (other than FB) suggest that this is not the case.

Table 5-16: Statistics for average slope gradient by treatment and block.

Treatment	n	Mean Gradient (%)	Std. Dev.	Min.	Median	Max.	CV
No Buffer	20	35	14	21	31	72	0.39
Partial Buffer	21	34	8	18	33	55	0.24
Full Buffer	22	27	7	18	25	44	0.27
Submature	21	34	14	13	30	62	0.41
Mature	22	33	10	15	33	51	0.30

Note: Statistics are calculated at the block scale.

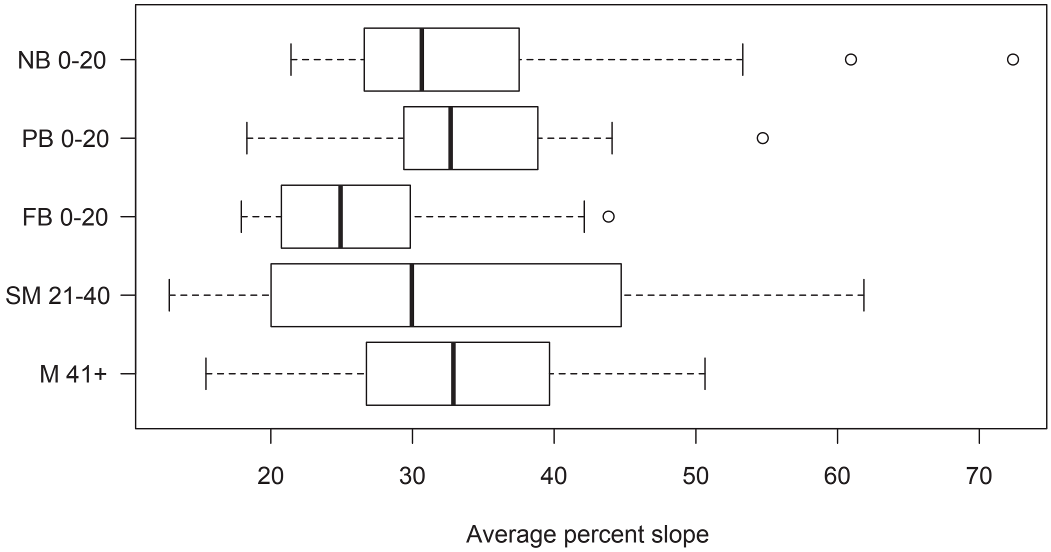


Figure 5-22: Box and whisker plot of average percent slope as calculated in a 10m DEM by treatment and block.

5.8.2. Landslides and landforms in the Partial Buffer treatment

When the study design was developed, it was assumed that Partial Buffer harvest units would be encountered infrequently because the treatment is not consistent with either the most common pre-FFR (i.e., no buffers on RIL) or post-FFR Forest Practices Rules (i.e., buffers on all RIL). During implementation of the study, it became apparent that the number of harvest units classified as Partial Buffer was greater than anticipated. Results indicated that 50% of the area that was harvested since 2001 (i.e., subject to current Forest Practices Rules) was classified as Partial Buffer, while 45% was classified as Full Buffer, and 5% was classified as No buffer. An early hypothesis was that the large amount of area in the Partial Buffer treatment was associated with harvest units in which unstable slopes were harvested because they had no potential to deliver to public resources. However, 46 of the 64 landslides that initiated on RIL in the Partial Buffer treatment from harvest units that were harvested since 2001 did deliver.

Because this result was likely to raise questions about the nature of the Partial Buffer treatment, a follow-up office review was conducted to characterize the harvest landforms in the Partial Buffer treatment. Eighty percent of the mainstem inner gorges were fully buffered, small sideslope inner gorges were fully buffered in 35% of the observations, and bedrock hollows were fully buffered only 21% of the time. Partial buffering instead of full buffering commonly occurred for both types of inner gorges where full buffering did not occur; partial buffering of bedrock hollows was not common. Approximately 60% of the area of RIL in the Partial Buffer treatment was buffered and 40% was unbuffered. Full results of that analysis may be found in Appendix A.4.

5.8.3. Road treatments

There were 208 delivering landslides identified as Hillslope Road or Stream Crossing Road by the field crew in 555 miles of road in 6.3 square miles of road corridor. Standard roads were the most common road group, representing 60% of the surveyed road length (Figure 5-23). Substandard roads were the next most common (16%) followed by Mitigated (10%). Orphaned roads were the least common, but they had a relatively high number of landslides.

Orphaned roads had the highest landslide density with 0.67 landslides per mile of road, while Abandoned roads had the lowest density (0.18, Figure 5-23). It is worth noting that the median (50%) landslide density was zero for all but one treatment (Table 5-17 and Figure 5-24). This means that for a given treatment, there were no sediment-delivering road-related landslides in at least half of the blocks. In fact, four blocks accounted for 70% of the road-related landslides that delivered to streams, and more than half of the blocks (59%) had fewer than four road-related delivering landslides (Figure 5-25). As a result, variability in the mean road landslide density is not only greater for blocks than among treatments, but the densities among treatments are predominately a function of the many landslides in a relatively small proportion of the study area (Figure 5-26).

Road landslide sediment yields (tons per acre) mimic the landslide count data in that they are highly skewed toward zero, with scattered higher values for both initial (Table 5-18, Figure 5-27) and delivered volumes (Table 5-19, Figure 5-28), but delivered volumes do appear to be slightly more variable.

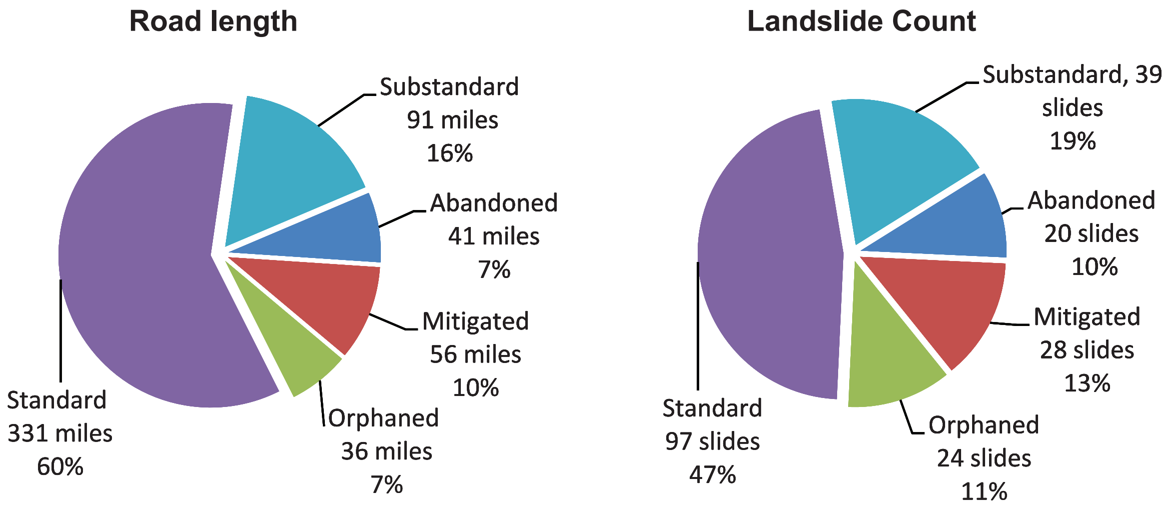


Figure 5-23: Pie charts of road length (left) and count of landslides that delivered to public resources (right) for each road treatment.

Table 5-17: Statistics for road landslide area density for delivering landslides.

Treatment	N	Mean density (slides / sq. mi.)*	Std. Dev.	Min.	Median	Max.	CV
Substandard	22	27.4	46.3	0	0	184	1.7
Orphaned	18	57.3	127	0	0	533	2.2
Standard	22	25.7	38.5	0	6.8	124	1.5
Mitigated	22	27.5	55.0	0	0	189	2.0
Abandoned	20	16.1	40.2	0	0	134	2.5

Note: Statistics are calculated at the block scale.
*Road corridor width assumed to be 60 feet for the purpose of calculating road area.

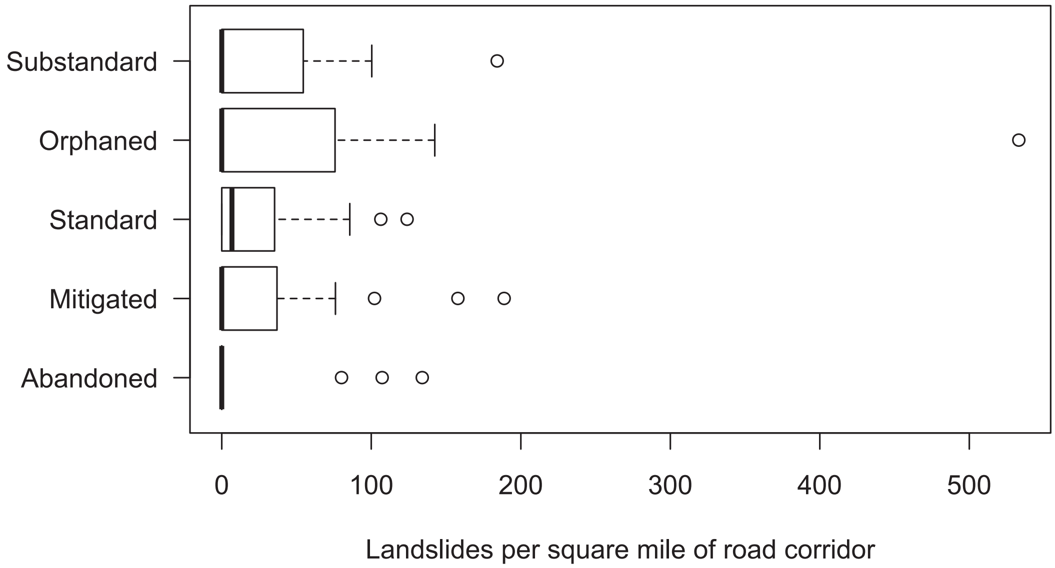


Figure 5-24: Box and whisker plot of road landslide area density by treatment. Block densities are right skewed, with medians of zero for all but the standard road treatment.

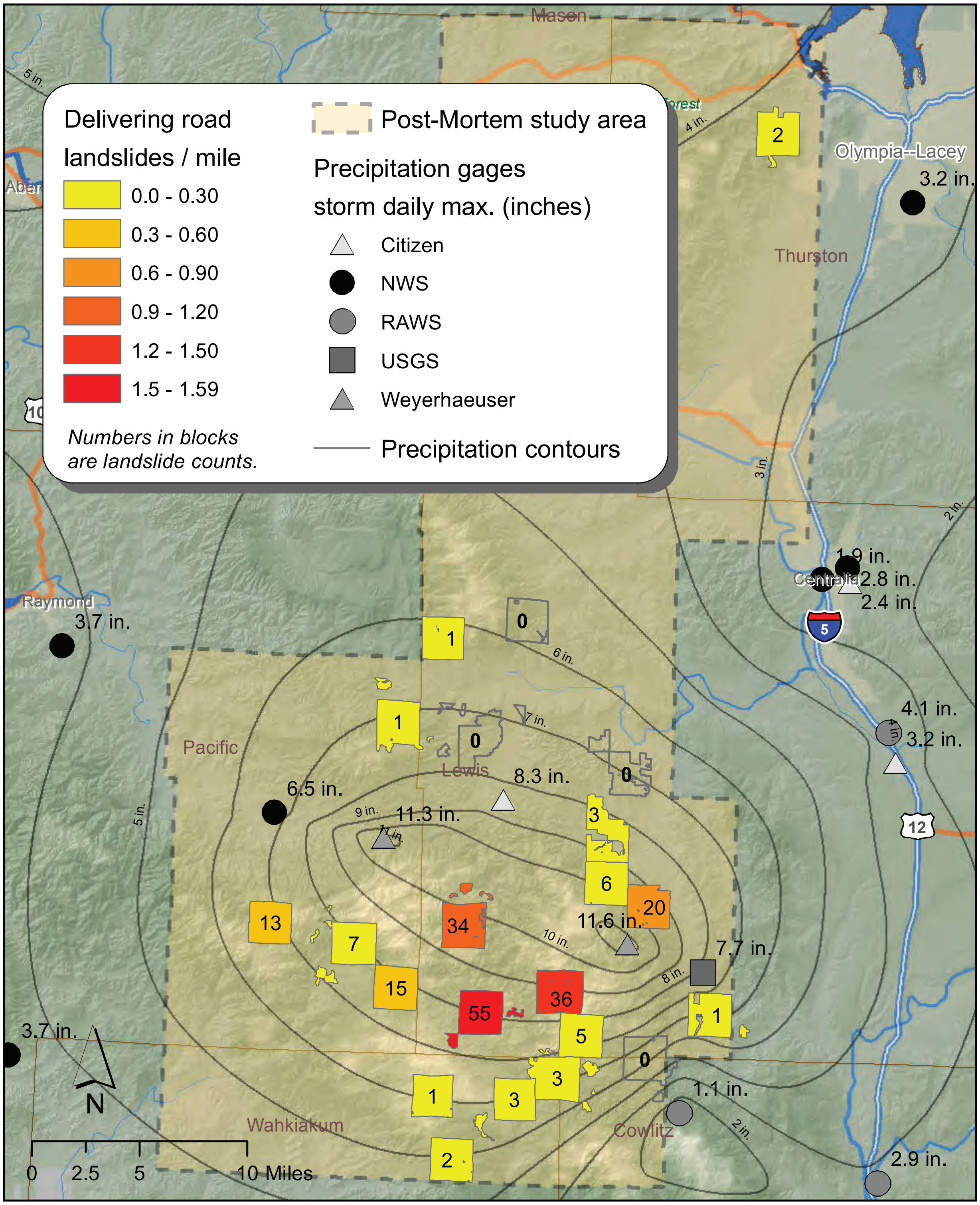


Figure 5-25: Road landslide density and count for landslides that delivered to public resources. Colors denote density, and the numbers within each block are landslide counts combined among all treatments. Precipitation contours are based on a nearest neighbor interpolation of gage station readings.

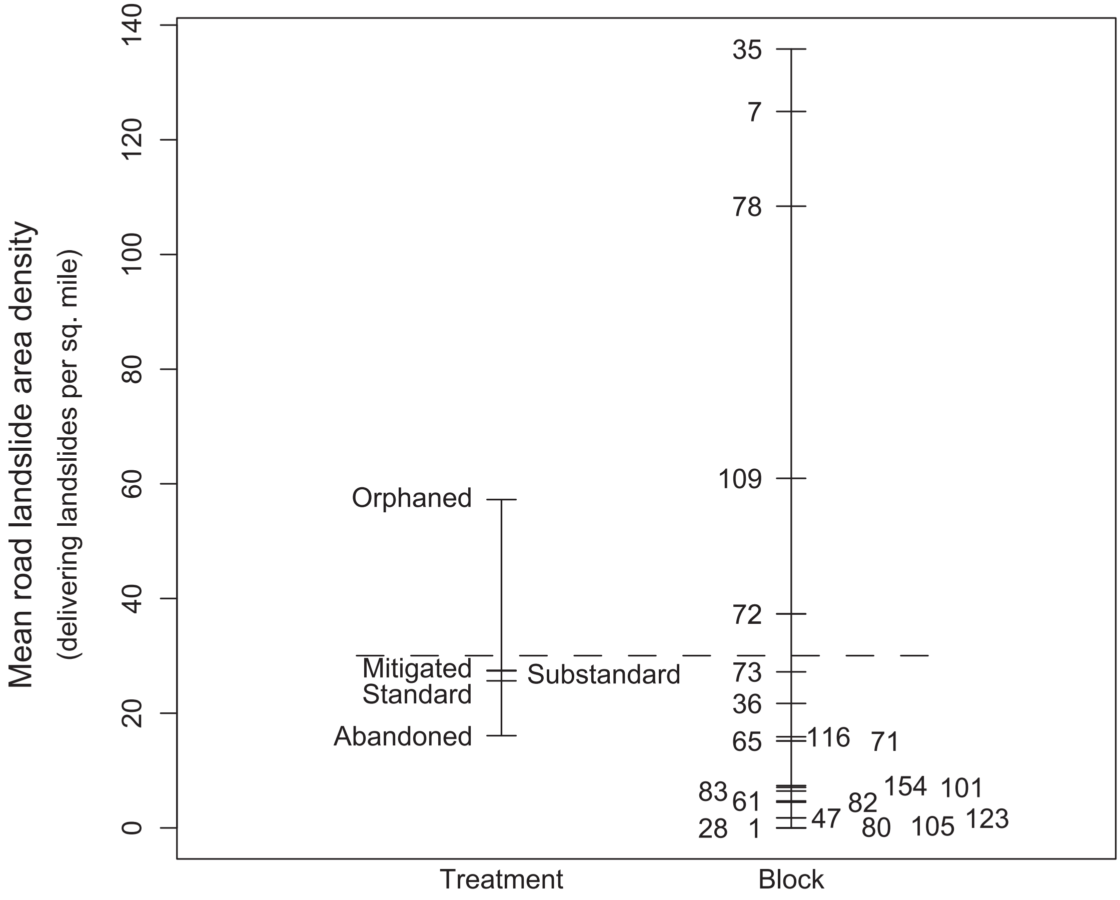


Figure 5-26: Cell means plot for delivering road landslide density (one cell for each treatment within each block). Short horizontal lines indicate the mean for each treatment (n=5) and block (n=18-22) with the grand mean (30 landslides per square mile) indicated by the dashed line.

Table 5-18: Statistics for initial sediment yield from road landslides that delivered to public resources.

Treatment	N	Mean initial sediment yield (tons per acre)*	Std.Dev	Min.	Median	Max.	CV
Substandard	22	155	338	0	0	1207	2.2
Orphaned	18	540	1696	0	0	7029	3.1
Standard	22	198	438	0	4.0	1625	2.2
Mitigated	22	499	1814	0	0	8536	3.6
Abandoned	20	146	358	0	0	1039	2.4

Note: Statistics are calculated at the block scale.
*oad corridor width assumed to be 60 feet for the purpose of calculating road area.

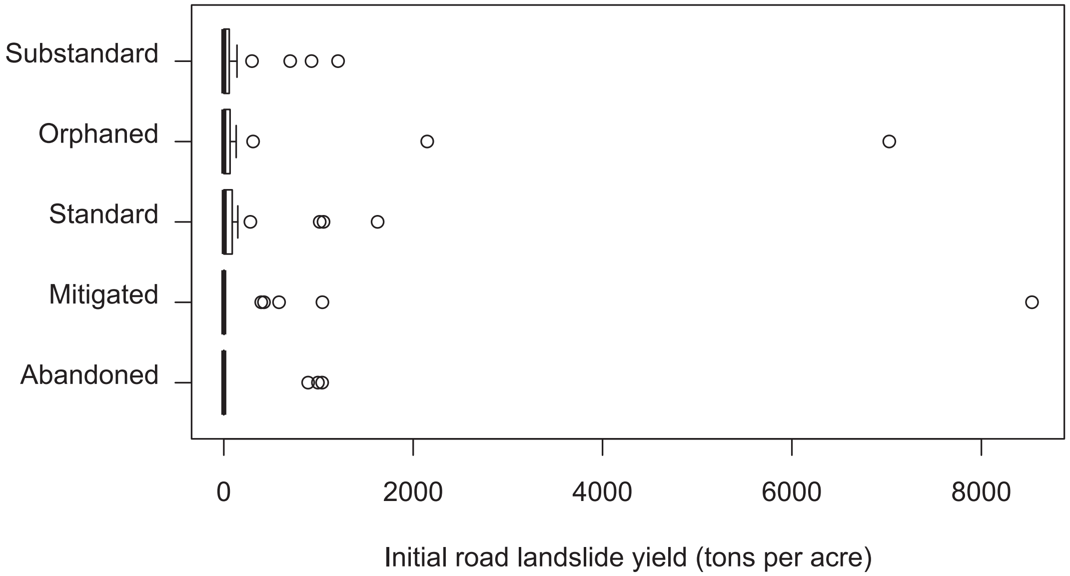


Figure 5-27: Boxplot of total initial sediment yield by road treatment (summarized at the block-level).

Table 5-19: Statistics for delivered sediment from road landslides that delivered to public resources.

Treatment	N	Mean delivered sediment yield (tons per acre)*	Std.Dev	Min.	Median	Max.	CV
Substandard	22	212	534	0	0	2155	2.5
Orphaned	18	633	1720	0	0	7029	2.7
Standard	22	177	395	0	4.1	1471	2.2
Mitigated	22	599	2328	0	0	10955	3.9
Abandoned	20	178	515	0	0	2121	2.9

Note: Statistics are calculated at the block scale.
* Road corridor width assumed to be 60 feet for the purpose of calculating road area.

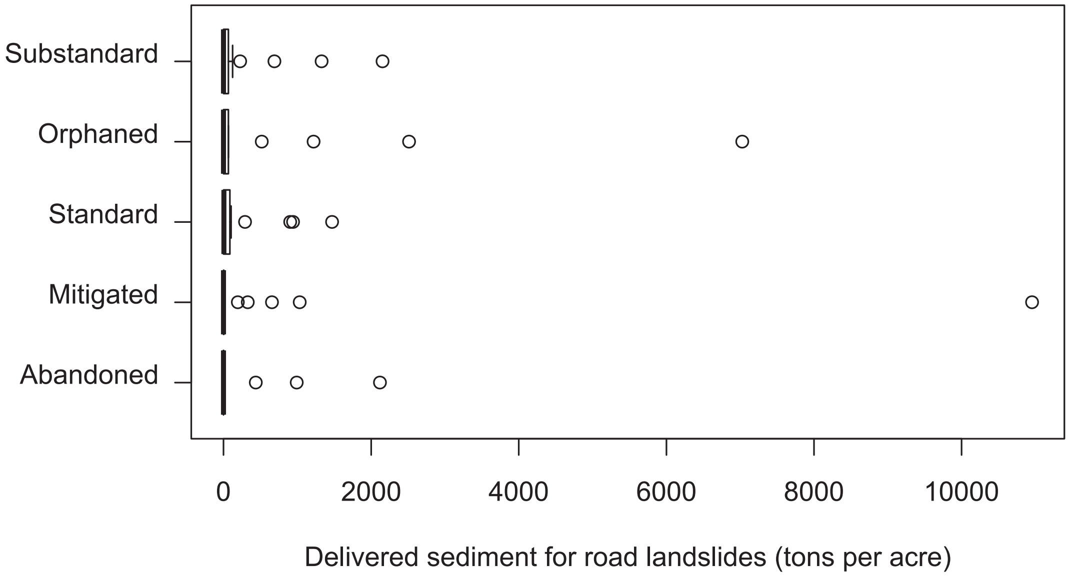


Figure 5-28: Boxplot of total delivered sediment yield by road treatment (summarized at the block-level).

SECTION 6: STATISTICAL COMPARISON OF TREATMENTS

This section contains the results that incorporate statistical analyses. Within the context of adaptive management, statistical tests are used to help identify small but potentially important differences among treatments, and to distinguish patterns of correlation from background variation and sample error (Sit and Taylor, 1998).

This study utilizes a randomized complete block design with five treatments in a one-way layout. The blocks (~4 sq. mile sample areas) were randomly selected from commercial forest lands in western Washington that had a landslide density of at least one landslide per square mile. Experimental units within blocks and treatments are pooled so that there is effectively one of each treatment type per block.

6.1 Harvest treatment landslide counts

Harvest treatment landslide counts were restricted to those landslides that delivered to public resources, for which we have a complete count. Area was included as a offset so results represent landslide densities. Data were considered to be Poisson distributed and block was treated as a random factor.

6.1.1. Competing models

Blocking accounts for factors that are likely to be similar within blocks (e.g., precipitation, geology, large scale topography); however, analyses that include covariates have the potential to increase the power and precision of regression estimates by incorporating auxiliary variables that are likely to vary within blocks and for which variability is reasonably well know (e.g., topography, stand age). A number of auxiliary variables that could serve as covariates in the analysis were identified for stand age and topography. The stand age auxiliary variable data were provided by landowners and the primary data source for topographic variables was the 10-meter DEM for western Washington.[28]

When multiple correlated auxiliary variables are incorporated into a single model, the model may be affected by (multi) collinearity which has detrimental effects on estimated parameters (Quinn and Keough, 2002). To avoid problems associated with collinearity among auxiliary variables in this analysis, only one variable from each class, which was determined based on an Akaike Information Criterion (AIC) score, was included in the final model.

Stand age was evaluated as a potential covariate because, as discussed earlier, numerous studies have reported a negative relationship between landslide occurrence and stand age such that landslide density is generally less in older forests which are likely to have greater root strength and different

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[28] The 10-meter DEM is currently the best widely available source of topographic information that encompasses the entire study area, and each of the topographic auxiliary variables are derived from it. The limitations of characterizing small landforms with 10-meter DEM topography are discussed in Section 3.4.

hydrologic characteristics than relatively young forests. Models of delivering landslide density, as a function of block and stand age, also support this finding, regardless of which stand age metric is used. In linear models that include area-weighted stand age or unit age (e.g., time since harvest), the models indicate that landslide density generally decreases with increasing stand age. When treatment is added to the model, stand age (and unit age) stop being significant factors because of their collinearity with treatment.[29] When competing models of stand age were compared, it appears that treatment is a better predictor of landslide density than either of the age functions that were evaluated (Table 6-1). Although we cannot separate age and treatment effects in this study, AIC scores indicate that the treatment effect is more than a function of age alone.

Several different topographic terms were evaluated as potential covariates in response to questions about equal distribution of RIL and/or landslide hazard among treatments (within blocks). Auxiliary variables that were evaluated as covariates included mean slope, median slope, percentage of ground classified as high hazard in the WDNR SlpStab model, and percentage of ground with a slope above 65%. Most RIL are defined by slopes greater than 70%, but 10-meter DEM slopes are asymptotically lower than the field measured slope for small steep features so 65% was used as a threshold. Based on AIC scores shown in Table 6-2, the final model included median gradient as a topographic covariate. Under the structure of the model, this covariate best accounts for differences in slope gradient among treatments within each block.

The final harvest landslide count model includes treatment and median gradient as fixed effects and block as a random effect.[30] The model correlation coefficient indicated a good fit between actual and predicted landslide count (predicted vs. observed $r^2=0.95$ with approximately normally distributed residuals) and the F-test for treatment and median gradient were significant (Table 6-3), so pairwise comparisons were conducted. When block is treated as a fixed effect, block is a significant factor and

Table 6-1: Example AIC scores for models that include treatment and stand age.

Age related factors	k*	log($\mathcal{L}$)	AICc	Rank
NULL model	3	-607.8	614.0	
Treatment	7	-571.7	586.9	1
Stand age	4	-592.3	600.7	2
Unit age	4	-592.8	601.2	3

Note: Lower AIC values indicate better predictive models, as reflected by the rank values.
*Table values calculated with SAS GLIMMIX, which includes fixed effect classes in the model parameter count (k).

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[29] We present the results as though model comparison occurred as a result of adding model terms for the sake of simplicity, but the maximal model was reduced through single term deletions and in every case, reductions of the maximal model (which included treatment) resulted in the removal of explicit stand age terms.

[30] As noted in Section 4.3, the GLIMMIX model also includes random blocks, ln(area) as an offset, and area as weight.

Table 6-2: Example AIC scores for models that include different auxiliary variables related to topographic landslide hazard.

Topographic factors	k*	log($\mathcal{L}$)	AICc	Rank
NULL model	3	-607.8	614.0	
Treatment	7	-571.7	586.9	
Trt+ Median gradient	8	-553.7	571.2	1
Trt+ Mean gradient	8	-554.7	574.6	2
Trt+ Pct area > 65%	8	-558.7	576.2	3
Trt+ MedHigh SlpStab	8	-561.3	578.8	4
Trt+ High SlpStab	8	-566.3	583.8	5

Note; Lower AIC values indicate better predictive models, as reflected by the rank values
*Table values calculated with SAS GLIMMIX, which includes fixed effect classes in the model parameter count (k).

Table 6-3: Type III tests of fixed effects on landslide density for all harvest treatments.

Effect	Num DF	Den DF	F Value	Pr > F
Treatment	4	79	10.53	<.001
Slope	1	79	18.45	<.001

Note: Unbalanced GLMM with area is used as an offset and block is treated as a random effect.

it explains more residual deviance than treatment or slope (respectively). Contrasts between each pair of treatments are contained in Table 6-4. Differences are in the natural log scale and significant Tukey-Kramer contrasts (at $\alpha=0.1$) are marked with an asterisk.

Figure 6-1 shows mean landslide density, after accounting for the effect of block (e.g., precipitation) and topography with 90% confidence limits. Bars with the same letter above them are not significantly different from one another at $\alpha=0.1$. Bars that do not have the same letter are considered significantly different from one another. Model results indicate that landslide densities in the No Buffer and Partial Buffer treatments were not significantly different from one another but were significantly higher than in the Mature treatment (56% and 93% increase respectively). Partial Buffer was also significantly higher than Full Buffer and Submature. Although landslide density values differed, No Buffer, Full Buffer and Submature were not significantly different from one another. Full Buffer and Submature densities were 15 % and 25 % higher than Mature respectively, but these differences were not statistically significant.

These results are based on an unbalanced dataset because changes made during QA/QC resulted in the loss of under-represented treatments from four blocks. Critical treatments are missing from two of the four blocks and no blocks have more than one under-represented treatment. Parameter estimates from unbalanced datasets can be biased if the missing blocks have a large amount of leverage (i.e., large effect on the outcome). To avoid concerns that an analysis based on the full unbalanced dataset would yield inappropriate conclusions, a second analysis was conducted on a completely bal-

Table 6-4: Pairwise comparisons delivering hillslope landslide density incorporating all treatments and all blocks.

Linear comparisons among treatments:	Estimate*	Std. Error	t-value	Pr(>|t|)	Adj. P †	Sig.§
NB 0-20 - PB 0-20	-0.2134	0.1297	-1.64	0.104	0.474	
NB 0-20 - FB 0-20	0.3071	0.2150	1.43	0.157	0.486	
NB 0-20 - SM 21-40	0.2220	0.1620	1.37	0.175	0.522	
NB 0-20 - M 41+	0.4439	0.1269	3.5	<.001	0.007	*
PB 0-20 - FB 0-20	0.5205	0.2033	2.56	0.012	0.088	*
PB 0-20 - SM 21-40	0.4354	0.1485	2.93	0.004	0.035	*
PB 0-20 - M 41+	0.6572	0.1057	6.22	<.001	<.001	*
FB 0-20 - SM 21-40	-0.0851	0.2408	-0.35	0.725	0.789	
FB 0-20 - M 41+	0.1367	0.2079	0.66	0.513	0.789	
SM 21-40 - M 41+	0.2218	0.1345	1.65	0.103	0.471	

* Linear difference estimates and standard errors are in log base e.
† P-values are adjusted using step-down Holm-Tukey method (p-values are conservative).
§ Asterisk denotes significance at α=0.1.

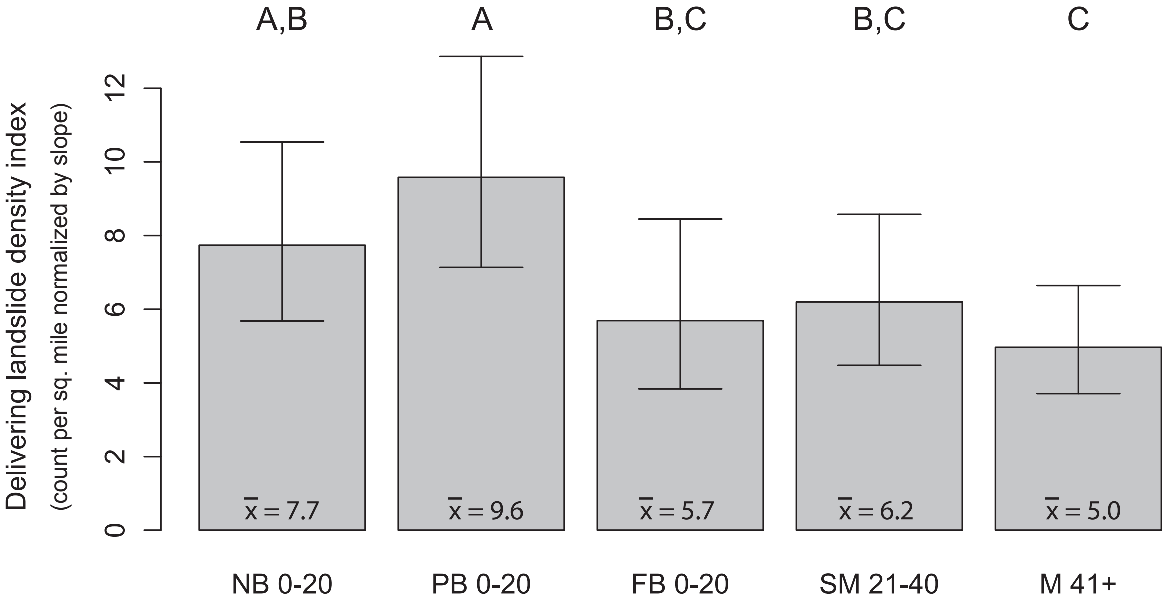

Figure 6-1: Barplot of delivering landslide density index with 90% confidence limits for all harvest treatments using all data. The term 'index' is used to denote that landslide densities are normalized to the area-weighted slope to account for topographic differences within blocks. Treatments with different letters are significantly different from one another at α=0.1.

anced subset of the data containing only critical harvest treatments.[31] This subset was analyzed using the same model as the full dataset with median slope as a covariate (predicted vs. observed r^2=0.92). The fixed effect of treatment was significant (Table 6-5) so multiple comparisons tests were conducted (Table 6-6).

As with the full model, results of this analysis indicate that No Buffer has a significantly higher landslide density than Mature (65% increase). Although landslide density values differed, No Buffer and Full Buffer treatments were not significantly different from one another (Figure 6-2). Full Buffer was intermediate to No Buffer and Mature (17% more that Mature, 30% less than No Buffer) and the confidence interval for the difference in landslide density between it and the other two critical treatments includes zero.

Table 6-7 shows the landslide density for both the balanced and unbalanced models, along with relative change versus the Mature treatment which is considered a baseline treatment in this study.

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[31] The critical harvest treatments are Full Buffer, No Buffer and Mature. Blocks 2 & 36 were removed because they did not contain any area of the No Buffer treatment.

Table 6-5: Type III tests of fixed effects on landslide density for critical harvest landslide treatments.

Effect	Num DF	Den DF	F Value	Pr > F
Treatment	2	37	6.9	0.003
Slope	1	37	14.64	<.001

Note: Balanced with respect to critical treatments. Blocks 2 & 36 dropped because they did not contain the No Buffer treatment. GLMM with area is used as an offset and block is treated as a random effect.

Table 6-6: Pairwise comparisons of landslide density for the critical harvest treatments.

Linear Hypotheses:	Estimate*	Std. Error	t-value	Pr(>|t|)	Adj. P †	Sig.§
NB 0-20 - FB 0-20	0.3437	0.2380	1.44	0.157	0.329	
NB 0-20 - M 41+	0.5004	0.1351	3.7	<.001	0.002	*
FB 0-20 - M 41+	0.1567	0.2340	0.67	0.507	0.507	

* Linear differences are calculated in base e.
† P-values are adjusted using the step-down Holm-Tukey method.
§ Asterisk denotes significance at α=0.1.

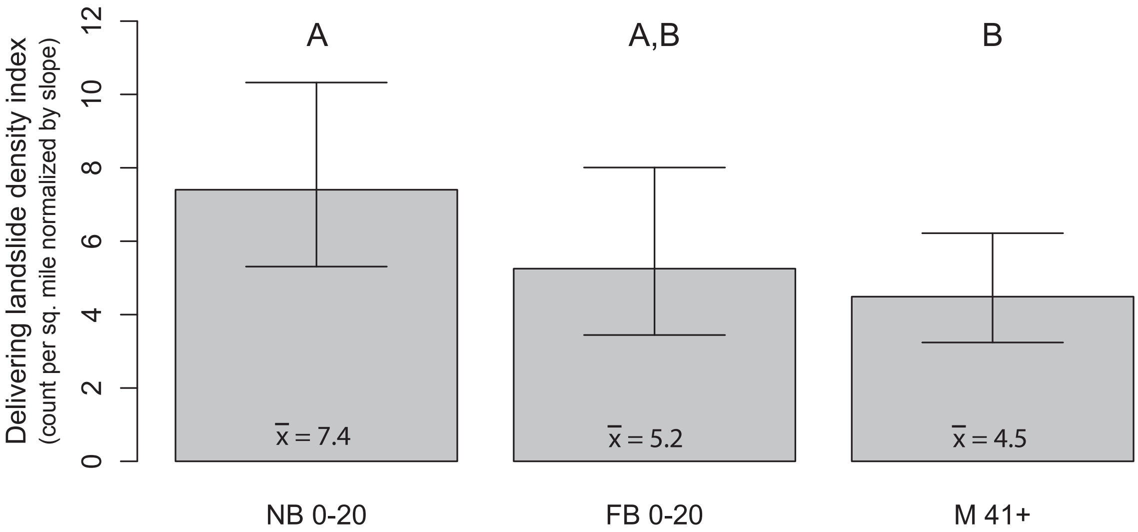

Figure 6-2: Barplot of delivering landslide density index with 90% confidence limits for critical harvest treatments based on a balanced design using data from 20 of the 22 blocks.

Table 6-7: Modeled landslide density index for hillslope landslides that delivered to public resources from an unbalanced analysis of all treatments, and a balanced set of critical treatments.

	All treatments (unbalanced)			Critical treatments (balanced design)		
Treatment	Density (slides/mi^2)	Sig. Diff.*	Ratio vs. Mature	Density (slides/mi^2)	Sig. Diff.*	Ratio vs. Mature
No Buffer (NB 0-20)	7.74	A,B	1.56	7.40	A	1.65
Partial Buffer (PB 0-20)	9.58	A	1.93			
Full Buffer (FB 0-20)	5.69	B,C	1.15	5.25	A,B	1.17
Submature (SM 21-40)	6.20	B,C	1.25			
Mature (M 41+)	4.96	C	1	4.49	B	1

Note: The all treatments analysis is based on an unbalanced dataset incorporating data from all 22 blocks. The critical treatments analysis is based on a balanced analysis with block 2 & 36 removed because No Buffer was not found in those blocks. Model estimates normalized by slope across all blocks and treatments used in the analysis.

*Treatments with different letters are significantly different based on a comparison of step-down Holm-Tukey adjusted p-values against a significance level of 0.1.

6.2 Sediment delivered from harvest treatment landslides

This analysis reports on the total amount of sediment delivered to public resources (per unit area).[32] Results are reported in terms of two different estimates of landslide size: 1) size based on the initial landslide volume (initial yield); and 2) size from field crew estimates of the volume that delivered to streams (delivered yield). Both volume estimates are incorporated into the results because field estimates of delivered volume are more subjective than initial volume and an observer variability test indicated that there was especially high variability in the estimation of delivered volume (Section 5-1), but delivered sediment is what the Forest Practices Rules seek to minimize. The sediment delivery results are further limited to shallow rapid landslides that delivered to public resources.[33]

6.2.1. Initial yield

Initial sediment yields were heavily skewed, with means greater than median and coefficients of variation (i.e., ratio of standard deviation to the mean) greater than one for all treatments (Table 5-11 and Figure 5-18). The Box-Cox transformation was employed with $\lambda=0.14$ and the resulting data were fit with a gaussian GLMM that incorporated treatment and median slope as fixed effects, and block as a random effect (predicted vs. observed transformed mass density $r^2=0.45$). F-tests based on this model indicate that there are significant differences among treatments (Table 6-8), so multiple comparisons tests were conducted (Table 6-9). Comparison tests indicate that once the effects associated with block (e.g., precipitation) and slope were accounted for, the only treatment difference that is statisti-

Table 6-8: Type III tests of fixed effects on Box-Cox transformed initial sediment yield for all harvest treatments.

Effect	Num DF	Den DF	F Value	Pr > F
Treatment	4	79	3.15	0.019
Slope	1	79	7.7	0.007

Note: Box-Cox transformation based on $\lambda=0.14$. Unbalanced GLMM with area is used as an offset and block is treated as a random effect.

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[32] Results are reported in terms of mass density (tons per acre) which is proportionately equivalent to total volume of sediment delivered per unit area.

[33] Shallow rapid landslides are debris avalanches, debris flow, and debris slides. Together they account for 916 (97.7%) of the 938 hillslope landslides that delivered to public resources.

Table 6-9: Pairwise comparisons of transformed initial sediment yield for delivering landslides incorporating all harvest treatments and blocks.

Linear comparisons among treatments:	Estimate*	Std. Error	t-value	Pr(>|t|)	Adj. P †	Sig.§
NB 0-20 - PB 0-20	1.072	1.176	0.91	0.365	0.892	
NB 0-20 - FB 0-20	2.734	1.213	2.26	0.027	0.171	
NB 0-20 - SM 21-40	3.751	1.177	3.19	0.002	0.017	*
NB 0-20 - M 41+	2.597	1.163	2.23	0.028	0.179	
PB 0-20 - FB 0-20	1.662	1.191	1.40	0.167	0.632	
PB 0-20 - SM 21-40	2.679	1.159	2.31	0.023	0.152	
PB 0-20 - M 41+	1.525	1.144	1.33	0.187	0.672	
FB 0-20 - SM 21-40	1.017	1.183	0.86	0.392	0.911	
FB 0-20 - M 41+	-0.138	1.161	-0.12	0.906	1	
SM 21-40 - M 41+	-1.154	1.143	-1.01	0.316	0.850	

* Linear difference estimates and standard errors calculated on Box-Cox transformed (λ=0.14) mass density in tons per acre.
† P-values are adjusted using step-down Holm-Tukey method (p-values are conservative).
§ Asterisk denotes significance at a=0.1.

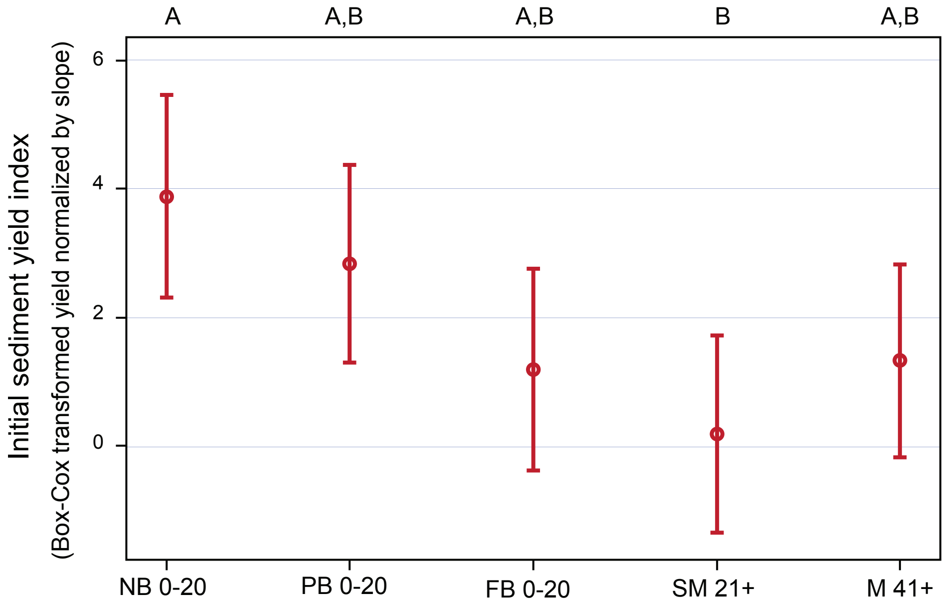

Figure 6-3: Modeled initial sediment yield for all harvest treatments. Transformed means (Box-Cox λ=0.14) are shown with 90% confidence intervals from a gaussian mixed model that incorporates treatment and slope gradient as fixed effects, and block (e.g., precipitation) as a random effect. Treatments displaying different letters (above the graph) are statistically different at α=0.1

cally significant is the one between No Buffer and Submature (p=0.017), with No Buffer having the highest initial yield and Submature the smallest yield (Figure 6-3).

As with the hillslope landslide density analysis, a second analysis was conducted on a completely balanced subset of the data containing only critical harvest treatments.[34] This subset was analyzed using the same model as the full dataset with median slope as a covariate (predicted vs. observed r^2=0.64). The fixed effect of treatment was significant (Table 6-10) so multiple comparisons tests were conducted (Table 6-11).

The multiple comparisons test results using only critical treatments differs slightly from the results using all treatments (Figure 6-4). In the unbalanced design with all treatments and all blocks, the differences in initial yield between No Buffer, Full Buffer, and Mature are found not to be statistically significant at α=0.1; but in the balanced case, Full Buffer and Mature have statistically (α<0.05) smaller yields than No Buffer. As shown in Table 6-12, the magnitude of change between Full Buffer and Mature is similar in both cases, so the differences in statistical significance are most likely related to a small reduction in the error term of the critical treatments model and different Tukey-Kramer multiple comparisons adjustments of the p-value.

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[34] The critical harvest treatments are Full Buffer, No Buffer and Mature. Blocks 2 & 36 were removed because they did not contain any area of the No Buffer treatment.

Table 6-10: Type III tests of fixed effects on Box-Cox transformed initial sediment yield for critical harvest treatments.

Effect	Num DF	Den DF	F Value	Pr > F
Treatment	2	37	4.72	0.015
Slope	1	37	4.97	0.032

Note: Balanced with respect to critical treatments. Blocks 2 & 36 dropped because they did not contain the No Buffer treatment. Box-Cox transformation based on λ=0.14. GLMM with area is used as an offset and block is treated as a random effect.

Table 6-11: Pairwise comparisons of transformed initial sediment yield for critical harvest treatments.

Linear Hypotheses:	Estimate*	Std. Error	t-value	Pr(>|t|)	Adj. P †	Sig.§
NB 0-20 - FB 0-20	3.048	1.082	2.82	0.008	0.021	*
NB 0-20 - M 41+	2.485	1.008	2.47	0.018	0.047	*
FB 0-20 - M 41+	-0.563	1.049	-0.54	0.595	0.854	

* Linear difference estimates and standard errors calculated on Box-Cox transformed (λ=0.14) mass density in tons per acre.
† P-values are adjusted using the step-down Holm-Tukey method.
§ Asterisk denotes significance at a=0.1.

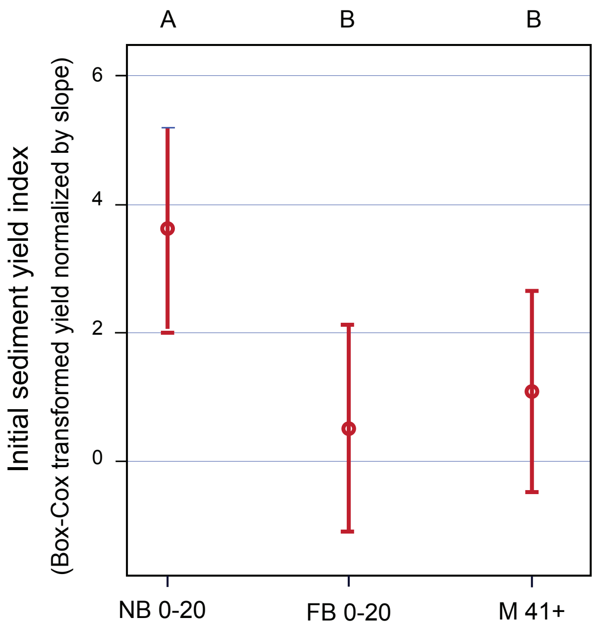

Figure 6-4: Modeled initial sediment yields for critical treatments in a balanced design. Transformed means (Box-Cox λ=0.14) are shown with 90% confidence intervals from a gaussian mixed model that incorporates treatment and slope gradient as fixed effects, and block (e.g., precipitation) as a random effect. Treatments displaying different letters (above the graph) are statistically different at α=0.1

Table 6-12: Modeled initial sediment yield index for hillslope landslides that delivered to public resources from the unbalanced dataset using all treatments and blocks and using only critical treatments in balanced design.

	All treatments (unbalanced)			Critical treatments (balanced design)		
Treatment	Init. yield (tons per acre*)	Sig. Diff.*	Ratio vs. Mature	Init. yield (tons per acre*)	Sig. Diff.*	Ratio vs. Mature
No Buffer (NB 0-20)	22.2	A	6.61	18.2	A	6.59
Partial Buffer (PB 0-20)	10.8	A,B	3.22			
Full Buffer (FB 0-20)	3.0	A,B	0.89	1.7	B	0.60
Submature (SM 21-40)	1.2	B	0.36			
Mature (M 41+)	3.4	A,B	1	2.8	B	1

Note: The all treatments analysis is based on an unbalanced dataset incorporating data from all 22 blocks. The critical treatments analysis is based on a balanced analysis with block 2 & 36 removed because No Buffer was not found in those blocks. Model estimates normalized by slope across all blocks and treatments used in the analysis. Index values have been back-transformed into the original scale using Box-Cox with λ=0.14.
*Treatments with different letters are significantly different based on a comparison of step-down Holm-Tukey adjusted p-values against a significance level of 0.1.

6.2.2. Delivered sediment yield by block and treatment

Delivered sediment yields were generally smaller than initial yields (Table 5-12). Delivered yield exhibits the same distribution as initial yield (Figure 5-19) and the SAS Box-Cox method suggested λ=0.13 for the normalizing transformation. Transformed data were fit with a gaussian GLMM that incorporated treatment and median slope as fixed effects, and block as a random effect (predicted vs. observed transformed mass density r^2=0.51). F-tests based on this model indicate that there are significant differences among treatments (Table 6-13), so multiple comparisons tests were conducted (Table 6-14). The resulting model indicated that the only statistically significant difference among the treatments was between No Buffer and Submature treatment (p=0.085, Figure 6-5) which is consistent with the finding based on initial volume when using all treatments in the unbalanced design.

Table 6-13: Type III tests of fixed effects on Box-Cox transformed delivered sediment yield for all treatments.

Effect	Num DF	Den DF	F Value	Pr > F
Treatment	4	79	2.14	0.083
Slope	1	79	13.82	<.001

Note: Box-Cox transformation based on λ=0.13. Unbalanced GLMM with area is used as an offset and block is treated as a random effect.

Table 6-14: Pairwise comparisons of transformed delivered sediment yield incorporating all harvest treatments and blocks.

Linear comparisons among treatments:	Estimate*	Std. Error	t-value	Pr(>|t|)	Adj. P †	Sig.§
NB 0-20 - PB 0-20	0.701	1.100	0.64	0.526	0.969	
NB 0-20 - FB 0-20	2.118	1.138	1.86	0.066	0.347	
NB 0-20 - SM 21-40	2.831	1.100	2.57	0.012	0.085	*
NB 0-20 - M 41+	1.953	1.087	1.80	0.076	0.383	
PB 0-20 - FB 0-20	1.417	1.117	1.27	0.208	0.711	
PB 0-20 - SM 21-40	2.131	1.083	1.97	0.053	0.292	
PB 0-20 - M 41+	1.253	1.070	1.17	0.245	0.768	
FB 0-20 - SM 21-40	0.714	1.108	0.64	0.522	0.967	
FB 0-20 - M 41+	-0.164	1.088	-0.15	0.880	1	
SM 21-40 - M 41+	-0.878	1.069	-0.82	0.414	0.923	

* Linear difference estimates and standard errors calculated on Box-Cox transformed (λ=0.13) mass density in tons per acre.
† P-values are adjusted using step-down Holm-Tukey method (p-values are conservative).
§ Asterisk denotes significance at a=0.1.

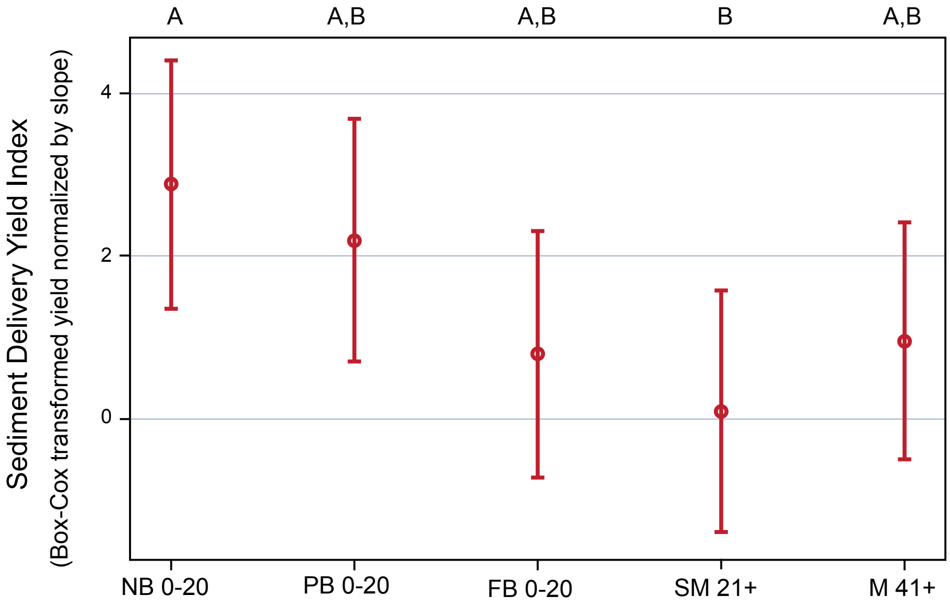

Figure 6-5: Modeled delivered sediment yields for all harvest treatments. Transformed means (Box-Cox λ=0.13) are shown with 90% confidence intervals from a gaussian mixed model that incorporates treatment and slope gradient as fixed effects, and block (e.g., precipitation) as a random effect. Treatments displaying different letters (above the graph) are statistically different at α=0.1

As with the other hillslope analyses, a second analysis was conducted on a completely balanced subset of the data containing only critical harvest treatments. This subset was analyzed using the same model as the full dataset with median slope as a covariate (predicted vs. observed transformed mass density r^2=0.76). The fixed effect of treatment was significant (Table 6-15) so multiple comparisons tests were conducted (Table 6-16).

As with the initial yields, comparisons based on critical treatments in a balanced design provide slightly different results with regard to statistical significance (Figure 6-6). In the unbalanced design with all treatments and all blocks, the differences in initial yield between No Buffer, Full Buffer, and Mature are found not to be statistically significant at α=0.1; but in the balanced case, Full Buffer and Mature have statistically (α<0.05) smaller yields than No Buffer. As with initial yield, the magnitude of the difference in estimated sediment yield between Full Buffer and Mature is slightly less for the critical treatment analysis even though the critical treatment analysis is the one which the differences are statistically significant. This indicates that differences in the significance test are most likely related to a small reduction in the error term in the critical treatments model and less conservative Tukey-Kramer multiple comparisons adjustment of the p-value.

Table 6-15: Type III tests of fixed effects on Box-Cox transformed delivered sediment yield for critical harvest treatments.

Effect	Num DF	Den DF	F Value	Pr > F
Treatment	2	37	3.58	0.038
Slope	1	37	13.3	<.001

Note: Balanced with respect to critical treatments. Blocks 2 & 36 dropped because they did not contain the No Buffer treatment. Box-Cox transformation based on λ=0.13. GLMM with area is used as an offset and block is treated as a random effect.

Table 6-16: Pairwise comparisons of transformed initial sediment yield for critical harvest treatments.

Linear Hypotheses:	Estimate*	Std. Error	t-value	Pr(>|t|)	Adj. P †	Sig.§
NB 0-20 - FB 0-20	2.167	0.8996	2.41	0.021	0.054	*
NB 0-20 - M 41+	1.827	0.8263	2.21	0.033	0.082	*
FB 0-20 - M 41+	-0.3395	0.8669	-0.39	0.698	0.919	

* Linear difference estimates and standard errors calculated on Box-Cox transformed (λ=0.13) mass density in tons per acre.
† P-values are adjusted using the step-down Holm-Tukey method.
§ Asterisk denotes significance at α=0.1.

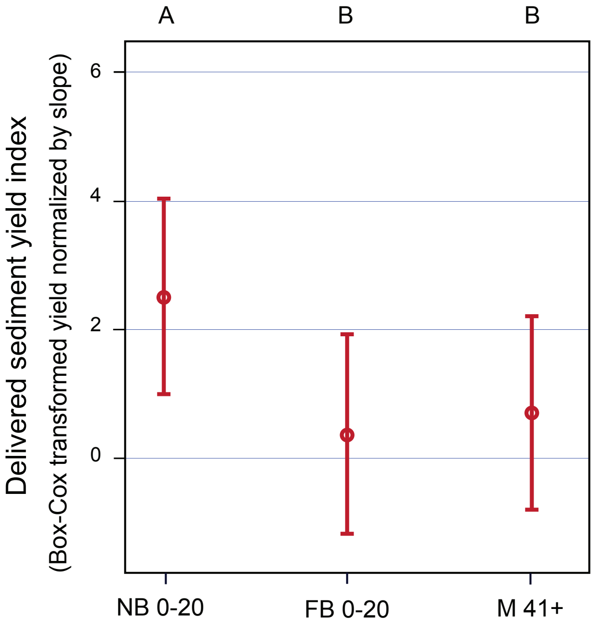

Figure 6-6: Modeled delivered sediment yields for critical treatments in a balanced design. Transformed means (Box-Cox λ=0.13) are shown with 90% confidence intervals from a gaussian mixed model that incorporates treatment and slope gradient as fixed effects, and block (e.g., precipitation) as a random effect. Treatments displaying different letters (above the graph) are statistically different at α=0.1

Table 6-17: Modeled delivered sediment yields for hillslope landslides that delivered to public resources from the unbalanced dataset using all treatments and blocks and using only critical treatments in balanced design.

	All treatments (unbalanced)			Critical treatments (balanced design)		
Treatment	Delivered (tons/acre)	Sig. Diff.*	Ratio vs. Mature	Delivered (tons/acre)	Sig. Diff.*	Ratio vs. Mature
No Buffer (NB 0-20)	11.6	A	4.70	8.7	A	4.48
Partial Buffer (PB 0-20)	6.9	A,B	2.80			
Full Buffer (FB 0-20)	2.1	A,B	0.86	1.4	B	0.73
Submature (SM 21-40)	1.1	B	0.44			
Mature (M 41+)	2.5	A,B	1	1.9	B	1

Note: The all treatments analysis is based on an unbalanced dataset incorporating data from all 22 blocks. The critical treatments analysis is based on a balanced analysis. Model estimates normalized by slope across all blocks and treatments used in the analysis. Index values have been back-transformed into the original scale using Box-Cox with λ=0.13.

* Treatments with different letters are significantly different based on a comparison of step-down Holm-Tukey adjusted p-values against a significance level of 0.1.

6.3 Road landslide density

Road landslide counts did not meet the distribution assumptions of parametric statistical tests so differences in road landslide density were evaluated with a non-parametric Friedman test. The Friedman test accounts for block effects but requires a completely balanced dataset. The study design (Dieu et al., 2008) allowed for missing road non-critical treatments, so Friedman tests were conducted on two different subsets of the data: 1) a set containing the three critical road treatments using all blocks (n=22); and 2) a set including the 17 blocks that contained all road treatments. Neither analysis detected differences in road landslide density among treatments that were statistically significant (p=0.48 and p=0.23 respectively).

6.4 Sediment delivered from road treatment landslides

As with road landslide density, comparisons of sediment yield among treatments were conducted with two different subsets of the data: 1) critical treatments in all blocks; and 2) all treatments in the subset of blocks in which they were all found. The first set of comparisons is likely to be more powerful because it has a larger sample size, but the second set gives some indication about the relative ranking of all treatments. These analyses were conducted for both initial and delivered yield because initial yield is a less subjective estimate and an observer variability test indicated that there was especially high variability in the estimation of delivered volume (Section 5-1), but Forest Practices Rules focus on delivered yield.

When all treatments were evaluated together with only the subset of blocks, the Substandard initial sediment yield was significantly higher than Mitigated. In this analysis, the Abandoned treatment had significantly smaller initial yields than Standard, Substandard, or Orphaned roads (top of Table 6-18).

Initial sediment yield results from the critical road treatments including all blocks indicated that the Standard treatment had a significantly higher initial sediment yield than the Mitigated treatment (bottom of Table 6-18), but Substandard, despite having intermediate initial yield values, was not significantly different from the Standard or Mitigated.

Table 6-18: Results from a non-parametric Friedman test on initial sediment yield for road landslides incorporating all treatments in a subset of blocks (top) and only critical treatments (bottom).

Friedman test		Treatment	Sum of ranks	Significance*
ChiSq	12.7	Standard	62.5	A
df	4	Substandard	57	A
p-Friedman	0.013	Orphaned	52.5	A,B
LSD	11.8	Mitigated	44.5	B,C
n	17	Abandoned	38.5	C

*Treatments with the same letter are not significantly different at $\alpha=0.1$.

Friedman test		Treatment	Sum of ranks	Significance*
ChiSq	5.41	Standard	50	A
df	2	Substandard	44.5	A,B
p-Friedman	0.067	Mitigated	37.5	B
LSD	8.7			
n	22			

Note: This is the more powerful test for assessing differences among the critical treatments.
*Treatments with the same letter are not significantly different at $\alpha=0.1$.

Delivered sediment yield does not follow the same pattern as initial sediment yield. When all treatments were evaluated together including only the subset of blocks, Standard and Substandard had a significantly higher delivered sediment yield than Mitigated and Abandoned while the Orphaned treatment was not statistically different from any of the other groups (top of Table 6-19). An analysis of the three critical road treatments (bottom of Table 6-19) using all blocks revealed no statistically significant differences in delivered volume.

Table 6-19: Results from a non-parametric Friedman test on delivered sediment for road landslides incorporating all treatments in a subset of blocks (top) and critical treatments (bottom).

Friedman test		Treatment	Sum of ranks	Significance*
ChiSq	8.55	Standard	58.5	A
df	4	Substandard	58	A
p-Friedman	0.073	Orphaned	52.5	A,B
LSD	12.3	Mitigated	45.5	B
n	17	Abandoned	40.5	B

*Treatments with the same letter are not significantly different at $\alpha=0.1$.

Friedman test		Treatment	Sum of ranks	Significance*
ChiSq	3.14	Standard	47	A
df	2	Substandard	46.5	A
p-Friedman	0.208	Mitigated	38.5	A
LSD	8.9			
n	22			

Note: This is the more powerful test for assessing differences among the critical treatments.
*Treatments with the same letter are not significantly different at $\alpha=0.1$.

SECTION 7: DISCUSSION

This chapter begins with responses to the five "critical questions" that the Post-Mortem Project was designed to answer (Dieu et al., 2008). Section 7.2 follows with a description of study limitations and a discussion of factors that are relevant to the interpretation of the study findings. Section 7.3 is focused on unexpected results with particular relevance to forest practices.

7.1 Responses to critical questions

Critical Question 1: "Are the Forest Practices Rules effective in reducing the numbers and volume of sediment delivered by management-induced landslides?"

Review of Forest Practices Rules for Reducing Landslides

As discussed in Section 1.2, the Forest Practices Rule methods for reducing landslide occurrence are embodied in Washington Administrative Code (WAC). WAC 222-16-050 requires that timber harvest or road building on RIL that have the potential to deliver sediment or debris to a public resource, receive additional State Environmental Policy Act (SEPA) review during permitting. If it is determined that a proposed activity is likely to have an adverse impact, WAC 222-10-030 requires that specific mitigation measures be designed to avoid accelerating the rate or magnitude of mass wasting that could deliver sediment or debris to a public resource. The performance target for harvest-related landslides is "Virtually none triggered by new harvesting on high risk sites verified per Report criteria" (U.S.F.W.S. et al., 1999). 'High risk sites verified per Report criteria' are interpreted here to be the named RIL defined in WAC 222-16-050.

The Forest Practices Rule strategy for road stability is embodied in WAC 222-24-010. Forest Practices Applications that propose road construction on RIL undergo additional SEPA review just like proposals to harvest on RIL (WAC 222-16-050). For existing roads owned by large landowners, WAC 222-24-050 requires that they be improved and maintained to the standards of WAC 222-24-052 or abandoned by July 1, 2016. Many of the road construction and maintenance rules in WAC 222-24 are related to drainage ditches, culverts, fill compaction, sidecast, and other factors that influence landslide occurrence. The two Schedule L-1 performance targets are for a 'favorable trend' in landslide rates from 'old' (pre-2001 construction) roads and for "virtually no new landslides triggered by 'new' (post-2001 construction) roads."

Effectiveness for hillslope landslides

This study attempted to test the effectiveness of the most common harvest strategy for reducing landslides, which is to leave forested areas, called buffers, on all RIL located within or adjacent to harvest areas. In part, because RIL buffers could not be distinguished from riparian or other buffers during treatment delineation, the study tested the combined effectiveness of all types of buffers at reducing landslide initiation and sediment delivery. Although harvest within RIL can be performed following SEPA review in some cases, this study did not attempt to evaluate the effectiveness of this type of harvest activity, as it was considered to be infeasible at the broad geographic scale of this study. Thus, the Full Buffer treatment is viewed as most closely approximating the current regulatory approach.

Although many of the results explained in Chapters 5 and 6 can inform questions related to rule effectiveness, the most directly relevant are comparisons of landslide metrics within the two critical harvest treatments – No Buffer and Full Buffer – relative to Mature forest, which serves as a baseline (Section 2.2). Results from analyses of the three landslide metrics among the three critical harvest treatments are summarized in Table 7.1.

Once the effect of sample block (largely a surrogate for precipitation intensity) and slope gradient were accounted for in the analysis, the No Buffer treatment had a significantly higher landslide density (a 65% increase) than Mature forest. The Full Buffer treatment had a landslide density that was intermediate to Mature and No Buffer (17% more than Mature, 30% less than No Buffer) but not statistically different from either. For the two sediment volume metrics – Initial Yield and Delivery volume - No Buffer delivered significantly more sediment than either Mature or Full Buffer (347% and 558% increase respectively). In contrast, Full Buffer delivered sediment volumes that were lower than, but were not statistically different from, Mature (Table 7.1).

These findings indicate that harvest without buffers (i.e., No Buffer) resulted in a larger number of delivering landslides and greater volume of sediment delivery than would be expected in Mature forest. In contrast, Full Buffer resulted in a landslide volume that was similar to Mature, but a density that was not statistically different than No Buffer or Mature. This indicates that complete buffering is effective at reducing sediment volumes, but has an indeterminate effect on landslide density. All these comparisons are subject to the interpretation issues noted in Section 7.2.

As a final observation, across the Post-Mortem study area, the FFR performance target "Virtually none triggered by new harvesting on high risk sites . . ." does not appear to have been achieved for the period of 2001-2007. Forty-seven delivering landslides initiated in RIL harvested under the current Forest Practices Rules. This result is discussed more fully in Section 7.3.3.

Table 7-1: Summary of differences between No Buffer and Full Buffer treatments relative to Mature, based on pair-wise analyses of critical harvest treatments. Further statistical details are provided in Section 6.

	No Buffer		Full Buffer	
Landslide metric	Significantly different than	Ratio vs. Mature	Significantly different than	Ratio vs. Mature
Density	Mature	1.65	N/A	1.17
Initial yield	Mature and Full Buffer	6.58	No Buffer	0.60
Delivered yield	Mature and Full Buffer	4.47	No Buffer	0.73

Note: Summary of data presented in Tables 6-7, 6-12, and 6-17.

Effectiveness for road-related landslides

Similar to the harvest treatments, the most relevant test of Forest Practice Rule effectiveness for roads is the comparison of landslide metrics within the two critical road treatments that meet rules – Standard and Mitigated – relative to Substandard roads. Differences among the three critical road treatments (Standard, Substandard, and Mitigated) are statistically inconclusive for all metrics. In Section 7.3.2, we comment on factors that may have contributed to the lack of statistically significant differences among the critical road treatments.

Although not included among critical road treatments in the study design, Abandoned roads represent a third road category that meets the Forest Practices Rules. Results indicate that Abandoned roads generated less sediment than all other road treatments besides Mitigated, and it delivered less sediment to public resources than was observed on Standard or Substandard roads (Tables 6-18 and 6-19). The landslide density for Abandoned roads is also lowest of all road treatments (Table 5-17), although differences in landslide density among the five road treatments were not statistically significant.

Critical Question 2: "Is the greatest proportion of landslide delivery from harvest units or roads?"

Hillslope landslides account for the greatest proportion (81%) of the delivering landslides, and they contributed a greater proportion (77%) of sediment to public resources than roads (Table 7-2). This finding may appear to contrast with the numerous studies which have reported that roads contribute as much or more sediment than forested areas (e.g., Swanson and Dyrness, 1975; Amaranthus et al., 1985; Guthrie, 2002). Further discussion of this difference and possible causes are included below in Section 7.3.1.

Table 7-2: Relative proportion of delivering landslides and sediment delivery for roads and non-road areas from the core of each block.

Metric	Active road	Inactive road	Harvest < 20 y.o.*	Harvest > 20 y.o.	Roads	Hillslope
Study area	6%	1%	43%	50%	7%	93%
Landslide count	15%	4%	39%	41%	19%	81%
Sediment volume	17%	6%	18%	60%	23%	77%
Landslide density	30%	49%	11%	10%	76%	24%
Sediment per unit area	28%	56%	4%	12%	80%	20%

Note: Summary of data presented in Table 5-9.

Critical Question 3: "Which harvest unit prescriptions or road improvements are performing well? Which are performing poorly?"

The decision to focus the sampling design on 'treatments' rather than individual prescriptions makes it difficult to identify which individual prescriptions are performing well and which are performing poorly. Every segment of forest road has experienced a sequence of overlapping site-scale prescriptions (e.g., grading, addition of culverts), while harvest treatments are defined primarily by RIL buffering. Thus, the treatments were designed to capture the combined effect of multiple prescriptions. Some generalizations can be made, however.

Harvest prescriptions

As discussed in response to Critical Question 1, retaining buffers on RIL was found to reduce the volume of sediment delivered to public resources and it also increased the probability that landslides would deliver beneficial woody debris to streams (Figure 5-14). Still, a substantial number of landslides originated within buffers and mature forests, indicating that forest cover does not entirely prevent landslides in a large storm event. As discussed in Section 7.2.5, although inconclusive, there is evidence to suggest that the effectiveness of RIL buffering is greatest when all RIL are buffered, yet we found that many RIL had been clearcut harvested since 2001 (Section 5.2.8). This indicates possible implementation difficulties with RIL identification and/or buffer implementation, as discussed further in Section 7.3.3. A potentially useful study would be to determine the relative effectiveness of buffers among the RIL landforms (Appendix B.6).

Road prescriptions

Results did support the stabilizing effect of road abandonment, which involves removal of culverts and any unstable fill material, thus addressing road components widely observed to contribute to road failures in the Pacific Northwest (Sidle 1985). The addition of water bars and other new drainage points also likely made Abandoned roads less vulnerable to ditch and drainage crossing obstructions that occurred during the large storm. Such drainage problems commonly trigger landslides within the road prism and hillslopes below that receive road-diverted runoff. Mitigated roads are likely to have similarly benefited from unstable fill removal and upgraded drainage features. Identifying which of these specific prescriptions contributed most to the collective success of Abandoned and Mitigated roads would require a site-scale experimental study (Appendix B.5).

Critical question 4: "What are the site-scale triggering mechanisms for landslides?"

Crews looked for and recorded any evidence of triggering mechanisms, termed 'contributing factors' during field visits to landslide initiation points. With hillslope landslides, crew members looked for evidence of 1) soil disturbance from logging, 2) forest stand management activities such as herbicide treatment or pre-commercial thinning, or 3) focused surface water delivery from roads; each of these activities has been identified as contributing to landslides in the past. In this study, crew members cited one or more of the listed factors as contributing to landslide initiation at only 10% of the sites, while another 4% were associated with management-related factors like windthrow along buffer edges (Table 5-6 and Section 5.3). These calls were largely corroborated by the observer variability team (Table 5-2), leading us to conclude that the listed activities seldom contributed to landslide initiation.

Similarly, field crews identified no obvious drainage contributing factor at 68% of the hillslope road failures (Table 5-7). This absence was more unexpected than that lack of contributing factors for hillslope landslides because the authors had previously observed that road failures commonly exhibit clear evidence of a contributing maintenance problem or drainage malfunction. Further, few of the road failure sites had evidence of post-storm repairs prior to data collection that could have destroyed evidence. Among the road landslides that had an identified contributing factor, the majority (about two-thirds) involved an outsloped tread and/or water delivery through a cross-drain. Both factors contributed to concentration of road runoff at the failure sites. Crews identified plugged pipes as contributing to 68% of the stream crossing failures and fill edge collapse without plugging in another 11% of the failures. For 21% of the stream-crossing failures, the field crews could not determine a contributing factor (Table 5-8). As with hillslope landslides, these findings were largely corroborated by the observer variability team.

Because these calls appear to be sound, we conclude that many road failures were caused by factors inherent to the treatments. Most road failures involved side-casted fill placed during initial road construction. Given that construction of every forest road segment involved the modification of the pre-existing hillslope geometry (i.e., hillslope cut and fill) and water movement pathways, we are inclined to conclude that the large precipitation input was, by itself, sufficient to disrupt the stability of roads at certain vulnerable locations, even without an evident drainage problem. The strong concentration of road landslides in blocks with highest precipitation (Figures 5-25 and 5-26) supports the importance of precipitation in road landslide initiation. The relationship of road failures to road characteristics is another possible direction for further analysis, as discussed in Appendix B.5.

Critical question 5: "Do those triggering mechanisms vary between harvest unit or road types?"

The triggering mechanisms (i.e., contributing factors) were not identified frequently enough (~6% of all landslides had contributing factors) during field data collection to allow for meaningful comparison between harvest or road types.

7.2 Limitations and factors affecting interpretation of results

As with any field-based landslide study, there are limitations and factors that influence the interpretation of the results. In this section, we identify key constraints imposed by the choice of study design and discuss factors that may have affected our findings.

7.2.1. Scope of inference

This study is based on landslide response to a single large storm event. Data collection was limited to managed forest lands in southwest Washington with a landslide density of at least four landslides per square mile. The population to which we can draw inference is therefore limited to similarly managed forests with similar climatic, geomorphic and land management histories; and a storm intensity that is able to generate a significant population of landslides over a large area.

A single large storm

As noted above, this study is based on the landslide response to a single large storm event. While the study includes spatial replication across a range of storm intensities, there is no temporal replication and it is possible that the findings are not representative of other storm events.

Studies indicate that the largest relative changes in soil water pore pressure (an important factor in landslide initiation) are likely to occur in small and moderate storms (Dhakal and Sidle, 2004b) and the hydrologic effects of forest harvest on peak flow generation are likely to diminish with increasing event magnitude and time since harvest (Moore and Wondzell, 2005). As a result, one might expect management influences to be 'drowned out' in blocks experiencing very high storm intensities, thereby reducing the power of the study to detect differences among treatments.

However, in a separate study conducted in the same area and in response to the same storm, Turner et al. (2010) compared the landslide response among three stand age and seven rainfall intensity categories and found the largest differences in landslide density among stand age categories to occur at the highest rainfall intensities, which led them to conclude that the effect of stand age is strongest at the highest rainfall intensities. As a result, one might expect confounding effect of stand age (as discussed in Section 7.2.4) to be especially pronounced in blocks with the highest precipitation intensities.

Similar results were found in another study, Reid and Page (2002), which reported that the difference in landslide rates between pasture land and forest or scrub cover in New Zealand were greatest at the highest storm intensities, leading them to conclude that the effectiveness of a forest or scrub cover for controlling landslides appears to increase with storm magnitude. As a result, one might expect the influence of vegetation cover to increase with increasing storm intensity.

Finally, simulation studies indicate that the greatest percentage of unstable ground over time may be associated with years where there are many small densely distributed rainfall events as opposed to a single large magnitude event (Gorsevski et al., 2006). As a result, it possible that the patterns observed in this event are different from patterns observed in longer term studies where the effect of precipitation intensity on landslide occurrence is less variable.

Long-term landslide rates

The results of this study are not applicable to, nor were they intended to determine, long-term landslide rates. In addition to the issue described above, event-based studies like this one typically have densities that are much greater than longer-term studies because there are long periods of little or no landslide activity between storms, and dividing storm densities by the period of record reduces the overall rate. In addition, landslide detection probabilities decrease rapidly following a storm event as active roads are repaired and forest vegetates. Long-term studies also generally involve air photos analysis, which is likely to significantly underestimate landslide occurrence, especially under forest canopies (Brardinoni et al., 2003; Miller and Burnett, 2007; Turner et al., 2010).

Comparisons to landslide rates in unmanaged forest

Finally, we make no inference to landslide occurrence in unmanaged forest because the study area did not incorporate any significant areas of unmanaged (i.e., old-growth) forest. A proposal to expand the study area further north into the Olympic Mountains to include unmanaged forests was considered but it was ultimately rejected. Reasons for not including it in this study were that it did not fit within the blocking design and that the areas of unmanaged forest affected by the December 2007 storm have greater topographic relief than the Post-Mortem study area as a whole.

7.2.2. Controlling for variability with blocking

Because the comparisons among harvest and road treatments are a key component of this study, the results hinge to some degree upon the accuracy and consistency of treatment delineations. An unavoidable difficulty in comparative landslide studies, such as this, is that they take place across a wide range of inherent instability. Landslides result from a complex inter-dependent set of spatio-temporal processes that include hydrology (rainfall, evapotranspiration and groundwater), root strength, soil conditions, topography, and human impacts (Wu and Sidle, 1995). Although such complexity cannot be controlled, it does need to be managed for so that potentially confounding effects are minimized.

In this study, a randomized block design was used to account for large-scale spatial variability of external factors affecting landslide occurrence. A large land base was incorporated in order to average out site-specific variability. With the block design, it is assumed that conditions within blocks are homogeneous relative to the variability seen across blocks and that by limiting comparisons to other treatments in the same block, the influence of large-scale variation in factors affecting landslides can be controlled for.

Controlling for the variation in precipitation and topography were considered very important since both have pronounced effects on landslide initiation. While topography can be inferred from Digital Elevation Models (DEM), precipitation is much harder to control for because it is both temporally and spatially highly variable. In a retrospective study like this, it is not possible to set up local precipitation gages prior to the event in order to accurately measure precipitation. It is also not possible to accurately estimate precipitation from gage data at monitoring stations that typically are many kilometers away from the study sites (Minder et al., 2009).

With the block design, we assume that precipitation intensity within a given 4-square mile block is relatively uniform, and that because the treatments are quasi-randomly distributed within blocks, any differences in rainfall intensity among treatments within a block are averaged out over the 22 blocks. Spatial variability within blocks contributes to lower power in statistical tests, as opposed to introducing bias among treatments. Because of our familiarity with the relatively homogenous landscape of the study area, we believe that the blocking approach was reasonably effective at controlling large scale spatial variation in factors like precipitation, species composition, soils, and geology which may have affected the landslide response.

7.2.3. The distribution of RIL among harvest treatments

As noted above, this study was designed to identify treatments by differences in buffering while attempting to minimize other differences affecting landslide susceptibility. Defining harvest treatments by their buffering has the potential to introduce topographic bias. Specifically, the Partial Buffer treatment, by definition, requires that at least one RIL must be present, while the other treatments do not. This may have created a condition in which the Partial Buffer treatment may have higher inherent susceptibility than other treatments, which could contribute to a greater landslide response.

To account for potential susceptibility differences in inherent instability among the harvest treatments, including the potential RIL bias associated with the Partial Buffer treatment, we quantitatively adjusted landslide rates using a simple index based on median harvest unit slope gradient as discussed in Section 6.1.1 (Table 6-2). Slopes were calculated using a 10-meter DEM and then summarized by treatments within each block. Although there are no data to determine the effectiveness of this design to minimize the effects of landscape differences between harvest treatments, the slope factor was highly significant in the analysis and we are confident that a ‘block and adjust’ approach was appropriate to these data. The degree to which the slope index fully captured differences in susceptibility among treatments remains unknown.

7.2.4. Factors affecting landslide density among the three 0-20 harvest treatments

It was expected that the pattern of landslide response among the three 0-20 harvest treatments would differ as a result of differences in leave tree density and corresponding differences in root strength in unstable areas. We predicted the highest landslide density to occur in the No Buffer treatment, with lower densities in Partial Buffer, and the lowest in the Full Buffer treatment. As described in Section 7.1, effectiveness was to be gauged by comparing landslide densities for the various treatments with Mature stands (Dieu et al., 2008). While the landslide density for No Buffer and Full Buffer appears to follow the predicted pattern, the higher densities in Partial Buffer did not, and the literature related to factors affecting slope stability provides some possible explanations for this result.

Effects of root strength and hydrology

Slope stability is determined by ratio of driving to resisting forces, and forest vegetation management affects those by changing patterns of windthrow, vegetation surcharge, root strength, soil moisture distribution and soil pore pressure (O’Loughlin, 1974). Simulation studies indicate that the areas of highest instability result from the combined effects of high pore water pressure, low root strength, and local site conditions (Wu and Sidle, 1995). Changes in root cohesion are often modeled in terms of two factors: 1) the exponential decay of residual root strength, and 2) a sigmoidal recovery of rooting strength and tree surcharge as vegetation returns (Sidle, 1991; Sidle, 1992). Total root strength is believed to be at a minimum between approximately 4 and 10 years after timber harvest (Schmidt et al., 2001). Within this general pattern, local differences in vegetation species composition and distribution have a large effect on localized root strength and total time to recovery (Schmidt et al., 2001; Roering et al., 2003).

While root strength provides resistance against landslide initiation, landslides do not only occur in response to reductions in root strength associated with forest management. As demonstrated in this study, a large number of landslides initiate in mature stands, or in leave buffers with mature timber, in response to heavy precipitation (Figure 5-6 and Figure 5-12). Hydrology (specifically soil pore pressure) is the primary driver and forest management affects soil moisture and soil pore pressure through changes in changes in rainfall interception, smoothing of precipitation intensities, snow accumulation and snowmelt (Keim and Skaugset, 2003; Moore and Wondzell, 2005).

While changes in root strength are expected to decrease slope stability for a period before recovery, the hydrologic changes are expected to be greatest just after harvest and then declining with time (Sidle, 1991; More and Wondzell, 2005). The result, as shown by recent landslide inventories, is that landslide densities are expected to increase for the first 10 years following clearcut harvest before gradually decreasing (Figure 7-1).

Harvest unit stand age differences for the 0-20 year-old treatments

The three 0-20 year-old treatments were chosen to cover the period of increased landslide hazard and it was expected that implementation of Watershed Analysis prescriptions in the late 1990's would have created older examples of the Full Buffer and Partial Buffer treatments to compare with the No Buffer treatment (Section 2.2). While there was some overlap in the stand age of 0-20 year old treatments (Figure 5-20), the mean ages (excluding the stand age of buffers) differ significantly (Full Buffer – 3.3 years, Partial Buffer 5.8, No Buffer – 12.7). Attempts were made to incorporate stand age into the statistical models, but as discussed in Section 6.1.1, the models would not accept treatment and stand age together because they were so highly correlated, and treatment was a more appropriate factor than stand age in a linear model.

As a result, landslide response for the three 0-20 buffer treatments is potentially confounded by 'time since harvest' which complicates interpretations of buffer effectiveness (Figure 7-1). The landslide density of the three 0-20 year-old treatments fits the observed pattern of increased landslide rates, as a function of harvest unit stand age, that has been found by others (e.g., Imaizumi et al., 2008; Turner et al., 2010). It appears likely that higher landslide susceptibility associated with 'time since harvest' contributed to the higher landslide density in the Partial Buffer treatment (see Section 7.2.5 for details). In the interpretations presented in Section 7.1, it is assumed that Full Buffer and No Buffer are equally affected by the effects associated with time since harvest.

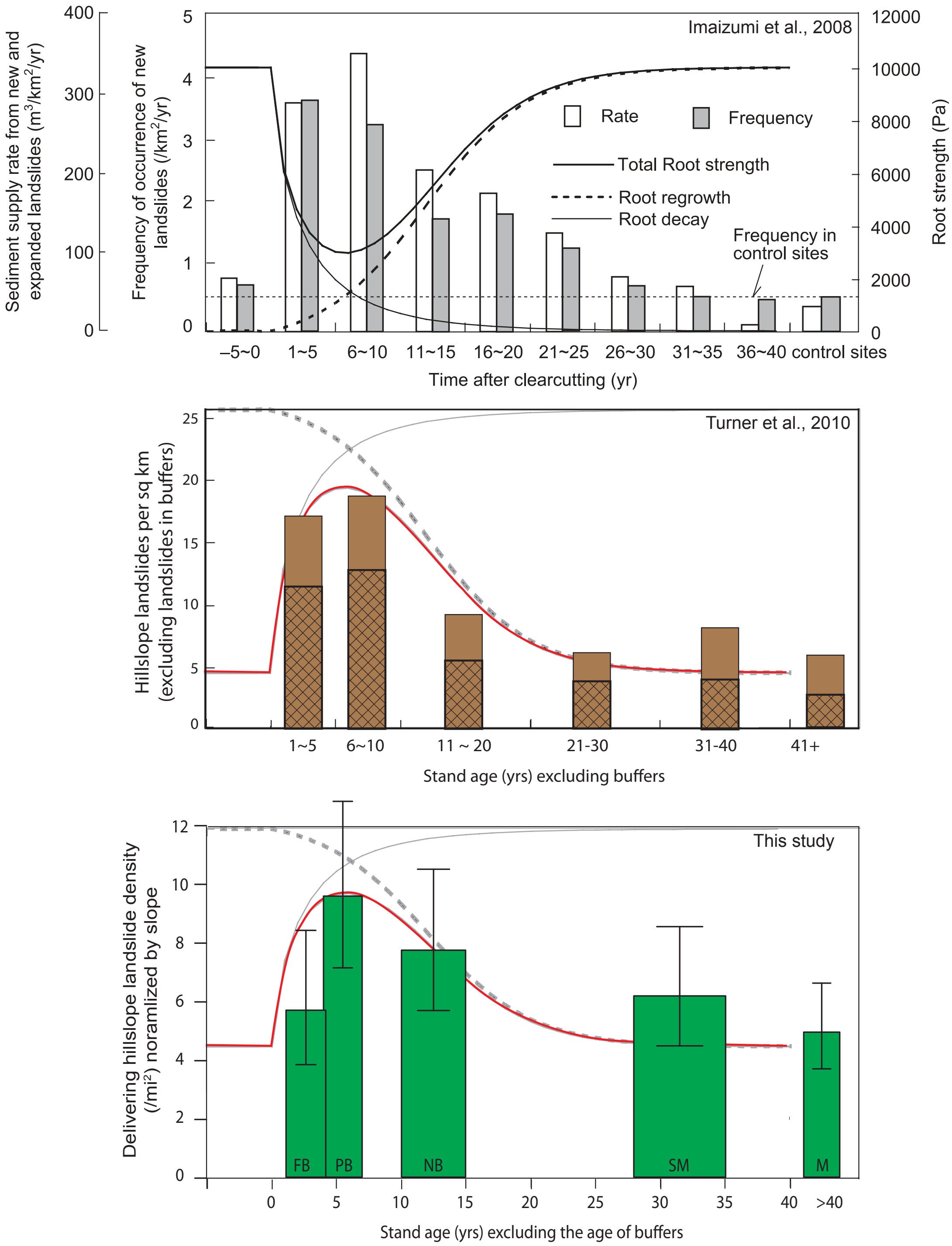

Figure 7-1: Expected changes in landslide density as a function of stand age. Top: Landslide frequency and sediment supply from a forested basin in Japan from as a function of root strength (Sidle 1991, 1992) from Imaizumi et al., 2008. Middle: Landslide density (roads and regulatory buffers excluded) for Weyerhaeuser land in western Washington with more than a few landslides (Turner et al., 2010). Bottom: Landslide density index from this study with 90% confidence intervals as presented in Figure 6-1; bar widths adjusted to the IQR of stand age (exclusive of buffers) from Figure 5-20. The middle and bottom figures are shown against a backdrop of root strength (red line - inverted to highlight susceptibility) taken from the top figure and visually fit to observed landslide densities.

7.2.5. An alternative approach to evaluating buffer effectiveness

In this study, buffer effectiveness was evaluated by comparing landslide densities for the critical harvest treatments with those for mature timber. This comparison is expected to address: 1) Whether the unstable slope rules are effective at reducing landslide rates, and 2) Whether the landslide rates in areas treated under the current Forest Practices Rules are comparable to rates observed in mature second growth forest (Dieu et al., 2008). Unfortunately, the analysis presented thus far only addresses the second question since it is entirely possible for treatments to be successful in reducing landslide densities compared to past management practices, yet still result in landslide densities that are significantly greater than those observed in mature forest.

An alternate analytical approach to answering the first question is to ask whether the observed treatment responses are different than what might have been expected in the absence of a buffer treatment. While the analysis conducted for this report, by itself, cannot answer this question with much confidence, comparing the results with those from a separate study conducted in response to the same 2007 storm event provides an opportunity for a qualitative assessment.

Turner et al., (2010) used air photos and field surveys to characterize the distribution of landslides across portions of the storm affected area, excluding landslides associated with roads or regulatory buffers from the analysis. If we assume that the results of Turner et al. (2010) represent the landslide response in the absence of buffers (since all buffers were excluded from the analysis), we can use those data to evaluate the effectiveness of buffers from treatments in this study. Using the landslide density of Mature stands as a baseline in each study, we see that the increased landslide density in stands that were harvested 10-40 years previously is very similar among the studies, but the two buffered harvest treatments in this study (i.e., Partial and Full Buffer) appear to exhibit smaller increases than similarly aged stands in the Turner et al. (2010) study (Table 7-3).

Table 7-3: Summary of differences between No Buffer and Full Buffer treatments relative to Mature, based on pairwise analyses of critical harvest treatments. Further statistical details are provided in Section 6.

Turner et al., 2010 (excludes all landslides occurring in buffers)			This study			
Stand age	Landslide density (/mi^2)*	Increase over Mature	Treatment	IQR of Age†	Landslide density (/mi^2)	Increase over Mature
0-5	6.6	193%	FB 0-20	1.1-4.2	5.7	15%
6-10	7.3	224%	PB 0-20	3.9-7.0	9.6	93%
11-20	3.6	59%	NB 0-20	9.7-15	7.7	56%
21-40§	2.8	24%	SM 20-40	27-35	6.2	25%
41+	2.2	0%	M 41+	46-57	5	0%

Note: Turner et al. (2010) densities visually estimated from the right side of Figure 13. Data for this study presented in the bottom of Figure 5-21 and Table 6-7.

While the differences may be influenced by other factors, it is consistent with simulation studies which indicate that partial cutting is likely to produce fewer landslides and reduced landslide volume compared to clearcutting (Dhakal and Sidle, 2003). Interpreted in this manner, our results suggest that the buffer treatments have reduced landslide densities in comparison to past management practices. Whether the current buffer requirements are adequate to meet FFR resource objectives remains unresolved. As shown in Section 6, partial buffering of RIL combined with a large storm occurring 4-7-years after harvest resulted in significantly greater landslide densities than were observed in neighboring mature stands. This raises the question as to whether stands with full buffering of RIL would be likely to meet performance targets if hit by a large magnitude storm, at a time when hydrologic and root strength effects are expected to create the most instability.

7.2.6. Potential influences of the 'worst first' approach to road landslide densities

In contrast to harvest treatments which are defined entirely on the basis of buffer presence, road treatment delineations were based on a handful of drainage and other indicators which required judgment in interpretation (Table 3.2). Determination of Abandoned and Orphaned roads were probably the most accurate because they have clear characteristics that contribute to consistent identification. A few Mitigated roads may not have been identified because side-cast pullback sites had re-vegetated, making that prescription difficult to recognize. In particular, we found that our criteria were not always adequate to clearly distinguish Standard from Substandard roads.

In addition to the difficulty in delineating road treatments, there are reasons to expect significant inherent differences in stability among road treatments due to regulatory and management incentives to focus more maintenance and repair resources on roads most likely to experience landslides. The primary regulatory driver is the Forest Practice Rules for Road Maintenance and Abandonment Plans (WAC 222-24-051) which specify that roads be improved in a 'worst-first' sequence. It is expected that this resulted in many unstable roads being improved in the six years between rule implementation and the 2007 storm. Landowners also have an incentive to improve roads in difficult terrain in order to minimize disruption of access and prevent larger repairs due to landslides.

Prior to 2001, Watershed Analysis prescriptions had required improvements focused on unstable roads since the mid-1990s. Watershed Analyses cover a sizable portion of the study area. The RMAP scheduling strategy and Watershed Analysis undoubtedly played a role in determining the location of highly-improved roads that populate the Mitigated road treatment. Additionally, many Abandoned roads appear to have been located on steep lower hillslopes, both because they were prone to instability and they were no longer needed for timber access. If these terrain differences are true, then the relative stability documented for Mitigated and Abandoned roads becomes even more notable, given their probable location in the most unstable terrain. Unlike the slope adjustments applied to harvest treatments (Section 6.1.1), we were not able to account for hillslope gradient differences among roads segments. The main reason was that topographic DEM reflect slope conditions as modified after the road was built, rather than before road construction, as would be needed to characterize inherent stability of each road segment.

A final factor that may have contributed to the lack of statistically significant differences among road treatments is that 70% of all road-related landslides occurred within four of the 22 blocks (Figure 5.24). This resulted in a zero landslide count for most treatments in the majority of blocks, greatly reducing statistical power. In the four blocks with most of the landslides, the majority of total road length (77%) was in the three treatments expected to be most stable – Standard, Abandoned and Mitigated (Figure 5.22). As a result, there were relatively few Orphaned and Substandard roads present by which to characterize the treatments expected to have the highest levels of instability. Additional analysis could characterize the roads in this study to better account for inherent instability among treatments and to better understand the factors that drive road-related landslides (Appendix B.4).

7.3 Other findings

While the critical questions provided the impetus for this study, many of the results do not fit well within their limited context or require nuanced evaluation not predicted by the study design. As with most studies, there were also results which were considered 'surprising'. These are results that have no direct relationship to the critical questions, but are still related to forest management.

7.3.1. Comparisons between hillslope and road-related landslides

As noted in response to Critical Question 2, roads accounted for one-fifth of delivering landslides and they contributed only a third as much sediment as hillslopes (Table 7-2). Because roads occupy only a small portion of the landscape, they have a three times greater landslide density and yielded four times as much sediment per area of affected ground when compared with hillslopes. But even in terms of landslide density, roads in this study contributed a smaller proportion than has been reported in the past (Table 7-4). A similar discrepancy was noted by Robison et al., (1999) in a similar field-based landslide inventory following the winter 1996 storms in Oregon, and was used along with evidence of smaller road-related landslides than previously reported to support an interpretation that management practices are reducing the size and number of road associated landslides. It raises the question: does this apparent shift reflect the effects of improved road management practices, or is it simply an artifact of different study methods or other factors?

It is important to recognize that most of the early landslide studies reflect initial performance of roads constructed in the 1950s prior to significant changes in the standards for road building. Our professional experience supports the conclusion of Robison et al. (1999) that current road management practices including minimization of road mileage on steep slopes and end-hauling of evacuated material (e.g., Session et al., 1987) and sidecast pullback are effective in reducing the size and number of road associated landslides. However, we caution against ascribing the lower landslide densities and yields observed here entirely to improved practices because it is likely that methodological differences contributed as well.

A key factor limiting the value of direct comparisons among studies is differences in landslide detection probability. As discussed in Chapter 2, numerous studies have demonstrated a detection bias in

Table 7-4: Relative landslide density for older or un-harvested forests in comparison to densities following harvest or from roads from a limited number of studies in the Pacific Northwest. See Robison et al. (1999) or Sidle and Ochiai (2006) for additional compilations of long-term rates.

Detection method	Study	Condition	Landslide count	Density (/mi^2/yr)	Relative density
Air photo & field observation	Swanson and Dyrness (1975) observations from the Andrews Forest, western Oregon, over a 24 year period (1950-1974).	Unlogged	32	0.16	1.0
		Clearcut	36	0.49	3.0
		Road	71	5.11	32
Air photo	Amaranthus et al. (1985) air photo inventory on the Siskiyou National Forest, southwestern Oregon over a 20 year period (1956-1976).	Unlogged	100	0.03	1.0
		Harvested	328	0.74	28
		Roads	216	2.32	87
Air photo	Johnson (1991) air photo inventory on South Fork Canyon Creek, southwestern Washington over a 42 year period (1948-1990).	Unlogged	22	0.04	1.0
		Clearcut	29	0.23	5.3
		Road	34	4.27	97
Air photo	Jakob (2000) air photo inventory of landslides less than ~ 20 y.o. and greater than 500 m^2 from 1996 photos of Clayoquot Sound, British Columbia.[*]	Natural	506	0.03	1
		Logging-related	498	0.25	8.8
Air photo	Guthrie (2002) air photo inventory on three watersheds, Vancouver Island B.C., based on 28-39 years of record (~1955-1996). [†]	Natural	121	0.05	1.0
		Harvested	92	0.06	1.3
		Road	150	1.29	27
Field survey	Robison et al. (1999) field inventory of delivering landslides in western Oregon, following winter 1996 storms.[§]	> 100 y.o.	111	13.6	1.0
		0-9 y.o.	61	18.7	1.4
		Roads	21	34.9	3
Field survey	This study (2012) field inventory of delivering landslides in Willapa Hills, southwest Washington, following a large storm event in Dec. 2007.[**]	> 40 y.o.	193	8.6	1
		< 20 y.o.	422	11.7	1.4
		Roads*	164	32.7	3.8

* Jakob combined the 268 clearcut and 202 road landslides for the purpose of estimating logging-related landslide rates. In addition, he used 40 years as the divisor for reporting temporal frequency in Natural. For comparison purposes, we use 20 years as the divisor for both Natural and Logging-related densities since his rational for using 40 years with Mature is unique to his report. Since roads are likely to occupy a small proportion of the watershed, they contribute a much higher landslide density than is indicated by the combined rate.

[†] Comparisons based on the combined density from Macktush Creek, Arlish Creek, and Nahwitti Creek watersheds.

[§] Comparisons based on data from Elk Creek, Vida, and Mapleton because comparable data were not available from other study areas, and roads are limited to active roads with a proportionally adjusted length in Elk Creek where active and inactive road were reported together.

** Analysis limited to core areas as described in Section 5.4. Data are presented in Table 5-9 with the exception of > 40 y.o. count and density which are reported as part of Harvest > 20 year old.. Road densities are limited to active road treatments.

landslide identification between field and air photo inventories. For example, Robison et al. (1999) reported that the ratio of landslides between clearcut and mature forest determined from 1:6,000 air photos was 21:1. An inventory of the same area by ground survey resulted in much higher landslide detection rate in older forest resulting in a ratio of 2:1 for the ground-based sample (Robison 1999). If our methods resulted in similar improvement in landslide detection in mature forests, this would result in reducing the relative proportion comprised by road landslides. Further, this could be a major cause of the smaller total fraction of road landslides in this and Robison's studies, relative to the densities reported in previous aerial photography based inventories.

Another factor affecting landslide triggers is the period of study. Single event based studies like this one have densities that are much greater than longer-term term studies because they are done in the wake of a large storm event when landslides are abundant and easier to locate than after scars revegetate. This should allow more accurate determination of landslide triggers. The disadvantage is that the landslide response is specific to the unique hydrologic stresses of that particular storm and the road and landscape conditions that were present. This could contribute to a pattern of instability (e.g., types of triggers) that would differ from what would be observed in a different storm or over a series of events.

7.3.2. Landslides initiating outside of RIL

As explained in Section 5.5, between 29% and 41% of the delivering hillslope landslides initiated outside of named RIL in Forest Practices Rules. This was a greater proportion than anticipated because it was generally assumed that most delivering landslides would initiate in a named RIL, in part because RIL are known to have efficient delivery mechanisms. These non-RIL landslides were distributed throughout the study area, and across a range of precipitation intensities, so they do not appear to be limited to an area of unique geology or extreme precipitation (Figure 5-9).

It's possible that some of these failure sites would have been regulated as unstable under WAC 222-16-050 which includes: "E) Any areas containing features indicating the presence of potential slope instability which cumulatively indicate the presence of unstable slopes." In this study, field crews were restricted to the named RIL and were not allowed to categorize landslides as having occurred within landforms that may have been ruled as unstable under section E because it would require too much subjectivity. Further, the authors have observed that the great majority of unstable terrain fits the description of one of the four named RIL (i.e., WAC 222-16-050, A through D). Nevertheless, field crews had some uncertainty in RIL determination, as discussed in Section 7.4.4.

Possible causes of the many landslides occurring outside RIL is a topic that has potential for further study as discussed in Appendix B.3. The Washington State Adaptive Management Program is currently involved in scoping a study that will attempt to identify additional landforms with a high probability of failure.

7.3.3. Recent clearcut harvest of RIL

Another unexpected observation from this study was the extent to which RIL had been clearcut harvested. Since harvest of RIL was not regulated prior to 2001 (aside from areas covered by Watershed Analyses) and Forest Practices Rules were enacted in 2001 that restrict harvest on such features (discussed in Section 7.1), the authors of the study design expected that the No Buffer treatment would consist mainly of sites logged prior to 2001. The Partial Buffer treatment was created as a category for harvests where RIL had been thinned or had corridors cut through for cable yarding. Since 2001, either of these activities or other types of harvest in RIL are permitted only where approved by a SEPA review or that meet the criteria of approved Watershed Analysis prescriptions. A focused aerial photography review of Partial Buffer units (Appendix A.4) seldom indicated that RIL harvest was limited to thinning or yarding corridors, but more commonly that a RIL had been clearcut. Of the portion of the study area logged since 2001, 50% was categorized as Partial Buffer indicating the broad extent of this pattern.

There are a number of possible reasons that so many RIL were harvested. One cause could be difficulty at accurately identifying RIL by foresters designing and reviewing harvest units. Although this study did not evaluate how any RIL determinations had been made, the field crews trained for this study encountered some difficulties in RIL identification. Results of field-based observer variability evaluations indicate that RIL identified as RIL by the observer variability team were not identified by field crews at 15% of the sites. RIL calls by the observer variability team were considered more reliable because it was led by a licensed engineering geologist with experience around the state in the employment of the WDNR Forest Practices Division. A possible component of some differences was that field crews, on average, measured slope gradients 5% lower than the observer variability team (Figure 5.1), and this could result in under-identifying RIL. The discrepancy in slope measurements was interesting, given that the field crew for this study were comprised of geologists and forest engineers with training and experience comparable to field layout and regulatory foresters. From this, we suspect that unidentified RIL overlooked during unit layout is likely to be a common cause of RIL harvest. A possible additional study of this RIL identification issue has been proposed (Appendix B.1).

Another possibility is that some unstable hillslopes in the study area were harvested because someone misjudged their potential to deliver to a stream. Regardless of their stability, slopes are not regulated as RIL under Forest Practices Rules if they are not judged to have potential to impact a stream or public safety (WAC 222-16-050 (1) (d)). A common example of an unregulated hillslope is an unstable feature located immediately above a broad flat area, where the landslide debris would deposit without reaching any stream or jeopardizing public safety. Although delivery potential is a key criterion for RIL determination, there is very little specific guidance for evaluating delivery potential within the Rules, Board Manual, or even the scientific literature. As a result, delivery calls rely on professional judgment and personal experience, making consistency difficult. It is possible that the magnitude of the storm event caused landslides to have longer runouts than previously observed, which allowed them to reach streams more commonly than would have occurred in response to a smaller storm event.

In addition to the identification-related difficulties discussed above, there are likely to be other factors at sites where the rules were not followed. Regardless, any conclusions on causes of RIL harvest are speculative without further evaluation.

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Glossary:

Adaptive Management Program (AMP): The AMP was created to provide science-based recommendations and technical information to assist the Forest Practices Board in determining if and when it is necessary or advisable to adjust rules and guidance for aquatic resources to achieve the resource goals and objectives of the Forests & Fish Report.

Akaike's information criterion (AIC): AIC scores are a measure of the goodness of fit of an estimated statistical model. The AIC methodology attempts to find the model that best explains the data with the fewest model parameters. Given a data set, several competing models may be ranked according to their AIC, with the one having the lowest AIC being the best.

Analysis of Variance (ANOVA): ANOVA is a method for partitioning observed variance in a continuous outcome variable into components associated with different explanatory factors. In its simplest form ANOVA provides a statistical test of whether or not the means of several groups are all equal.

Analysis of Covariance (ANCOVA): ANCOVA is a merger of ANOVA and regression. ANCOVA tests whether certain factors have an effect on the outcome variable after removing the variance for which quantitative predictors account. The inclusion of covariates can increase statistical power because it accounts for some of the variability.

Block: Blocks are used in statistical designs to account for variability from sources that are not of primary interest to the experimenter. In this study, blocking was employed in part, to account for the effect of precipitation on landslide occurrence.

Box-Cox transformation: The Box–Cox transformation, by statisticians George E. P. Box and David Cox, is a particular method of transforming data using power functions that preserve data ranks. This technique is used to help stabilize variance and make the data fit an approximately gaussian (normal) distribution.

Cluster: Four contiguous Public Land Survey (PLS) sections that were randomly chosen as part of the first stage of a multi-stage cluster sample design. Clusters sometimes were augmented with additional adjacent experimental units drawn from the frame. The cluster and experimental units drawn from the frame form blocks.

Critical treatment: Three of the harvest, and three of the road, treatments were considered critical to the sampling effort because they best represent past and present forest practices or because they serve as reference conditions for evaluating the effectiveness of the current Forest Practices Rules.

Digital Elevation Model (DEM): A digital representation of ground surface topography or terrain.

Experimental unit: The units on which observations are recorded. Road segments, harvest units and even-age forest stands serve as the experimental units in this study.

Exposure: A term used to describe the area of harvest treatments and length of road treatment. Within a given block, precipitation and geomorphology are assumed to be relatively constant, and landslide counts are expected to vary in proportion to exposure. In the statistical analysis, offsets are used to account for the differences in exposure resulting from the observational nature of the study.

Forests & Fish Report (FFR): A document issued in 1999 and adopted by the Washington State Legislature to be used by the Forest Practices Division to write new rule language. The Forests & Fish Report was the result of a collaborative effort by of diverse stakeholders, including tribes, forest landowners, local governments, environmental groups, and other interests. It outlined several ways to protect water quality and aquatic and riparian-dependant species on non-Federal forestlands in Washington.

Forest Practices Application (FPA): An application to perform timber harvest, road construction and maintenance, or aerial chemical application activities on state and private forest lands in Washington State. Once approved, the application serves as a permit.

Forest Practices Application Review System (FPARS): An online system that provides for the collection, distribution, and archiving of Forest Practices Applications.

Forest Practices Board (Board): Established by the 1974 Forest Practices Act, the Board is an independent state agency chaired by the Commissioner of Public Lands or his designee. The Board's job is to adopt rules that set standards for activities related to Forest Practices activities.

Forest stand: In commercial forest land, it is an even-age stand of timber that has regrown after previous harvest.

Frame: Composed of 12 sections that surround a cluster, the frame is used to augment cluster area in cases where critical treatments were underrepresented. Experimental units drawn from the frame are added to the original cluster sample to form a block for analysis.

Friedman test: A non-parametric statistical test applicable to complete block designs. The test uses ranked data and is a special case of the Durbin test.

Geographic Information System (GIS): Software used to create, analyze and display geographically referenced information including maps.

Global Positioning System (GPS): GPS is a space-based global navigation system that provides reliable time and location information anywhere there is an unobstructed line-of-sight to four or more GPS satellites. There are several different GPS networks; the system used to collect data for this study is maintained by the United States government and can be freely accessed by anyone with a GPS receiver.

Harvest treatment: Treatments are characterized by a set of prescriptions that were applied at the time of harvest. Treatments are identified by the era in which they were harvested, and in some cases, the degree to which RIL are buffered (see Section 2.2.1).

Harvest unit: A relatively contiguous parcel of land from which timber is harvested as part of a single operation.

Hillslope landslide: A landslide that was not associated with the prism of a road.

Light Detection and Ranging (LiDAR): An optical remote sensing technology that is used to measure distance (range) to a distant target. When mounted on a plane, and using appropriate geographic controls, LiDAR can be used to create high-resolution digital elevation models (DEM).

Public Resources: Defined as water, fish and wildlife , and in addition means capital improvements of the state or its political subdivisions (WAC 222-16-010).

Quality Assurance / Quality Control (QA/QC): A review process to evaluate and ensure that the data used in the analysis was reasonably free of errors that could affect the result.

Randomized Complete Block (RCB) design: A statistical sampling and analysis framework in which sampling takes place in experimental units (blocks) that are similar to one another. Typically, the blocking factor controls for sources of variability that are not of primary interest to the experimenter.

Road treatment: Treatments are characterized by a set of prescriptions associated with road maintenance. Treatments are identified by the condition of the road segment (See Section 2.2.2).

Rule-Identified Landform (RIL): RIL are the potentially unstable slopes recognized by Washington Forest Practices Rules and defined in WAC 222-16-050. Current regulations require RIL be identified prior to harvest or road construction. Forest Practices, to the extent practicable, are designed to avoid management activities on RIL unless the forester and/or regulatory agency staff have determined that there is little potential for sediment delivery to public resources.

Slope Stability Model (SlpStab): A grid data layer of modeled shallow-rapid slope stability for forested watersheds of western Washington State. This layer was an anticipated outcome of the Forestry Module negotiations as outlined in the Forests & Fish Report (1999) and legislated in the Engrossed Substitute House Bill 2091 (1999).

Sum of ranks: In the Friedman test, observations are sorted by the relative magnitude of the observation within each block and the ranks are used in the analysis. For example, given five treatments where NB=7.7, PB=9.6, FB=5.7, SM=6.2, and M=5.0 in a given block, the data would be sorted and the observations would be replaced with the relative ranking such that PB=5, NB=4, SM=3, FB=2, and M=1. Sum of ranks is simply the sum of the ranked values across all of the blocks and is used to test the null hypothesis. In its use of ranks it is similar to the Kruskal-Wallis one-way analysis of variance by ranks.

Treatment: In this retrospective study, experimental units are *ex post facto* assigned to 'treatments' based on the condition of the harvest unit, forest stand, or road segment. Some treatments have particular relevance in the evaluation of Forest Practices Rule effectiveness and are considered critical to the study.

Untyped water: Small, non-fish-bearing streams that do not connect to the rest of the channel network are classified as "untyped" and are not considered public resources under Forest Practices Rules.

Washington Department of Natural Resources (WDNR): Administered by the Commissioner of Public Lands, the WDNR is the primary governing agency for forest practices in Washington State. The WDNR oversees the Adaptive Management Program (AMP).

Appendices to

The Mass Wasting Effectiveness Monitoring Project: A Post-Mortem examination of the landslide response to the December 2007 storm in Southwestern Washington

APPENDIX A: MINORITY REPORTS

The next 18 pages contain the minority reports from A.J. Kroll, Douglas Martin, and Leslie Lingley.

Minority Report on the Evidence, Interpretations, and Conclusions of the Mass Wasting Effectiveness Monitoring Project (The Post-Mortem Report)

A.J. Kroll
Weyerhaeuser NR
CMER Reviewer
May 14, 2012

Executive Summary

The Mass Wasting Effectiveness Monitoring Project report (hereafter, the PM report) presents data, interpretations, and conclusions regarding the effectiveness of slope-stability prescriptions for mitigating management influences on landslide densities and sediment delivery in forested watersheds in southwestern, WA. In this minority report, I summarize my technical position on the PM report. In so doing, I have organized my comments around the 6 Critical Questions that the PM study was intended to address. Generally, I argue that an insufficient amount of information was collected to answer these critical questions. In addition, I contend that the PM report is burdened by two substantial and interrelated problems that the PM authors appear unwilling to address or to remedy: an inadequate study design and the incorrect interpretation of statistical results. I acknowledge that the study design issue has existed for several years. However, I also document the concerns expressed over study design issues by ISPR reviewers of the final report. In so doing, I emphasize the continuing relevance of this issue and how it is inextricably associated with interpretations and conclusions that can be made in the PM report. As I document in this minority report, the PM report remains encumbered by significant technical problems; these technical problems were identified in the ISPR review of the final report; and changes made to the PM report in response to these comments are not sufficient. Finally, these issues are technical and are not "process" issues. As such, these technical issues should be addressed, and remedies identified, within UPSAG and CMER.

Introduction

This report summarizes my technical position on the PM report. I have organized my comments around the 6 Critical Questions that the PM study was intended to address. For each of the six Critical Questions, I summarize the interpretations and conclusions presented in the PM report. I discuss whether sufficient evidence was presented to support these findings. Generally, I contend that the PM report is burdened by two substantial and interrelated problems that the PM authors appear unwilling to address or to remedy: an inadequate study design and the incorrect interpretation of statistical results. Finally, I discuss what I think are the interpretations and conclusions supported by the study design and the information that was collected by the PM study and presented in the PM report.

The PM report provides substantive information on only the first 3 of the Critical Questions. For the 3 Critical Questions that are addressed in detail, I note that the PM report includes contradictory conclusions. For example, the PM report claims that "The study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the density and volume of landslides" (p. V, v. 8a). However, the PM report also claims that "…complete buffering is effective at reducing sediment volumes, but has an indeterminate effect on landslide density" (p. 90, v. 8a). Also, the PM report claims that "This finding (results presented on p. 98, v. 8a) lends further credence to the hypothesis that RIL buffers reduce landslide densities" (p. V, v. 8a). That the specific effectiveness of RIL buffers in reducing landslide densities could not be evaluated by the PM study was made clear later in the report: "…because RIL buffers could not be distinguished from riparian or other buffers during treatment delineation, the study tested the combined effectiveness of all types of buffers at reducing landslide initiation and sediment delivery" (pp. 89-90, v. 8a).

The inadequate study design implemented by the PM study is a long-standing, contentious issue that has been raised by numerous reviewers during the development of 8 versions of the report. The position of the PM authors and CMER co-chairs that these issues can be willed away based on the CMER process is untenable. In the ISPR review of the PM report, Reviewer 1 responded to Question 4 (Do the stated conclusions logically flow from the results?) as follows:

"As stated in my answer to Question 1 (Are rigorous, transparent and sound research and statistical methods followed?) above, a substantial part of the conclusions cannot be supported by the results. This is because the results of the statistical tests of significance have been ignored so that the conclusions are consistent with the co-authors expectations. ***In the process weaknesses or inadequacies of the experimental design that may have caused the lack of statistical significance between various treatments have been overlooked****."* (Bold text added; Line 35 of ISPR review response matrix)

In response to Question 3 (Were data reasonably interpreted?), Reviewer 1 responded:

"I think the final decision whether there is a difference or not between the landslide response of the various treatments should be based solely on whether the difference is statistically significant. If the difference is statistically significant there is a difference, otherwise there is none. ***This kind of interpretation should be used consistently in the entire report.*** *Otherwise, one could ask the question as to why bother designing a rigorous statistical experiment with all the efforts of blocking, clustering, randomization, introducing auxiliary variables as covariates, etc. The possibility that the lack of statistically significant difference is an artifact of the experimental design of focusing on a single large storm should also be clearly articulated."* (Bold text added; Line 32 of ISPR review response matrix)

PM authors have maintained, over the last 2 years, that study design issues were resolved when the response matrix for the ISPR review of the *study design* was approved. However, these comments by Reviewer 1, made during the ISPR review of the *final report*, indicate that study design issues remain relevant, because they influence any interpretations and conclusions made from data collected and analyzed during the PM study. As I make clear in this minority report, the PM report remains encumbered by significant technical problems, including an inadequate study design; these technical problems were identified again in the ISPR review of the final report, and changes made to the PM report in response to these comments are not sufficient; and these technical issues should be addressed, and remedies identified, within UPSAG and CMER.

EVALUATION OF THE 6 CRITICAL QUESTIONS

Critical Question #1: *Are the Forest Practices Rules effective in reducing the number of management-related landslides that deliver to public resources?*

The PM report claims that "The study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the **density** and volume of landslides" (p. V, v. 8a). This hypothesis seems reasonable if one hopes to mitigate management influences on slope stability in forested watersheds. However, I argue that several other factors are likely to influence slope stability as well. As a result, I contend that this conclusion cannot be supported by the information presented in the PM report.

The PM report found significant differences in landslide density between the No Buffer (NB) treatment and the Mature (M) treatment (Figure 6-1; p. 76, v. 8a). Landslide density did not differ significantly between either the Full Buffer (FB) and Mature treatments or the Full Buffer and No Buffer treatments. As a result, the PM report concluded that complete buffering has "an indeterminate effect on landslide density" (p. 90, v. 8a).

However, the validity of either of these conclusions is difficult to assess due to the nature of the PM study design. First, FFR harvest-related unstable slope prescriptions are less than 15 years of age, and are thus younger in age than other prescriptions. As a result, harvest unit age is confounded with the 5 treatment prescriptions, because many of the harvest units in the Submature (SM) and Mature treatment categories were harvested before FFR prescriptions were developed. Also, the average harvest unit age in the No Buffer treatment is greater than either the Partial Buffer or Full Buffer treatments (Table 5-21; p. 64, v. 8a). The steep hydrologic recovery and limited root strength recovery > 10 years after harvest cited by PM authors (p. 9, v. 8a) suggest that differences across the 3 young treatments may have been a factor in study results.

The inherent problem unequal application of FFR prescriptions across harvest unit ages was noted in the ISPR review of the *study design*. For example, David Tarboton (Utah State University) stated:

> "*I do not think that this (FFR prescriptions in young stands only; a range of prescriptions in older stands) is controlled for in the proposed study and this is a shortcoming that needs to be acknowledged and evaluated. It may however, not be possible to fully address this."*

Similarly, George Ice (NCASI) stated:

> *"How will the unavoidable age-dependency of treatments be addressed? Are there pre-FFR practices for harvest units that will be recent? Will there be post-FFR practices that will be the same age as the pre-treatment practices. Response within a window of 20 years is likely to not have the same risk of failure at the beginning or end of that time period."*

To allay these concerns, PM report authors stated 2 potential solutions. The first solution was to identify "older pre-FFR practices where buffers were left for watershed analysis prescriptions (~1992-1999)." The second solution was "to collect exact stand ages while in the field, particularly for these <20 year age classes. Exact stand ages might help us place individual harvest blocks on the root strength curve to better understand different responses, but these results will not have much statistical rigor. In a perfect world (and we're rapidly missing the right window), we'd have pre-FFR harvest areas on the upswing of the root strength curve that correspond to post-FFR harvest areas on the downswing."

The first of these solutions was clearly infeasible as random application of pre-FFR practices is unlikely to have occurred on the landscape, rendering statistical comparisons invalid. The second solution was unlikely to promote much understanding, because any results would have rested on hypothetical arguments about root strength, which was not measured in the PM study. In addition, as PM authors noted, any results from this type of analysis would also have lacked the statistical rigor that a well-designed study is constructed to achieve.

That these proposed solutions were insufficient was noted by Reviewer 1 during the ISPR review of the *final report*:

"Indeed, the outcome of the statistical analysis in this study contains several anomalies (i.e., unexpected results). The co-authors made attempt to find physical explanations for these anomalies but did not consider the possibility that such anomalous results could just be a normal outcome of the random sampling experiment, especially when the evaluation is based on a single large storm. For instance, I quote from the ISPR Report page 84: 'We did not expect PB to have more landslide than NB, because PB should benefit from having buffers on some RIL. ***Although both treatments were harvested within 20 years prior to the study, the NB sites were typically harvested earlier in this age window, which may explain part of this difference. Another possible explanation is that the PB treatment contained more unstable terrain as discussed in Section 7.3.2 below, though the inclusion of slope as covariate is expected to account for the effect****.'"* (Bold text added; Line 12 of ISPR review response matrix)

"Although the co-authors have decided to conclude that buffering reduces landslide based on some statistically insignificant differences, someone else reading this report could make the opposite conclusion that buffering is not effective in reducing landslide, on the basis of statistically insignificant differences. Therefore, a substantial part of the conclusions from this report cannot be supported by the results of this study." (Line 10 of ISPR review response matrix)

Second, while the PM study evaluated the association between 5 different buffer treatments and landslide density, the conditions that existed on the study landscape prior to the 2007 storm represented a mixture of watershed analysis prescriptions and Forest and Fish Rule prescriptions for riparian zones. As a result, it is difficult to determine how many landslides were management-related and the effectiveness of site-specific practices. For example, the size and placement of riparian buffers are not entirely based on the presence of RILs. Also, RILs were not mapped across the study landscape. If true population size is unknown, it is impossible to determine whether buffered RILs were less likely to fail than unbuffered RILs, given similarity in other conditions such as precipitation and stand age. Due to these issues, the PM report states that "the comparison of buffer effectiveness among treatments is really a test of the effectiveness of all buffer types as they limit either landslide initiation of landslide delivery; the effectiveness of unstable slope buffers to limit landslide initiation cannot be separately quantified by this study design" (p. 10, v. 8a). The PM report cannot distinguish management landslides from non-management related landslides, and it cannot assess the effectiveness of unstable slope (RIL) buffers specifically.

***Critical Question #2:** Are the Forest Practices Rules effective in reducing the volume of sediment that delivers to public resources as a result of management-induced landslides?*

The PM report claims that "The study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the density and **volume** of landslides" (p. V, v. 8a). This hypothesis seems reasonable if one hopes to mitigate management influences on slope stability in forested watersheds. However, I argue that several other factors are likely to influence slope stability as well. I contend that this conclusion cannot be supported by the information presented in the PM report.

The PM report found significant differences in initial sediment yield between the No Buffer treatment and both the Full Buffer and Mature treatments (Table 6-11; p. 81, v. 8a). However, significant differences in delivered sediment yield were only found between the No Buffer and Submature treatments (Table 6-14; p. 83, v. 8a).

Several factors make the validity of these interpretations questionable. First, measurements of delivered sediment yield were characterized by a significant amount of bias and variation within and across observers (Figure A-2, p. A-5, Appendices to The Mass Wasting Effectiveness Monitoring Project: A Post-Mortem examination of the landslide response to the December 2007 storm in Southwestern Washington). If the sampled data were inaccurate, results from statistical tests may be spurious. At the very least, confidence interval coverage for statistical tests will be overly optimistic. Either way, resulting inferences will mischaracterize any associations between the 5 treatments and sediment delivery. I note that the use of these data to conduct statistical analyses and to make conclusions about the effectiveness of FFR practices is one of the most troubling aspects of the PM report.

Second, the different treatments may have unequal inherent risk of delivering sediment. For example, the Full Buffer treatment has lower slope gradients on average than the other 4 treatments (Table 5-16; p. 65, v. 8a). Also, the Partial Buffer treatment is the only treatment that must have, by definition, a rule-identified landform (RIL). Rule-identified landforms have been identified as landscape features particularly prone to landslide events (pp. 4-5, v. 8a). While the PM report attempted to control for differences in slope by including the effect of median slope gradient within treatment tests (p. 74, v. 8a), it remains unclear whether this covariate remedied the problem. The PM authors state that the "degree to which the slope index fully captured differences in susceptibility among treatments remains unknown" (p. 96, v. 8a). Similarly, the PM report did not map RILs within the study area and assumed that RIL distribution was equivalent across the 5 treatment types (p. 10, v. 8a).

Third, and perhaps most critically, the PM report was unable to identify site-specific triggers for ~85% of the landslides sampled in the PM study (see Critical Question #5 below). As a result, any interpretations about management-related influences are conjectural because no evidence is available to either support or refute such interpretations.

***Critical Question #3:** Which are responsible for the greater proportion of landslides and sediment volume, hillslopes or roads?*

The PM report found that hillslope landslides delivered the largest amount (78%) of sediment in total (Table 7-2; p. 91, v. 8a). Also, active and inactive roads delivered the largest amount (28% and 56%, respectively) of sediment by unit area (Table 7-2; p. 91, v. 8a).

Hillslopes occupy the largest proportion of the study area, so the result that they contributed the most sediment in total is intuitive. While road-related landslides contributed ~4 times as much sediment per unit area as hillslope landslides (p. 101, v. 8a), the PM report notes that road-related landslides were a smaller proportion of the total number of landslides than found in previous studies.

However, in addition to the problems associated with the measurement of sediment delivery volume, making specific statements about effectiveness of road maintenance practices across studies is hampered by two issues. First, road maintenance practices have changed through time and may differ by study area and within study area by ownership. Also, study regions may experience storms of varying magnitude and frequency (i.e., the strength of treatment effects may differ by region), in which cases roads are "tested" under a range of conditions that render comparisons difficult. The latter problem was noted in the ISPR review of the study design by David Tarboton (Utah State University):

"I think that the analysis needs to find a way to factor in the prior weather event history relative to the timing of forest activities whose effectiveness is being evaluated. This concern applies to both roads and harvest units prescriptions, but is more of a concern with roads where quite old substandard or orphaned roads may be compared to much more recent standard or mitigated roads. Roads that have been there a long time have been "tested" already so are less likely to exhibit new landsliding, relative to if they were built to that standard today."

***Critical Question #4:** Which harvest unit prescriptions or road improvements are performing well? Which are performing poorly?*

The study design was not adequate to evaluate this question. The PM report indicates that the "decision to focus the sampling design on 'treatments' rather than individual prescriptions makes it difficult to identify which individual prescriptions are performing well and which are performing poorly" (p. 92, v. 8a).

The PM report states that retention of buffers on RIL reduced the "volume of sediment delivered to public resources…"(p. 92, v. 8a) and concludes that "effectiveness of RIL buffering is greatest when all RIL are buffered…(p. 92, v. 8a). However, the PM study did not map individual RILs in the PM study area ("during the development of the study design, it was decided that the mapping individual RIL over the entire study area was infeasible;" p. 10, v. 8a). In addition, it is unknown whether treatments (i.e., retention of buffers on individual RILs) were applied randomly or as a result of RIL type or site-specific factors. As a result, the effectiveness of RILs cannot be determined conclusively based on evidence presented in the PM report.

Critical Question #5: *What are the site-scale triggering mechanisms for landslides?*

Field crews identified site-scale triggers at less than 15% of the landslide initiation points identified in the study (pp. 92-93, v. 8a).

Field crews identified "no obvious drainage contributing factor at 68% of the hillslope road failures" (p. 93, v. 8a). Report concludes that road failures were "caused by factors inherent to the treatments" (pp. 93, v. 8a). In addition, the report noted "a strong concentration of road landslides in blocks with highest precipitation (sic; p. 93, v. 8a).

In general, the study design was not adequate to quantify and identify trigger mechanisms at landslide initiation points. For example, trigger mechanisms were identified categorically, and no numeric measurements were sampled at landslide initiation points.

Critical Question #6: Do those triggering mechanisms differ between harvest or road type?

This critical question could not be addressed by the PM study. The report states "The triggering mechanisms (i.e., contributing factors) were not identified frequently enough (~6% of all landslides had contributing factors) during field data collection to allow for meaningful comparison between harvest or road types" (p. 93, v. 8a).

Appendix 1: Existing technical issues in the PM report. These issues were raised repeatedly by Ted Turner (Weyerhaeuser NR) in UPSAG but were not resolved prior to UPSAG advancing the report for CMER approval in April 2012.

Page	Section	Comment
II	Study Design and Methods: ***Harvest treatment definitions***	Treatment definitions are not consistent. For example, the No Buffer definition states, in more than one section of the report (e.g., here in the ES and on pg. 22), that this treatment included buffers. This is important, because it becomes unclear to the reader how it differs from Partial Buffer. Also, it needs to be clear that the PB treatment was the only one requiring RIL presence and that this is a potentially confounding design flaw. **Solution:** Clarify the definitions and include influence of strata implementation variability on interpretation of study results.
III	Results and Discussion: ***Landslide data***	Landslide associations, and relevant management implications, are for hillslopes and roads only. It needs to be stated that: (1) debris flows were a frequent type of landslide, (2) landslides were most frequently associated with inner gorge landforms, and (3) the inclusion of RMZs buffers in the study confounds interpretations of potential management influences. This is because an unknown, but potentially high number of identified landslides were triggered by fluvial and/or debris flow processes, unrelated to road construction practices of removal of timber from steep slopes. **Solution**: Include statement acknowledging confounding influence of stream-proximal landslides on rule effectiveness and management implications.
III	Results and Discussion: ***Slope gradient***	No mention that slope-adjusted values do not directly address bias from unequal RIL distribution (based on treatment definitions and implementation challenges) or that gradients are not normalized. Median slope is not a proxy for RIL area and AIC scores for slope tests were similar. Limitations and uncertainties must be clear to the reader. No mention that buffer treatments have lower slope gradients (pg. 65). From page 65: *"The Full Buffer treatment*

		is associated with lower slope gradients than any of the other four treatments. It is therefore possible that differences in gradient help explain differences in the landslide response among treatments." **Solution:** Include clear statement that treatments are potentially confounded by unequal distribution of landslide prone terrain.
III	Results and Discussion: ***Site-level contributing factors***	This paragraph sounds like data were analyzed (e.g., like a standard factor of safety analysis), or direct evidence was available, to quantify contributing factors. Should say that "…field crews gathered <insert **qualitative**> information at each landslide…". Presence/absence associations are circumstantial. The primary result here is that no significant management triggers were associated with the landslides and that management influences could not be quantified. **Solution:** Clarify that the data for potential management triggers identified for (field-detected) associations with landslide sites were qualitative and data were inconclusive.
	Results and Discussion: ***RIL / non-RIL associations***	The last paragraph on page III says a "sizable fraction" with regard to landslides outside of RIL. Just report the data with the relevant limitations. The last sentence regarding non-RIL landform sensitivities to rainfall was not quantified. Non-RIL counts are not classified by landform type and are not normalized by area or rainfall intensity. How many landforms are in the non-RIL suite of landforms and how much area is represented by each of them? How many non-RIL landslides were RIL-associated landslides (field crews were not asked to interpret rule category "E" landforms)? RIL and non-RIL densities both increased with increasing rainfall, as expected. This subjective statement is not based on any available data and does not belong in the ES. **Solution:** Report specific data and delete subjective interpretations.
IV	Management implications: ***Influence of buffering***	The first sentence of the first paragraph starts with a general statement not based on statistical significance and ends with a general statement of statistical significance that sounds like a disclaimer. Just state the specific, statistically significant results. There are least 3 lines of evidence that contradict the interpretation of results in this section. (1) Full Buffer is not significantly different than Mature; however, there is also no difference in landslide density between Full Buffer and No

		Buffer. These data indicate that buffering all regulatory features versus not buffering them will result in equal landslide densities on average. (2) Differences in density may also be explained by differences in terrain susceptibility to landsliding (e.g., see page 65 and elsewhere in the draft). (3) All regulatory buffer types were included in the study, including RMZs. Landslide associations with buffers specific to mass wasting prescriptions are unknown. Therefore, how can one conclude that RIL buffers are effective? The anecdotal comparison to other studies is an interesting armchair discussion, but it is confounded by unequal data and methods of analysis and is not a primary finding based on the PM study. It's okay, and correct, to say that unstable slope buffer effectiveness could not be determined in this study. **Solution:** Correct the misinterpretations and make it clear that unstable slope buffer effectiveness could not be determined.
IV	Management implications: ***Road management***	Current standards may be effective at reducing road slides, but you can't get there directly from the results of this study. Harvest versus road slide proportions were skewed toward hillslope failures, but as correctly stated elsewhere in the report, this is very likely due to different inventory methods (e.g., ground-based methods of landslide detection versus air photos), and the fact that very large storms have always resulted in higher in-unit to road slide ratios. Road failures were uncommon outside the portion of the study area receiving the most rainfall. This has been true for all major storms under different rule packages, so why not infer that insufficient hydrologic stress confounds the interpretation of improved road practices? **Solution:** Make it clear that the relative road/in-unit proportions can't be effectively compared to most previous studies. It is difficult to conclude, with high confidence, that current road standards are more effective in this study area compared to past practices.
22	3.4 Harvest Treatment Delineation: ***NB and FB***	Treatment definitions here need to be checked to see if they jive with other sections of the report. Because FB and NB harvest units with no apparent RIL were split by date of planting relative to 2001 (weighted average stand age with buffers or harvested area stand age is not clear here), unequal exposure to landslide producing storms among the two treatments exists. The NB treatment includes harvested units exposed to extreme storms in 1996, for example. This

		confounds interpretations of landscape response and management implications using the single, 2007 storm event. **Solution:** Clarify the definitions and include influence of strata age variability between FB and NB treatments on interpretation of study results.
23	3.4 Harvest Treatment Delineation: ***Partial Harvest Definition***	This section states that Partial Buffer units, by definition, must contain one or more RIL and that "we have observed significant areas within many harvest units or forest stands that do not contain RIL" among the other 4 treatments. Regardless of median slope gradient corrections, which are not proxies for RIL distribution, Partial Buffer densities, in the absence of forest management, could be expected to be higher than the other treatments. Management implications of this unequal inherent risk among treatments are referred to section 7.3.2; however, this section refers to non-RIL landslides. Perhaps this should reference 7.3.3. **Solution**: Include terrain variability as a possible explanation for higher landslide densities in the PB treatment where appropriate in the ES and Discussion.
49	5.5 Landslides Outside of RIL: ***Hypotheses to explain proportion of landslides outside RIL***	The first paragraph states that percentage of landslide in RIL "illuminates how completely the existing RIL criteria describe terrain that fails and delivers…". Actually, landslide fractions within and outside RIL do not address relative susceptibility, which is what we are interested in knowing. Landslide densities by landform type do. However, relative susceptibility is unknown for this study area because landform types outside of RIL are not defined and densities cannot be determined. This section spends considerable time discussing working hypotheses to explain the percentage of landslides outside of RIL, but fails to discuss that percentages are confounded by area outside of RIL. If RIL account for more than 50% of the observed landslides and RIL area is represented by only 5-10% of the study area (or less), then non-RIL landform area is very large and landslide densities are therefore very small. Also not discussed is the fact that the named RIL were never meant to include all landform types (or sites where landslides occur independent of slope morphology). Landslides outside RIL are expected and landslide densities within RIL should be relatively high which appears to be the case even without estimates of density by landform type. There is no argument that this is of interest and potentially worthy of further study; however, until the data are normalized by area among the suite of landform

		types outside RIL, the percentages are not useful for inference. Ironically, note that the next section of the report, 5.6, correctly acknowledges that landslide counts by stand age are meaningless without knowing the area in each age class. This is the same point that needs to be made here. **Solution:** Note that percentage of landslides outside RIL is meaningless without density data
56	5.7 Landslides in Buffers: **Comparison of slide size**	The first barplot in Figure 5-13 lumps all shallow landslides in the buffered, not buffered comparison. Not sure why this was ever done in the first place. Landslide size is primarily controlled by landslide type, landform type, and landform scale. The only way to test harvest effects on landslide size is to carefully control for these factors, one landform at a time. Many of the larger landslides occurred outside of RIL where buffer were not required, which is likely why the data were skewed in the first barplot. Restricting the data to` landslides in RIL (mostly inner gorge and steep, convergent landforms in this study) partially gets there, which is why the results are more similar. **Solution**: Not a fatal flaw, just a lot of explanations without addressing the most obvious controlling factors.
65	5.8.1 Harvest Treatments: Slope gradients	Difference in gradient as shown in Table 5.6 (and perhaps more importantly, variability in RIL density per strata) may explain the differences in landslide density in whole or part, at least among the young treatments. This needs to be clear in the discussion and ES. Median slope can be the same between strata with different inherent risk based on RIL. **Solution**: This problem is discussed above in the ES comments.
66	5.8.2 Landslides and Landforms in the PB Treatment: **Scaling effects**	You need to discuss the potential scaling effects of small treatment areas for landslide counts and sediment volumes normalized to area. For the post 2001 harvest units, PB was 50% of the area, FB was 45%, and NB only 5%. Is it possible that one or two anomalously large landslides in NB can skew the sediment volumes while at the same time densities are not significantly different? You can reference Miller and Burnett (2007) for this. **Solution**: Address potential scaling bias of densities and volumes due to small areas using available data from the literature

Minority Report Concerning the Technical Acceptability of Post Mortem Version 8a
Douglas Martin, May 15, 2012

Overall Conclusion

The Post Mortem (PM) Version 8a is technically not acceptable for CMER approval because of inadequate responses by the co-authors to "substantive concerns" that were raised by the ISPR and the ISPR Associate Editor (AE). Below I summarize my evaluation of the PM response to two substantive concerns that were identified in the Conclusion section of the AE report.

Substantive Concern 1 – the conclusions do not follow logically from the results

The AE addressed the CMER technical review question "Do the stated conclusions logically flow from the results" with the following conclusion:

> "However, a number of substantive concerns are raised on the matter of whether the research method and associated statistical analyses are followed appropriately and, therefore, whether the data were reasonably interpreted. As a consequence, there is concern that some of the conclusions do not follow logically from the results."

Version 8a addressed some AE concerns; however the revision still includes questionable interpretations and conclusions that are not supported by the study results. The study results for the harvest-related landslides are reported in Sections 6.1 and 6.2 where the statistical findings are: the Full Buffer treatment had a landslide density that was intermediate to Mature and No Buffer, but not statistically different from either. In contrast, the Full Buffer and Mature had delivered sediment volumes that were not statistically different, but were statistically smaller than the No Buffer treatment. These results clearly show a differential response between landslide density and volume to the harvest treatments. Accordingly, in the Discussion of these findings in Section 7.1, the co-authors state a reasonable conclusion (p.90) that "This indicates that complete buffering is effective at reducing sediment volumes, but has an indeterminate effect on landslide density." However, these findings and associated facts are variously interpreted and are not consistent with several conclusions that are reported in the Executive Summary (ES) and in other subsections of the Discussion.

First, in the ES (p.V, par. 2) it is stated that "The study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the density and volume of landslides" This statement implies that both density and volume responded similarly among treatments even though the facts indicated otherwise. Following their conclusion, the co-authors present a subjective and confusing explanation as the basis for their interpretations, and completely ignore the statistical-based findings that were reported in Section 6.0 and in the Results section of the ES.

Second, in the ES and in Discussion Section 7.2.5, the co-authors present another interpretation for hillslope landslides that is based on a qualitative comparison of landslide density response by age group between this study and Turner et al. (2010). They reported that "This finding lends further credence to the hypothesis that RIL buffers reduce landslide density" (p.V, par. 2), and on p. 100 that "Interpreted in this manner, our results suggest that the buffer treatments have reduced landslide densities in comparison to past management practices." Such conclusions are speculative and misleading because they are based on a non-statistical observation of landslide response patterns between two studies with different sample units (i.e., landslides versus treatment polygons) and different data collection methods.

Furthermore, it is illogical to substitute the quantitative findings of this study (i.e., FB had no significant effect on landslide density) with a qualitative observation?

Third, the ES presents contrasting interpretations that confuse the reader about what is fact and what is speculation in this report. For example, in the third paragraph of the ES, the co-authors discuss how the findings indicate that the landslide performance targets may not be met. Here, they use the study statistical findings to support the first of three reasons, where it states "The first is the lack of statistically significant differences in landslide density for the Full Buffer treatment. Based on this finding, it is not clear that the magnitude of the reduction in landslide densities associated with buffering of currently regulated RIL is sufficient for meeting performance targets." In this case the lack of a statistical significant differences in landslide density is used as the basis for the co-authors to appropriately question the Full Buffer effectiveness, yet this fact is ignored in the topic sentence of the same paragraph (p.V, par. 3) where they state "Although we conclude that buffers likely reduce landslide density and sediment volume, it is not clear that existing performance targets for hillslope landslides are being met." Note, the discrepancy between how the facts are used, and not used, to formulate the different conclusions.

The preceding examples demonstrate some of the inconsistencies with interpretation of data and the formulation of conclusions in Version 8a. Both the ES and the Discussion section contain a number of technical inconsistencies. The ISPR identified similar interpretation inconsistencies in Versions 7a and the AE highlighted this concern by quoting Reviewer #1 with the statement "a substantial part of the conclusions cannot be supported by the results... because the results of the statistical tests of significance have been ignored".

Substantive Concern 2 - uncertainties and limitations of study design are not fully addressed

The AE identified several concerns by the ISPR about the study design shortcomings and recommended that the uncertainties and limitations be more fully addressed. These concerns were captured in the AE statement:

> "These collective shortcomings are attributed in part to the study design, and in part to the belief that the spatial temporal impact of the very large the storm of December 2-3, 2007 may have obscured subtle differences between forest prescriptions at the harvest-unit scale and between different road types at the road-segment scale…."

Recognition of the study limitations is important for understanding and interpreting the study results. About this concern, the AE referenced Reviewer #1 and #2 concerns about the reasonableness of the PM conclusions given the study limitations. In particular, Reviewer #1 stated:

> "the process weaknesses or inadequacies of the experimental design that may have caused the lack of statistical significance between various treatments have been overlooked,"

and Reviewer # 2 stated:

> "It may be implied the reasonableness of the conclusions is founded on subjective expectation."

The Discussion in Version 8a addresses some uncertainties and limitations of the study, but some key concerns remain overlooked. Also, there is a prevailing tendency in the discussion to minimize the potential effects of confounding factors on the study findings.

A key concern is the absence of discussion about the reliability of measuring landslide density and volume and how variability in these metrics may have influenced the statistical analyses and study conclusions. The AE noted this concern with the comment:

> "Reviewer #2 raises some concern for greater recognition of uncertainties arising from the skewed nature of the landslide dataset"

This concern is particularly relevant to the estimate of landslide volume which was found to have considerable measurement error (QA/QC Report). Overlooking the volume issue raises concern about the validity of the statistical findings (i.e., FB and M had delivered sediment volumes that were not statistically different, but were statistically smaller than the No Buffer treatment) and PM conclusions about the effectiveness of the FB to reduce landslide sediment volume.

A major element of the study design is the assumption that spatial variability of external factors affecting landslide occurrence was controlled by the blocking approach. The PM Section 7.2.2 recognizes a number of physical factors (i.e., hydrology, root strength, soil conditions, and topography) could affect landslide susceptibility, but discounts these concerns based on the assumptions that "conditions within blocks are homogeneous relative to the variability seen across blocks" and that "the treatments are quasi-randomly distributed within blocks." No facts, only generalizations, are given about the distribution of key physical factors to support or validate the homogeny assumption, and no information is given to support the claim about treatment distribution. Existing information about certain physical factors could have been evaluated and the treatment mapping (Appendix D) could have informed these assumptions. Unfortunately, the co-authors dismiss the spatial variability concerns with the statement "we believe that the blocking approach was reasonably effective at controlling large scale spatial variation in factors like precipitation, species composition, soils, and geology which may have affected the landslide response." Given such limited information, it is not clear how the "we believe" statement can be supported.

WASHINGTON STATE DEPARTMENT OF
Natural Resources
Peter Goldmark - Commissioner of Public Lands

Caring for
your natural resources
... now and forever

MEMORANDUM

To: Chris Mendoza and Mark Hicks - CMER Co-chairs

From: Leslie Lingley – Forest Practices Science Team Lead

Date: May 15, 2012

Subject: Policy dispute resolution: *Mass Wasting Effectiveness Monitoring Project: An examination of the landslide response to the December 2007 storm in Southwestern Washington*

There are numerous unresolved issues associated with this report. The following are representative examples I believe relate to the ISPR review process. These examples are supported by the Forest Practices Science Team, who members have 90 years combined experience with landslides in the forest. For this reason, these comments should not be binned or otherwise edited without our written permission.

1) First and foremost, despite many requests, none of the non-author UPSAG members or ISPR reviewers has ever seen the actual data.

2) Two of three key response variables, "*Total Sediment Yield*" and "*Initiation Volume*", cannot be quantified without a high precision map of "before and after" Lidar or of engineered topographic data. These key response variables were used to generate the statistical comparison among treatments, the author's primary indicators of Forest Practices Rule effectiveness.

   The reason Total Sediment Yield and Initiation Volume cannot be determined casually is marked uncertainty in: 1) The depth of scour, 2) The depth of fill, 3) The location of the initiation point [e.g., did it start by under-cutting at the bottom by a debris flow or stream and subsequently ravel up slope, or did it initiate in the middle at a spring or steep layer?], 4) Whether one or multiple landslides have occurred at the site, and most importantly, 5) Each landslide's area, unless depicted on a detailed map.

Note that many convergent Rule Identified Landforms (RILs) including those assessed in this study formed by fluvial down-cutting and multiple landslides within the same feature.

As part of the Mass Wasting Effectiveness Monitoring Project, observer variability in estimating Total Sediment Yield was tested using the actual volume of ten of the largest landslides. The actual sediment yield was properly determined with localized "before and after" Lidar analysis of cut and fill. Variability among individual field estimates by nine expert observers was +/- 300%; thus rendering some conclusions of this report equivocal at best. (Note that the largest landslides during a storm commonly deliver most of the sediment.) Many (most?) experienced geologists have abandoned use of these non-scientific field volume estimates.

3) Only limited statements can be made regarding the efficacy of the Slope Stability rules because the study simply does not address the rules or the Forest Practices Applications (FPAs) to which they pertain. In fact, effectiveness of the rules may be much better or much worse than indicated in this study.

Implementation of the rules is based on field verification and protection of RILs as identified in individual Forest Practice Applications (FPAs), yet the authors never looked at any FPAs to determine if potentially unstable slopes were present.

The Department of Natural Resources and industrial landowners employ geologists to assist foresters, mainly in applying WAC 222-16-050-(1) (D) (i)(E), which includes "Any areas containing features indicating the presence of potential slope instability which cumulatively indicate the presence of unstable slopes". These features and indicators are contained in the relevant FPA. If you don't look at the FPA, you don't know whether or not a landslide issued from a RIL or not.

Moreover, many features and indicators of potentially unstable slopes will have been obliterated by the landslides that they caused.

For this reason, one cannot scientifically conclude that "a sizeable fraction of landslides originated from terrain that did not fit the definition of any named RIL, as stated by the authors.

I believe that Washington's slope stability rules can be improved such that sediment delivery to public resources more closely reflects desired natural conditions. However, in a straight up/down vote that did not include the authors of this study, UPSAG decided 2 to 1 with two abstentions against forwarding this report to CMER.

APPENDIX B: AUTHORS' RESPONSES TO MINORITY REPORTS

The next 12 pages contain the authors responses to the minority reports.

The Mass Wasting Effectiveness Monitoring Project

Co-author responses to issues raised in the minority reports

Gregory Stewart, Ph.D. – Geomorphologist, CMER staff
Julie Dieu, Ph.D., L.E.G. – Geologist, Rayonier Forest Resources
Jeff Phillips, M.S. – Geomorphologist, Skagit River System Cooperative
Curt Veldhuisen, M.S., L.E.G. – Hydrologist, Skagit River System Cooperative
Matt O'Connor, Ph.D., L.E.G.[1] – Geomorphologist, O'Connor Environmental

This report summarizes the authors' technical response to the minority reports provided on the Mass Wasting Effectiveness Monitoring Report (Version 8a). We begin by highlighting several key points that serve as background to our position, and conclude with responses to each of the issues raised in the three minority reports.

1. **The study design was carefully chosen after evaluating other options and was determined to be a sound design by the ISPR**. This study went through a scoping process that involved selecting a single study approach from a number of options. Based on the questions that were asked of us, we decided to focus on the landslide response after a high-magnitude storm in order to have an adequate population of landslides for statistical analysis. We also concluded that it was important to evaluate each landslide in the field (not using photos) to minimize detection bias and have confidence in trigger information. We chose a randomized block design to control for spatial variability because we knew there would be many factors that would affect landslide rates such as precipitation intensity, soil depth and topography, etc. Harvest and road treatments were identified to represent categories of forest practices known to be present on the landscape, and the data gathered were widely available or within the scope of the project to collect. The technical merits of many alternatives were discussed in detail and all of the alternatives have significant limitations; most are infeasible or outside the scope of this study's objectives.

2. **The authors designed and conducted this major project while following procedural requirements inherent to a publically-funded, time-sensitive project in the multi-stakeholder CMER environment.** UPSAG followed every step of the CMER process for all phases of the Post Mortem project. This was necessary to accommodate input in a manner that was both timely and equitable to all reviewers. The authors have entertained and responded to extensive input, much of it being raised well after the appropriate stage which has delayed completion.

3. **The authors made extensive efforts to understand and incorporate comments from a large number of reviewers.** The authors submitted drafts to UPSAG (2010), CMER (2010),

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[1] Dr. O'Connor was involved in study implementation and writing of the initial drafts, but did not participate in the drafting of this response.

and ISPR (2011) for review and comment and made significant revisions to the report based on the comments received. After each review, the revised draft went through a subsequent review to determine whether the comments had been addressed. We had abundant input from Dr. Kroll's colleagues at Weyerhaeuser, and relocated numerous UPSAG meetings to their offices to facilitate the attendance of Ted Turner and statistician Steve Duke. Initially, their comments improved the clarity of the report and strengthened our documentation of various uncertainties. At this point, the authors contend that Version 8a discusses and explores uncertainties to a greater extent than any CMER report we are familiar with. We feel the report is sufficiently transparent that readers can see the potential uncertainties and judge the bases for all management conclusions.

4. **Based on our extensive interaction with Dr. Kroll, Dr. Martin and Ms. Lingley, the authors are not convinced that further dialogue is likely to be productive.** Despite efforts over the last three years to understand and s reviewers, we appear no closer to resolution. The authors would like nothing more than to complete this report and provide the most value to policy makers, but we do not see a path forward with respect to issues raised.

AJ Kroll, Ph.D. – Wildlife biologist, Weyerhaeuser

Kroll: "The PM report includes contradictory conclusions"

Dr. Kroll lists two sets of statements to support his argument that the report includes contradictory conclusions. The authors believe these statements are not contradictory when viewed in context.

In the first example, he cites from the Exec Summary "*The Study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the density and volume of landslides*" and from the response to Critical Question 1 in the Discussion, "*This indicates that complete buffering is effective at reducing sediment volumes, but has an indeterminate effect on landslide density*." There is nothing contradictory about these statements when taken in context. The paragraph in the Discussion Section (p. 90) reads:

> *"These findings indicate that harvest without buffers (i.e., No Buffer) resulted in a larger number of delivering landslides and greater volume of sediment delivery than would be expected in Mature forest. In contrast, Full Buffer resulted in a landslide volume that was similar to Mature, but a density that was not statistically different than No Buffer or Mature. This indicates that complete buffering is effective at reducing sediment volumes, but has an indeterminate effect on landslide density.* ***All these comparisons are subject to the interpretation issues noted in Section 7.2.****"* (Emphasis added)

Section 7.2 describes study limitations and factors affecting interpretation of the results. The section includes a discussion of factors that vary with stand age (e.g., root strength and hydrology that affects the interpretation of the results (Section 7.2.4 p. 96-98) and provides an additional analysis (Section 7.2.5, p. 99-100) which supports the hypothesis that buffers reduce landslide density (as well as volume).

The first sentence drawn from the Exec Summary is the topic sentence for a paragraph that describes (in summary) how we came to our conclusions regarding the effect of buffers on landslide density. The paragraph of the Exec Summary that contains the cited sentence reads:

> *The study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the density and volume of landslides. This interpretation is based on the comparison of landslide density in harvested areas with fully buffered and unbuffered RIL against areas of Mature forest, which serves as a control. As expected, harvest units in which RIL were clearcut (i.e., No Buffer) had significantly higher landslide densities and volumes than Mature forest, which confirms that harvest without RIL buffers increases resource impacts. In contrast, landslides in the Full Buffer treatment had a smaller overall volume and delivered less sediment than treatments in which the RIL were clearcut harvested. Interestingly, landslides densities in the Full Buffer and No Buffer treatments were not statistically different from each other. However, mean landslide densities in the Full Buffer treatment were closer to those found in Mature forest than the No Buffer treatment. As described in the report, there are several factors including changes in hydrology and root strength that appear to affect slope stability following harvest; and some of these factors create differences in slope stability that appear to vary with stand age. In a non-statistical analysis of densities among the five harvest treatments, in which each treatment is compared against expected density in the absence of buffering, stands with RIL buffers appear to have exhibited smaller increases in landslide density over those observed in mature forest than was observed in stands of similar ages where buffer influence was not included. This finding lends further credence to the hypothesis that RIL buffers reduce landslide density.*

The second set of sentences that Dr. Kroll suggests are contradictory are: "*This finding lends further credence to the hypothesis that RIL buffers reduce landslide density*" (last sentence in the Exec Summary paragraph shown above), and from the discussion "*In part, because RIL buffers could not be distinguished from riparian or other buffers during treatment delineation, the study tested the combined effectiveness of all types of buffers at reducing landslide initiation and sediment delivery.*" The sentence describing RIL buffers states that the data support the **hypothesis** that RIL buffers reduce landslide density, but we clearly indicate that there may be mature trees in riparian areas of all the treatments and state that we are actually testing total buffer effectiveness. From the Exec Summary (p. IV-V):

> *All buffers, regardless of the reason for which they were left, are included in the treatments, and all delivering landslides, whether initiating in RIL buffers, other buffers or just within the general harvested area or stand, are included in the analyses. This means that RIL buffer effectiveness is not directly quantified, but that total buffer effectiveness, which may include other mitigating processes such as increased LWD delivery and decreased landslide delivery, was tested.*

From the discussion (p. 89-90):

> *This study attempted to test the effectiveness of the most common harvest strategy for reducing landslides, which is to leave forested areas, called buffers, on all RIL located within or adjacent to harvest areas. In part, because RIL buffers could not be distinguished from riparian or other buffers during treatment delineation, the study tested the combined effectiveness of all types of buffers at reducing landslide initiation and sediment delivery. Although harvest within RIL can be performed following SEPA review in some cases, this study did not attempt to evaluate the effectiveness of this type of harvest activity, as it was considered to be infeasible at the broad geographic scale of this study. Thus, the Full Buffer treatment is viewed as most closely approximating the current regulatory approach.*

As in the first case, it is clear that the statements are not contradictory when taken in context. In the authors' opinion, no changes need to be made to the document in response to these examples of "contradictory" statements.

Kroll: "ISPR reviewer 1 noted that in the previous draft, "process weaknesses or inadequacies in the experimental design that may have caused the lack of statistical significant between various treatments may have been overlooked" and that the decision regarding differences in landslide response between treatments should be based on whether the differences were statistically significant."

As noted by Dr. Kroll, this issue was identified in the ISPR review matrix and our response was that "we agree that Section 7.1 should be re-written to further separate the statistical findings and our discussion of factors that may or may not have influenced those findings, especially as they relate to the statistical design." The section was re-written between drafts 7 and 8 per the CMER-approved response matrix. In the latest draft, we clearly state the results based on their statistical significance, and then highlight important issues that have bearing on those tests (Section 7.2 Limitations and factors affecting interpretation of results, p. 93-101).

It is not clear to the authors that anything in the current draft (v8a) needs to be changed to address this issue.

Kroll: "Generally, I contend that the PM report is burdened by two substantial and interrelated problems that the PM authors appear to be unwilling to address or remedy: an inadequate study design and the incorrect interpretation of statistical findings." "The inadequate study design implementation by the PM study is a long-standing, contentious issue that has been raised by numerous reviewers during the development of 8 versions of the report." "Comments made by ISPR Reviewer 1 indicate that the study design issues remain relevant because they influence any interpretations and conclusions made from [the] data."

Contention that the study design is inadequate

As noted above, we disagree that the study design is inadequate and that it has been a long-standing contentious issue. Dr. Kroll is correct in that numerous reviewers have commented on potential limitations to the study design and results over the years; these are discussed in detail in the report. However, many reviewers have noted that other potential alternatives may not be feasible or appropriate to resolve the questions we were tasked to answer. In fact, the contention that the study design is inadequate is limited to these minority opinion reports and previous comments from the same reviewers; many other reviewers including two sets of ISP reviewers have not criticized the fundamental design. There are several areas of disagreement between the authors and Dr. Kroll regarding the strengths and weaknesses of design alternatives the details of which are discussed in subsequent sections.

Inappropriate citation of Reviewer 1 ISPR comments

Dr. Kroll is attempting to employ a statement by Reviewer 1 to inappropriately support his own contentions. Reviewer 1's comment on this topic is "**The co-authors are to be commended for a thorough sampling and analytical design (conventional statistics).**" (page 1, bullet #1). ISPR reviewer 1's primary issue with the study design was that it focused on landslide density following a single storm event as opposed to evaluating changes in the frequency of landslide occurrence through time. Evaluating landslide occurrence through time was never within the scope of the study because it is not as feasible for landslide studies as it is for hydrologic studies. In section 2.5 on design limitations (p. 15) we state that "*the study was limited to evaluating the spatial density of landslide response to a single large storm rather than the temporal frequency of landslides to storms of varying intensity*." We go on to explain why a frequency approach was not incorporated in the study design, namely that a frequency distribution framework is "*not feasible for field-based landslide studies […] because of the long duration between landslide events and the spatial variability in landslide response*." It was never within the scope of this study and does not represent a technical review of the relative merits of this study design. ISPR reviewer 1 clearly would have preferred a study focusing on changes in landslide frequency but does acknowledge that the frequency-based method does not meet our research needs and that a discussion of limitations, which we have done, is sufficient remedy.

From reviewer #1, page 8, last full P:

> *"Although I understand that a frequency distribution framework is not feasible for the experimental design and kind of data collected in the ISPR study, the implication of not invoking the dimension of frequency and not pairing by equal frequency on the results and conclusions of this study should be discussed, especially in light of the more recent literature."*

Aside from the single-storm issue, none of the ISPR reviewers took issue with our implementation of the CMER approved study design.

Contention of incorrect interpretation of statistical findings

We strongly disagree with Dr. Kroll that we are incorrectly interpreting our statistical findings. ISPR reviewers did comment that we should further separate the statistical results and our discussion of factors that may or may not have influenced those results, which was done per the

ISPR comment matrix. Aside from those edits already made, it is not clear that any additional edits are needed to address issues raised by ISPR. The authors believe statistical findings are clearly stated in the current draft.

Kroll: "Changes made in response to ISPR comments are not sufficient."

While Dr. Kroll may feel that the changes made in response to ISPR comments are not sufficient, this is a matter of judgment and the authors and the majority of CMER reviewers disagree with him. As stated above, the authors have attempted to respond to the requests of all UPSAG, CMER and ISPR reviewers. Where there are study limitations, they have been noted. In fact, the Executive Summary discussion starts with limitations of the study and almost half of the pages in the Discussion are dedicated to potential limitations and factors that could be affecting interpretation of results.

For reasons discussed above, the authors are satisfied with the current report. To retroactively change the study design, incorporate data that were not collected, make subjective changes to the data set (e.g., eliminate non-RIL RMZ landslides from the study) or make misleading statistical statements (e.g., stating that because the Full Buffer treatment is not statistically distinguishable from the No Buffer and Mature treatments, it is equal to both) would be inappropriate and unacceptable to us.

Kroll: Evaluation of Critical Question 1 (see Kroll minority report)

In this section of his comments, Dr. Kroll largely summarizes what is already included in the report. As noted above, the current draft includes a large section on limitations and other factors that could have affected the results (Section 7-2, p.93-101). This section was significantly expanded based on comments received in the ISPR review. As noted in the report, there are issues other than management that affect landslide occurrence (which is why we used a block design), and that the ANOVA used to compare harvest treatments cannot account for effects associated with differences in stand age. The report already includes sections that address these issues in detail (see Section 7.2 on Limitations and factors affecting interpretation of results - p. 93-101).

Dr. Kroll correctly notes that we cannot distinguish landslides into 'management' or 'non-management' bins in any given treatment. In fact, most landslides are thought to have multiple triggers which may be a mix of natural occurrences and management influences. As some triggers are difficult or impossible to identify, and as it takes a tremendous effort to quantitatively assess different influences (usually only done for large, deep-seated features that are moving slowly), the notion that there are "management" and "non-management" landslides is a fallacy. Our fundamental inability to identify every landslide trigger and determine factors influencing its initiation was, in fact, a key driver for the choice of study design. In the study design that was chosen, 0-20 year old treatments are delineated by the degree of RIL buffering (management action of primary interest) and these are compared against older stands which are expected to have little management influence. It is assumed that other “natural” landslide triggers are evenly distributed and do not confound the results.

Given the extensive discussions of study limitations in the draft version 8a, it is not clear that anything further needs to be changed.

Kroll: Evaluation of Critical Question 2 (see Kroll minority report)

Dr. Kroll copied the critical question and first paragraph from his CQ 1 review (Critical Question 2 is: “Is the greatest proportion of landslide delivery from harvest units or roads?”). As above, there do not appear to be any specific issues around which we could develop edits.

Dr. Kroll asserts that statistical analyses of delivery volume estimates from 10 of the largest landslides exhibited considerable variability among observers. Variability introduced into treatments can cause there to be spurious results if there is an inherent bias, but unbiased error contributes to the model error and simply makes it harder to detect differences in treatments. We acknowledge that the delivered volume estimates displayed large observer variability (Exec Summary p. III, Results p. 36) but also that the findings are consistent with those for initial volume (Discussion table 7-1, p. 90) which is expected to exhibit less variability because it does not incorporate sediment gains and losses along landslide runouts, which requires much greater observer judgment. Initial volume is included because it was also part of the CMER-approved study design and its use reduces reliance on estimates of sediment delivery (see section 5.2 on Landslide process, volumes and sediment delivery). As elsewhere in Dr. Kroll’s review, the authors’ attempts to openly remedy and acknowledge uncertainty in the writing are then used as an avenue for attack.

In his 4th paragraph, Dr. Kroll notes that the differences in slope gradients among various harvest treatments may be confounding results. Of course, any field study will inevitably have differences among treatment sites being compared, which is why the authors included Table 5-16 and accounted for slope as a covariate in the statistical analysis. Slope was a significant covariate in all the hillslope landslide models and the model of landslide density for all five treatments (r^2 of 0.95 between observed and predicted landslide density). Dr. Kroll goes on to note that Partial Buffer treatment is the only treatment that must have, by definition, a rule-identified landform. Study authors investigated this concern and described findings in detail in Section 7.2.3 (p. 96). In addition to the slope correction, the authors shifted their emphasis to that of critical treatments, which do not include Partial Buffer, in the statistical evaluation of buffer effectiveness.

In his last paragraph, Dr. Kroll takes a separate finding out of context and states that “any interpretations about management-related influences are conjectural because no evidence is available to either support or refute such interpretations." At no point in the report do we attempt to relate site specific triggers to overall differences in landslide density and volume. As described throughout the report, management influences are collectively evaluated in terms of differences in landslide response among the five road and harvest treatments.

Kroll: Evaluation of Critical Question 3 (see Kroll minority report)

In this section, Dr. Kroll appears to be referencing Critical Question 2, which is: "Is the greatest proportion of landslide delivery from harvest units or roads?" This is a relatively clear question that is answered in a straightforward manner in the report.

Dr. Kroll cites a concern regarding temporal dependence among road treatments. We address issues of temporal dependence among the three critical road treatments which are all relatively modern (Section 7.2.6., p. 100-101) as well as other issues related to comparisons among road and harvest treatments (Section 7.3.1, p. 101-103). The block design provides assurance that the roads being compared within a block are of similar age and underwent a similar storm history.

Kroll: Evaluation of Critical Question 4 (see Kroll minority report)

As noted in his comments, **we clearly state** that the study design makes it difficult to identify which individual prescriptions are performing well and which are performing poorly.

In his second paragraph, Dr. Kroll takes a part of a sentence out of context in an attempt to make it appear that we have said something we have not. In his minority report, Dr. Kroll states that we conclude that the effectiveness of RIL buffering is greatest when all RIL are buffered and states that "the effectiveness of RILs cannot be **determined conclusively** based on evidence presented in the PM report." At no point do we state the study has conclusively determined anything with respect to the effectiveness of RIL. In fact, we state that the study results are **inconclusive** with respect to the effectiveness of RIL. The paragraph in question, when read in entirety, already acknowledges Dr. Krolls point:

> *As discussed in response to Critical Question 1, retaining buffers on RIL was found to reduce the volume of sediment delivered to public resources and it also increased the probability that landslides would deliver beneficial woody debris to streams (Figure 5-14). Still, a substantial number of landslides originated within buffers and mature forests, indicating that forest cover does not entirely prevent landslides in a large storm event. As discussed in Section 7.2.5, **although inconclusive,** there is evidence to suggest that the effectiveness of RIL buffering is greatest when all RIL are buffered, yet we found that many RIL had been clearcut harvested since 2001 (Section 5.2.8). This indicates possible implementation difficulties with RIL identification and/or buffer implementation, as discussed further in Section 7.3.3. A potentially useful study would be to determine the relative effectiveness of buffers among the RIL landforms (Appendix B.6).* (Emphasis added).

In the report, we acknowledge that we could not use RIL as the experimental unit and that we are testing total buffer effectiveness. Treatments were, however, delineated in terms of the degree of RIL buffering and the results (and peer-reviewed literature) are entirely consistent with the hypothesis that the effectiveness of RIL buffering is likely to be greatest when all RIL are buffered. It is not clear that anything needs to be changed in the current draft.

Kroll: Evaluation of Critical Question 5 (see Kroll minority report)

Dr. Kroll complains that the study design wasn't adequate to determine triggers for most landslides; in fact, this is not a mechanistic landslide study and there was no attempt to identify or relatively quantify potential triggers of each landslide. The evaluation of triggers was limited to the recording of visible, management-related triggers as listed in the field manual, and a separate QA/QC group of WDNR foresters and geologists largely supported the findings of the field crews. In addition, Dr. Kroll's assertion that "…no numeric measurements were sampled at landslide initiation points..." is inaccurate, since slope gradient, tree age, and initial landslide dimensions were all recorded.

We are surprised by the basic (but sound) finding, which is that not many landslides had a visible, management-related trigger. Dr. Kroll and other reviewers have questioned this shortage of triggers, perhaps because their empirical knowledge tells them that many landslides do have an observable management-related trigger. We reasonably hypothesize that this appears to be a positive result (CQ 4, p. 92-93).

Kroll: Evaluation of Critical Question 6 (see Kroll memo)

Again, it is not clear what Dr. Kroll's issue is, aside from drawing attention to a shortcoming of this study that is already clearly acknowledged. Given the data we have, there weren't enough triggers identified to make meaningful comparisons between road and harvest categories.

In summary, based on his assessment of the critical questions, Dr. Kroll's preference appears to be for the authors to conclude that the study was not able to address any questions successfully and leave it at that. The authors feel strongly that this would be a severe overreaction and that the extensive exploration of results and uncertainties provided in Version 8a is far more appropriate.

Kroll: Kroll Appendix 1 issues

The authors have already responded to each of these issues. They were largely either resolved in edits to the last draft, or are study design issues that the authors have no control over. Readers can consult the 'UPSAG Turner PM non consensus document' that went out with the CMER mailing on 4/17/2012 for complete details.

Douglas Martin, Ph.D. – Fisheries Scientist, Martin Environmental

Martin: Concern 1 – the conclusions do not follow logically from the results

In this section, Dr. Martin focuses on two paragraphs of the Executive Summary (shown below), and indicates that our conclusions do not follow from the results because we reference both statistical findings and non-statistical findings in the Executive Summary.

In response, we would like to point out that we are careful to qualify each of our statements and to identify what information is being used as the basis for the statement. An evaluation of a complex issue like this often requires the use of both quantitative (e.g., statistical) and qualitative information, including other peer reviewed literature, to arrive at conclusions which are consistent with the evidence. The two paragraphs read:

> *The study results support the hypothesis that the avoidance of clearcut harvest on unstable terrain reduces the density and volume of landslides. This interpretation is based on the comparison of landslide density in harvested areas with fully buffered and unbuffered RIL against areas of Mature forest, which serves as a control. As expected, harvest units in which RIL were clearcut (i.e., No Buffer) had significantly higher landslide densities and volumes than Mature forest, which confirms that harvest without RIL buffers increases resource impacts. In contrast, landslides in the Full Buffer treatment had a smaller overall volume and delivered less sediment than treatments in which the RIL were clearcut harvested. Interestingly, landslides densities in the Full Buffer and No Buffer treatments were not statistically different from each other. However, mean landslide densities in the Full Buffer treatment were closer to those found in Mature forest than the No Buffer treatment. As described in the report, there are several factors including changes in hydrology and root strength that appear to affect slope stability following harvest; and some of these factors create differences in slope stability that appear to vary with stand age. In a non-statistical analysis of densities among the five harvest treatments, in which each treatment is compared against expected density in the absence of buffering, stands with RIL buffers appear to have exhibited smaller increases in landslide density over those observed in mature forest than was observed in stands of similar ages where buffer influence was not included. This finding lends further credence to the hypothesis that RIL buffers reduce landslide density.*
>
> *Although we conclude that buffers likely reduce landslide density and sediment volume, it is not clear that existing performance targets for hillslope landslides are being met. The performance target for harvest-related landslides under the current Forest Practices Rules indicates that new harvest should result in virtually no additional landslides triggered by harvest on high risk sites. Three findings raise the question of whether the performance targets are, or will, be met. The first is the lack of statistically significant differences in landslide density for the Full Buffer treatment. Based on this finding, it is not clear that the magnitude of the reduction in landslide densities associated with buffering of currently regulated RIL is sufficient for meeting performance targets. Second, it was observed that 47 landslides initiated on named RIL that were harvested under current rules and subsequently delivered to public resources. As discussed in the report, there are a number of possible reasons for recent clearcut harvest of RIL. Finally, it was found that the Partial Harvest treatment, which included some but not complete buffering of RIL, had significantly greater landslide densities than Mature forest when hit with a large storm event 4-7 years after harvest. Further work will be required to determine whether stands with full buffering of RIL can be expected to meet performance targets if hit by a large magnitude storm at a time when hydrologic and root strength effects are expected to elevate instability.*

This text was completely re-written following the ISPR review and is consistent with the changes made in response to ISPR reviewer comments. To be clear, a failure to reject the null hypothesis in a statistical test may result from any of the following: 1) a type II error, 2) insensitive or inappropriate measurements, and additional variables being confounded with the variable of interest, and 3) too small of a sample size. It is neither unreasonable, nor misleading, to provide ancillary information which provide insight into an issue even though the results of a statistical test are inconclusive. This is reflected in ISPR reviewer 2's summary comment on version 7: "There is a good balance between drawing conclusions from the definitive results of the statistical analysis, and making tentative but reasonable conclusions from field observations where the statistical results are inconclusive."

Martin: Concern 2 - uncertainties and limitations of study design are not fully addressed

Dr. Martin states that a key concern is the absence of discussion about the reliability of measuring landslide density and volume and how variability in these metrics may have influenced the statistical analyses and study conclusions. He goes on to criticize our discussion of blocking (Section 7.2.2., p. 95&96) in which we state that 'we believe the blocking approach was reasonably effective' but do not explain why as well as we might.

We state that we believe the blocking was effective because our statistical model accounted for between 92% and 95% of the variability in landslide density across the study area (predicted vs. observed r^2, p. 74 & 77). We used the same model structure for analyzing initial and delivered volume, but less of the variability was accounted for by the model because these measurements are inherently more variable. As noted above, however, the results for initial volume are similar to those of delivered volume, and both are consistent with the density data and our understanding of landslide processes. We agree that this supporting information would strengthen this assertion and agree to amend the text with the above information on why we believe the blocking was effective. We think we have adequately explained the influence of variability in measurement for the metrics.

Leslie Lingley, L.E.G. – Geologist, WDNR Forest Practices

Lingley: Despite many requests, none of the non-author reviewers have ever seen the actual data.

Data requests are handled by the Adaptive Management Program Administrator (AMPA), not the authors. The report contains all the summary data that was used in the process of drawing conclusions. The raw data would be of little use to most reviewers, but it will become publically available once the AMPA releases it.

If the authors had known that access to the raw data was essential to Ms. Lingley's review of the Post Mortem report, we would have supported such a request. However, this was the first time we'd heard about it.

Lingley: Two of the three response variables (i.e., initial and delivered sediment volume) cannot be quantified without a high precision map of 'before and after' LiDAR or of engineered topographic data.

The landslide measurement techniques used were consistent with the study design and the field manual that were approved by CMER and ISPR. Initial failure volume is based on field measurements of landslide scarp width, length, and average depth measured at every landslide using digital range finders and other measuring tools. Most sediment budgets are predicated on much less information (e.g., polygons digitized from air photos and converted to volume estimates using scour depths from a limited number of landslides). A QA/QC team of WDNR employees found estimates of scarp length, width, and depth to be reasonable in 88-94% of the landslides they visited. The availability of 'before and after' LiDAR for landslide producing events is extremely limited and LiDAR was not available for our entire study. Lastly, this technique for estimating volume has not been formally tested for accuracy as far as we are aware.

Lingley: Only limited statements can be made regarding the efficacy of the Slope Stability rules because the study does not address the rule or the Forest Practices Applications (FPAs) to which they pertain.

Although it could be useful to inspect FPAs associated with landslides included in the study, such an effort would have required a huge increase in the scope of the study. This study was never intended to focus on administrative process or compliance.

APPENDIX C: ADDITIONAL ANALYSIS

This section includes the results of a subset of different analyses that were conducted to answer questions that came up during data analysis, or were part of the original study design but that cannot be used to draw logical inference.

C.1 Under represented treatments

During the development of the Post-Mortem study, the study authors performed bootstrap resampling on 1996 landslide data from the Siuslaw National Forest (D. Miller pers. comm.; Miller and Burnett, 2007). Samples drawn from a range of sample areas were used to help determine the appropriate cluster size and sample size for the Post-Mortem study. While reviewing the Siuslaw data, the authors noted that landslide counts tended to become binomial (e.g. ones and zeros) as the area of a given treatment became small, and densities derived from small areas were likely to have a higher variance than estimates derived from larger areas (Dieu et al., 2008).

To avoid problems associated with binomial counts and high variance in small sample areas, the authors made provisions in the study design to ensure that each treatment would represent at least 5% of the core cluster area (~ 0.2 sq. miles for harvest treatment).

> *"If an individual stratum* (i.e., treatment) *is not contained within a cluster or it occupies less than 5% of the total road network length or timber harvest area, the cluster will be augmented by adding one mile on a side (twelve additional sections). Within the augmented cluster area, a section will be randomly selected and canvassed for underrepresented treatment. Sections will continue to be selected in a counterclockwise direction until the 5% threshold is reached or the twelve additional sections have been used up."*
>
> *"In the event that sufficient acreage of each of the critical treatment cannot be found within or near the four-square-mile block and its one-mile-wide frame, the results will be presented to UPSAG for consultation."*
>
> *"A cluster may be rejected if less than 5% (of the four-square-mile block) of one of the critical treatment cannot be found within the 16-square-mile cluster."*

During the data collection period of the study, aerial photographs were reviewed for harvest treatment. If it was clear that the 5% criteria could not be met within the cluster or through augmentation of units within the frame, clusters were discarded. Crews' field verified and calculated treatment area for clusters that were included in the study. At the end of the field season it appeared that treatment in each cluster met the 5% criteria.

In the QA/QC period of the study, a number of the harvest treatment determinations were changed. Some of the clusters lost treatment entirely and others were found not to meet the 5% criteria. The study design stated that clusters could be rejected if treatment occupied less than 5% of the study area, but gave no guidance for determining when or how to reject a sample for which data had already been collected. Given the original concern that small areas might provide biased density estimates, an analysis was conducted to determine how sample estimates changed with sample area.

Using pre- stand age QA/QC data, a permutation resample was performed over the entire study area using a range of sample areas.[1] Over twenty-one million distinct, but often overlapping, density estimates were extracted. These estimates include all possible permutations of landslide density that could be extracted for a given sample area. Sample areas ranged from 1.9 square miles (~50% of cluster area) to 0.019 square miles (~0.05%). Differences between the subsample density estimate and whole cluster landslide density were then calculated for each cluster.

As Figure C-1 shows, as sample area becomes small, the variance of the density estimate increases and individual density estimates become less reliable. This finding is to be expected as landslides are not uniformly distributed across clusters, but instead tend to be clumped together. Although the graph shows increasing variance, very little change occurs with regard to the mean of the sample. This suggests that samples drawn from small areas may be less precise, but are unlikely to be biased.

Mean cluster areas for harvest treatment range from 358 acres to 727 acres (14% to 28% of the average cluster area) and no single treatment is under-represented in all clusters. Strata that are less prevalent in one cluster tend to be more prevalent in another cluster. While care was taken to balance the area in each treatment by providing a minimum area criteria, there do not appear to be reasonable objective criteria for eliminating data that have already been collected. The loss of data in an RCB design is likely to have a much greater affect on statistical power than the inclusion of less accurate estimates. To help account for the error introduced by densities derived from small areas, sample area was used as a weighting factor in the GLM's. In this way, samples drawn from larger areas had a proportionately greater influence on the results than samples drawn from small areas.

C.2 Justification for block-scale pooling of landslide data

Pseudoreplication is the testing of treatment effects using an error term inappropriate to the hypothesis being considered, and it often occurs when samples have been collected in close proximity to one another resulting in correlated (i.e., non-independent) responses (Hulbert, 1984). There are two solutions to pseudoreplication associated with spatially nested sampling designs; 1) treat blocks as a random sample from the population of blocks, and to perform the analysis on block summary statistics (Sit and Taylor, 1998; Thompson, 2002); or 2) analyze the landslide response in each experimental unit, and to properly account for the nesting of spatial scales through the use of nested linear models (Quinn and Keough, 2002).

Early in the review process it was noted that pooling of data resulted in the calculation of auxiliary variables at a very coarse scale, and that nested linear models could be used to reduce the size of mod-

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[1] Permutation resampling was performed with the focal sum function in ArcGIS using 10x10m grid of landslide counts, a square neighborhood and the NoDATA keyword.

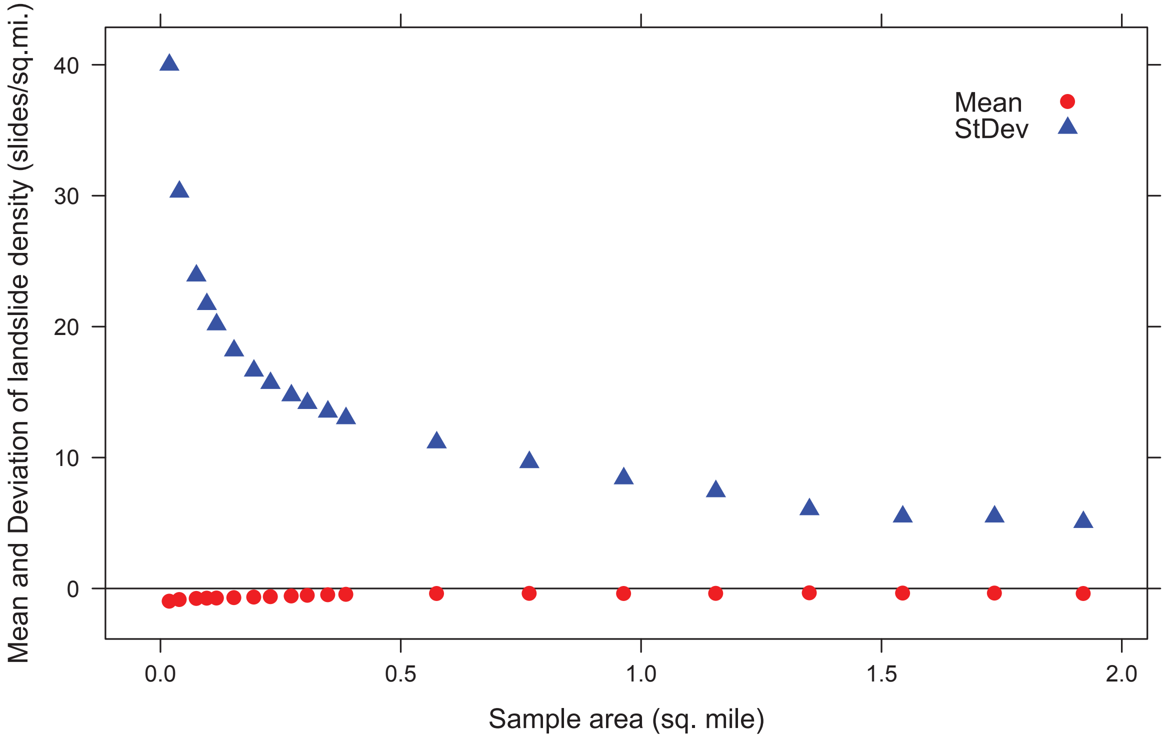

Figure C-1: Mean and standard deviation of landslide density estimates (landslides per square mile) as a function of sample area based on permutation resampling of landslides in Post-Mortem clusters.

el cells to the scale of individual experimental units (as opposed to pooling at the scale of blocks). It was argued that the auxiliary variables (e.g., topographic indices) calculated at the scale of an experimental units would be more likely to be correlated with landslide response. While this may be true, we feel that study design necessitates the pooling of data because individual experimental units vary so greatly in terms of exposure.

As noted previously, experimental units were *ex post facto* classified into treatments so there was no control over the number of experimental units or the total area assigned to a treatment in a given block. Experimental unit boundaries were defined by stand age (i.e., buffer composed of older timber were explicitly delineated) and clipped to the boundaries of blocks, and as a consequence, some experimental units were arbitrarily small. While ratio estimators can be used to account for the effect of different exposures (See Section 2.3.2), they do not resolve the problem that different treatment sizes create different response distributions. At very small exposures landslide counts are likely to be binomially distributed, but as the size of the experimental unit becomes large, the response is likely to be Poisson distributed. If a dataset containing a large number of zeros associated with insufficient exposure is analyzed using a Poisson model, the estimated parameters may be biased and the excessive number of zeros can cause overdispersion (Zuur et al., 2009).

To estimate the potential effect of zero-inflation, the harvest landslide dataset was analyzed using Zero-Inflation Poisson (ZIP) models. ZIP models treat the data as though it were derived from two different processes, a binomial process and a count process (Zuur et al., 2009). PROC Countreg from SAS was used to perform the ZIP modeling on the unpooled harvest landslide dataset. The minimally adequate model results indicated that size of an experimental unit was a strong predictor of whether the count was binomial or Poisson distributed, with the probability of generating a binomial count exceeding 50% when the area of a harvest treatment polygon area fell below 19.3 acres. Approximately 58% of the harvest treatment polygons in the harvest treatment dataset have areas less than 19.3 acres. Unfortunately ZIP models do not currently allow for the nesting of fixed and random effects and therefore cannot be used for statistical analysis of treatment responses.

It is our contention that the large number of zeros within very small experimental units represents insufficient exposure, as opposed to low inherent risk of failure. While we recognize potential advantages of performing analyzes at other scales, the compromise we chose was to perform the analysis on summary statistics calculated at the level of treatments and blocks, which is consistent with the analysis performed for the study design.[2] Under this framework, the analytical model is similar to that for a randomized block design.

We see the primary disadvantage of performing analyses on summary statistics is being the inability to incorporate interaction effects (a multiplicative model). Although the potential for an interaction between treatment and block has been proposed (perhaps as a function of precipitation intensity), we see no evidence of interactions and feel that the inability to test for them is not an issue for this study. With the current model, interaction effects are incorporated into the error term, so that the resulting model is conservative with respect to interactions.

C.3 Observer variability in estimates of delivered volume for large landslides

Near the end of the field season, 10 members of the field crew were asked to estimate the delivery volume (cubic feet) for 9 large landslides in one of the clusters. One of these 10 crew members was one of the two experienced geologists on the crew. The cluster was chosen for both high landslide density and for the existence of pre- and post-LiDAR datasets. The individual landslides were selected for their ease of field access and so that a volume estimate had already been done by the second experienced geologist who could not be present for this analysis. The goal of the exercise was to evaluate observer variability in estimations of delivered landslide volume.

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[2] The problem of excess zeros in small sample areas was noted in a presentation to UPSAG in August 2006 based on an analysis of landslide occurrence in the Siuslaw National Forest following the 1996 storm in Oregon. It was the primary motivating factor in setting a minimum treatment area for critical treatments within blocks. While the data could be analyzed using a binomial model, those models are not appropriate for the larger experimental units with landslide counts greater than one.

Another method of estimation was also attempted as a comparison. Pre- and post-storm LiDAR datasets were used to derive cut and fill volumes on the hillslope; the difference between cut and fill volumes were assumed to have been delivered to the channel network. Elevation differences of less than 1 meter between the LiDAR datasets were not counted to remove some of the noise from the dataset.

Basic plots of the data revealed that there was significant variation in estimated delivery volume among observers and the LiDAR results (Figure C-2). For some landslides, the estimated delivered sediment volumes are at least in the same order of magnitude (e.g., 1204275845 on the far right side of Figure C-2). For others, the volumes vary by more than two orders of magnitude (e.g., 1204260283). Field crew attributed differences in estimation to difficulties in estimating the original depths of material in debris flow tracks (i.e., the volume a debris flow entrains as it scours down a stream valley) and to difficulties in estimating the remaining deposit at the base of the hill as compared with the volume that flowed across the floodplain and into the channel.

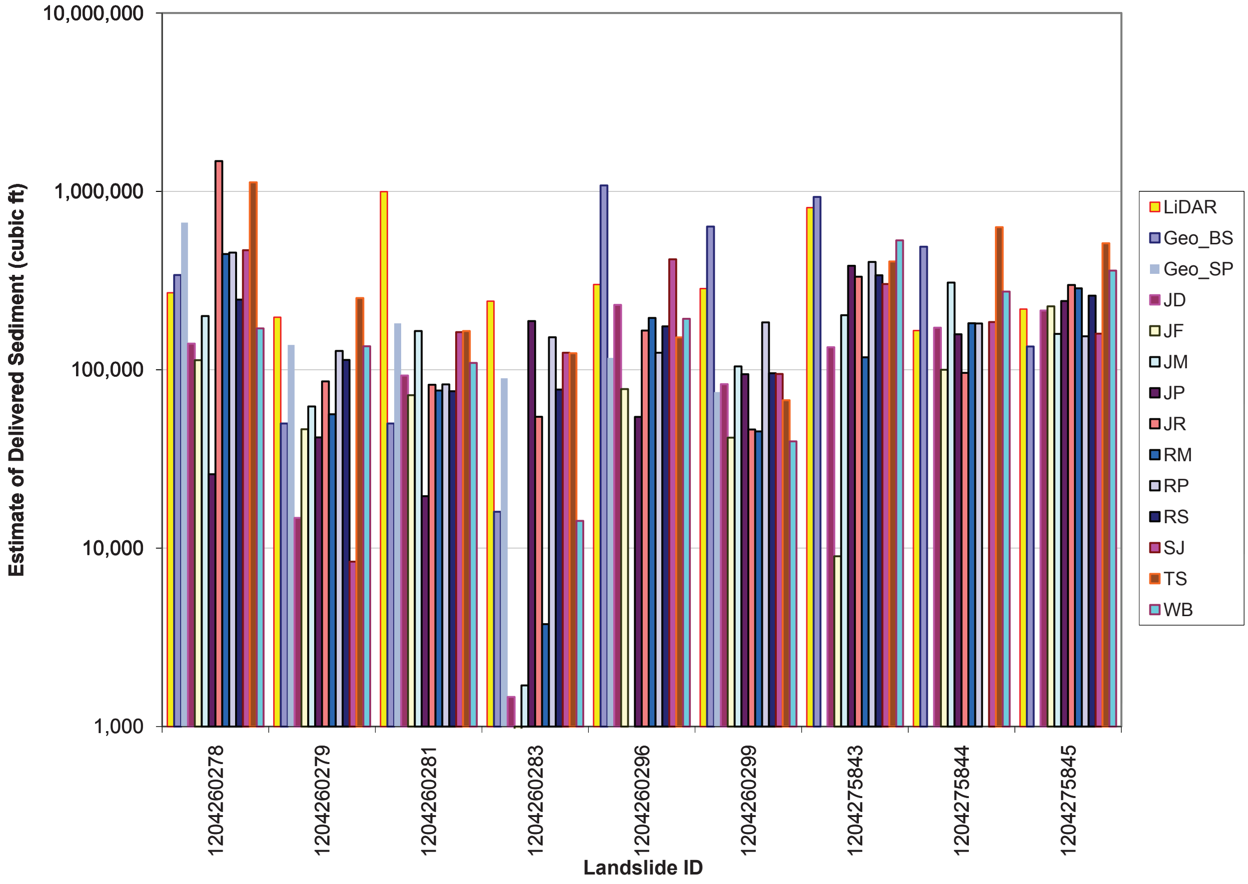

Table C-1: Observer estimates of delivered sediment volume (cubic feet) by landslide. Each bar represents a different observer.

In addition to the visual observations of observer variability, the data were analyzed statistically. Linear modeling using log-transformed landslide volumes (to reduce heteroscedasticity in model residuals) indicated that both Landslide_ID and Observer_ID were important factors in determining delivered sediment volume (Table C-1). To evaluate how exactly the variation of measurements changed, a variability gage analysis (REML method) was conducted within JMP. The variability gage analysis showed that Landslide_ID explained only 37% of the variance in the measurements of landslide volume (Figure C-3). Approximately 9% of the variance was explained by the person who took the measurement, and 54% of the variance was unexplained or was associated with the interaction between observer and landslide. The results indicate observer and landslide number are both important predictors of estimated delivery volume, but that the unexplained variability still exceeds explained variability even when both factors are known.

While the accuracy of the LiDAR estimates has not been tested, it is interesting that those measurements are consistently among the higher of the observer estimates or are higher than any of the observer estimates. This may indicate that observers are most often underestimating delivered volumes.

Table C-2: Effect tests for Landslide ID and Source (of the observation).

Source	Nparm	DF	Sum of Squares	F Ratio	Prob > F
Landslide ID	8	8	62.321460	9.5584	<.0001*
Source	12	12	23.688752	2.4221	0.0090*

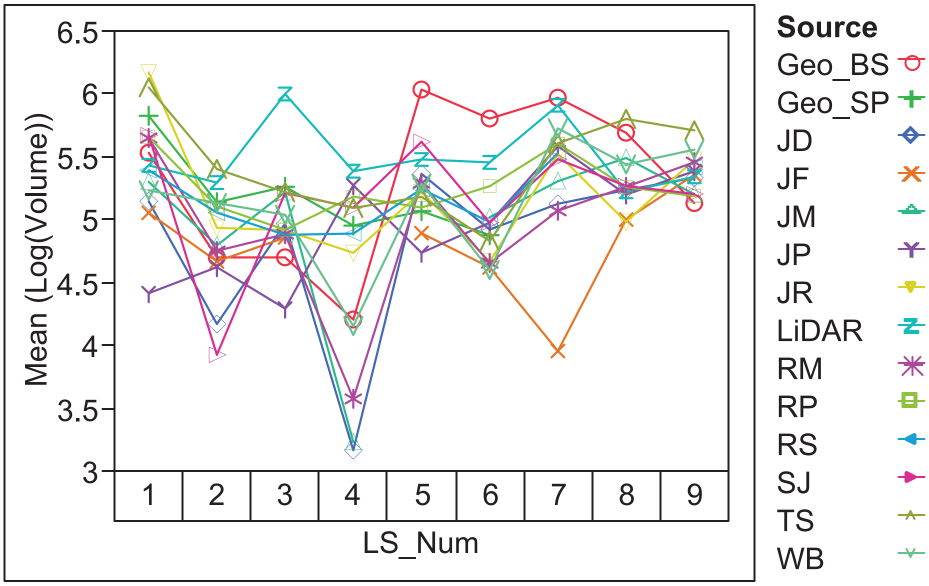

Figure C-2: Interaction between observer and landslide id in estimating (log-transfomed) delivered volume.

However, it should be noted that many of these nine landslides were debris avalanches or debris flows with a debris avalanche component; the large initial failure volumes may be accurately estimated by the LiDAR, but the shallow debris avalanche deposits (usually <1 foot) were not estimated as fill because of the less than 1-meter screen.

C.4 Landslides and landforms in the Partial Buffer treatment

The Partial Buffer treatment is a high percentage (55%) of the land harvested after the inception of the current Forest Practices Rules (Section 5.8.2) and this surprising result coupled with the treatment's comparatively high landslide density raise questions about its nature. It was supposed to be a catch-all for a variety of operational circumstances which were hypothesized to include commercial thinning of RIL or yarding corridors cut across RIL (Dieu et al., 2008), and it was expected that there would be only an occasional harvest unit unless Watershed Analysis Prescriptions had caused pre-FFR partial buffering. Although not a component of the study design, UPSAG decided it would be valuable to policy makers to characterize the harvested landforms in the Partial Buffer treatment. These results, presented below, also informed the QA/QC of the harvest treatments that was conducted during the stand age analysis.

The characterization of the Partial Buffer treatment was initiated with a random selection of two harvest units from each block. Not every block contained two harvest units of PB, and not every harvest unit designated PB was PB (a problem corrected during the QA/QC process). If a harvest unit was determined to not be PB, then the next closest harvest unit of PB was included in this analysis.

The description of the Partial Buffer (PB 0-20) treatment is based on 34 harvest units covering 2132 acres spread among the 22 blocks. Two UPSAG geologists, with extensive landslide inventory and hazard mapping experience, mapped both buffers and individual RIL within and adjacent to each polygon of the Partial Buffer treatment. The mapping effort utilized stereo aerial photography, 10-meter and LiDAR DEM and SlpStab.

Six of the 34 Partial Buffer units sampled (18%) in the descriptive analysis were likely harvested under a pre-FFR FPA; 26 PB harvest units were likely harvested under a post-FFR FPA. This designation was based initially on an estimate of stand age from the stereo photos and subsequently verified or changed with the use of the landowner stand age data.

Mainstem inner gorges were mapped separate from tributary or sideslope inner gorges because there were large discrepancies in the buffering of the two types. In general, mainstem inner gorges were buffered in both pre-FFR and post-FFR harvest units. Of 20 mainstem inner gorges, 80% were completely buffered, and 15% were partially buffered (Table C-2). Sideslope inner gorges were less likely to be buffered. Of 78 distinct sideslope inner gorges, only 35% were buffered, and 24% were partially buffered, leaving 41% unbuffered. Where partial buffering of sideslope inner gorges occurred, it was often a small portion of the lowermost inner gorge that was protected in the riparian management zone or within the buffer of the mainstem inner gorge. The partial buffer call was reserved for

Table C-3: Summary of RIL buffering for a randomly selected set of Partial Buffer units.

Landforms	Buffered Acres	Unbuffered Acres	No. of Rule-ID Landforms	No. of Completely Buffered	No. of Partially Buffered	No. of Un-buffered
Mainstem Gorges	72.01	7.57	20	16	3	1
Sideslope Gorges	32.05	17.19	78	27	19	32
Bedrock Hollows	12.99	23.52	125	27	12	86
Toes of DSL	0.20	0.31	3	0	1	2
Totals	117.25	48.59	226	70	35	121

sites where significant lengths of the sideslope inner gorge were unbuffered. Bedrock hollows were most often unbuffered. Of 125 bedrock hollows, 22% were buffered, 10% were partially buffered, and 68% were unbuffered. In summary, approximately 40% of the area of RIL in the Partial Buffer treatment was unbuffered and 60% was buffered.

There are several caveats to consider in evaluating this analysis. First, inner gorges are quite easy to identify and delineate from aerial photography within or adjacent to recent clearcut areas because the slope break is such a definitive feature. However, it is possible that field efforts would reveal that a small subset of the sideslope inner gorges were misidentified or would not deliver into the channel network. Second, RIL bedrock hollows are harder to conclusively identify from aerial photography and other remote tools because the 70% gradient requirement is difficult to verify. As mentioned above, the team attempted to be conservative in these designations, mapping only those bedrock hollows that appeared to be substantially greater than 70% and not mapping those that appeared close to the threshold. Despite these efforts, it is possible that a small subset of the bedrock hollows were falsely identified or are unlikely to deliver to the channel network. And, lastly, the toes of deep-seated landslides were seldom encountered and the results of this remote interpretation should be considered with that in mind.

C.5 Other micro-hypotheses

The study design includes a set of 'micro-hypotheses'. Some were included in the body of the report, but others either failed to allow for logical inference or could not be resolved.[3] As a result, it is unclear whether inference drawn from the data relate to landslides or characteristics of the landscape. No inference should be drawn from these results.

H5: Landslides will occur in association with buffer blowdown.

Blowdown within buffers on potentially unstable slopes has the potential to reduce rooting strength and potentially increase landslide rates in buffered areas subject to blowdown.

Data preparations for evaluation of this hypothesis include:

- Limited data to landslides where a pre-storm blowdown determination was made.

Of the 634 landslides where a buffer blowdown determination was made, only 35 landslides were associated with units where the percentage of buffer blowdown exceeded 10% (Figure C-4). The average percentage of buffer blowdown was 4%. A reasonable interpretation of these data, supported by extensive air photo and field observations, is that buffer blowdown is very limited across most of the Post-Mortem study area. Nothing can be concluded about the micro hypothesis, but UPSAG has learned that the Mass Wasting Buffer Integrity and Windthrow Assessment Project will require near-coastal implementation if the relationship between buffer blowdown and landslide occurrence is to be evaluated.

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[3] Hypotheses that used data from landslide initiation points but that related to landscape characteristics, provide no logical inference because it isn't clear whether they relate something about landslides susceptibility or simply characterize relative abundance across the landscape. See R2 for an example.

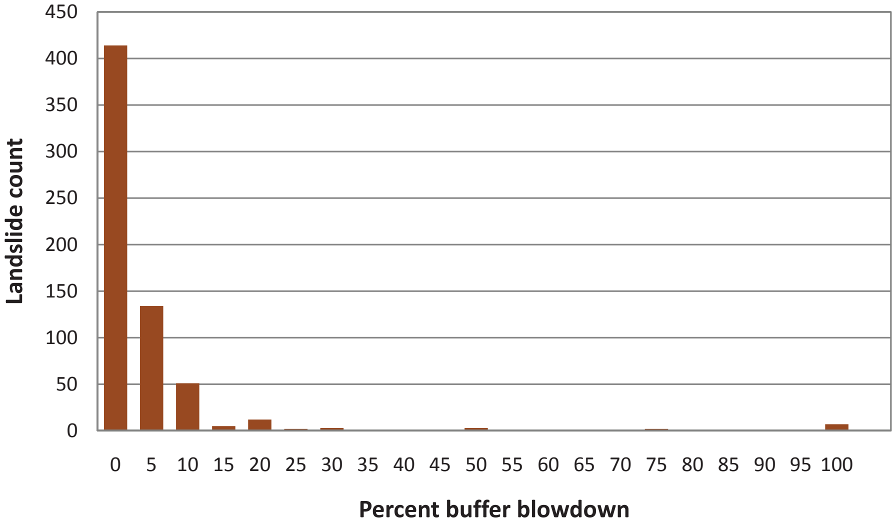


Figure C-3: Landslide count as a function of the percentage of buffer blown down (n=634)

H6: Harvest upslope of unstable landforms will increase landsliding.

This hypothesis was intended to evaluate how hydrologic changes associated with forest harvest affect landslide susceptibility in down slope locations. The database developed for the study does not include a layer that identifies rule-identified (or other) unstable landforms. It was not possible to evaluate this hypothesis without information on the location of unstable landforms in the study area, so this hypothesis was not evaluated.

H7: Landslide delivery will be inversely proportional to buffer/riparian stand width and density.

This hypothesis was intended to evaluate whether larger trees and/or dense stands of trees reduce the volume of sediment delivered to streams. The presence of larger, denser trees is hypothesized to create a mechanical barrier to transport of mobilized sediment.

Questions related to riparian stand density could not be analyzed because the riparian stand density (e.g., basal area per acre) was never collected. With regard to stand width, it was determined that the variability inherent in estimates of delivered sediment volume (see Appendix C.3; Miskovic and Powell, 2009) would be compounded with errors associated with determining stand width. For the purpose of this question, average width should be calculated along the runout path, but landslide mapping only included initiation points. In the absence of better data, this hypothesis could not be effectively tested.

H9. Focused water from upslope roads will be associated with hillslope landslides.

This hypothesis was not evaluated because it requires evaluating the contributing area for each individual landslide and is likely to require explicit information on drainage structure locations, which were not collected in this study. This hypothesis may be evaluated in the future following additional information gathering.

H10. Landslides will occur along yarding corridors.

This hypothesis was intended to evaluate the effect of yarding corridors on landslide initiation, but was not evaluated because yarding corridors were not mapped. However, very few hillslope landslides were associated with yarding corridors, suggesting that yarding corridors may not be a significant triggering factor. This hypothesis is probably not worthy of further work.

R1: Landslides will occur on planar slopes with no or insufficient pullback.

This hypothesis was intended to evaluate whether there is a greater number of landslides on planar road fillslopes that have not received sidecast pullback. It was hypothesized that these areas may have been overlooked during pullback operations because they are not included in current RIL definitions (Dieu, et al., 2008).

Data preparations for evaluation of this hypothesis include:

- Limiting data to ‘Hillslope Road’ slides (excludes ‘Road Crossing Slides’);
- Limiting data to Mitigated and Abandoned road treatment.

Analyses conducted thus far are indeterminate. This question may be evaluated as part of a follow-up study.

R2: Small stream-crossing pipes will be associated with landslides.

Undersized stream-crossing structures can contribute to the frequency of landsliding when water is diverted around the structure and onto unstable soils (Dieu et al., 2008). This hypothesis seeks to establish whether a correlation exists between the diameter of stream-crossing pipes (relative to the size of the stream) and frequency of stream-crossing failures. Impaired flow capacity through culverts has been observed to be a common cause of stream-crossing failures in other studies (Furniss et al., 1991).

Data preparations for evaluation of this hypothesis include:

- Limited data to stream-crossing landslides with culverts where the culvert size was observed.

It was not possible to determine culvert diameter at many sites because the culvert had been transported down stream. In addition, field data on channel width (as an index of peak flow) intended to relate culvert diameter to stream size, were not reliable because of the extent of flood impacts on channels. Therefore, it was not possible to evaluate whether “undersized” stream crossings had an increased risk of failure.

The analysis indicates that culvert size alone is a poor indicator of landslide susceptibility. Once normalized by their relative distribution on the landscape, most culvert sizes have similar probabilities of failure (Figure C-5).

R3: Inadequate water control measures will be associated with landslides.

This hypothesis was intended to evaluate whether landslides were associated with poor road drainage. Field data that were collected to help address this hypothesis include a determination about whether the landslide was associated with pirated water, too few drainage structures, inadequate ditch design and construction, or a ditch not flowing to an appropriate drainage point.

Data preparations for evaluation of this hypothesis include:

- Limited data to landslides where pirated water observations were specified as ‘yes’ or ‘no;’
- Limited data to landslides where upslope road drainage distance was observed;
- Limited data to landslides where ditch flow was characterized as continuous, discontinuous or ponded.

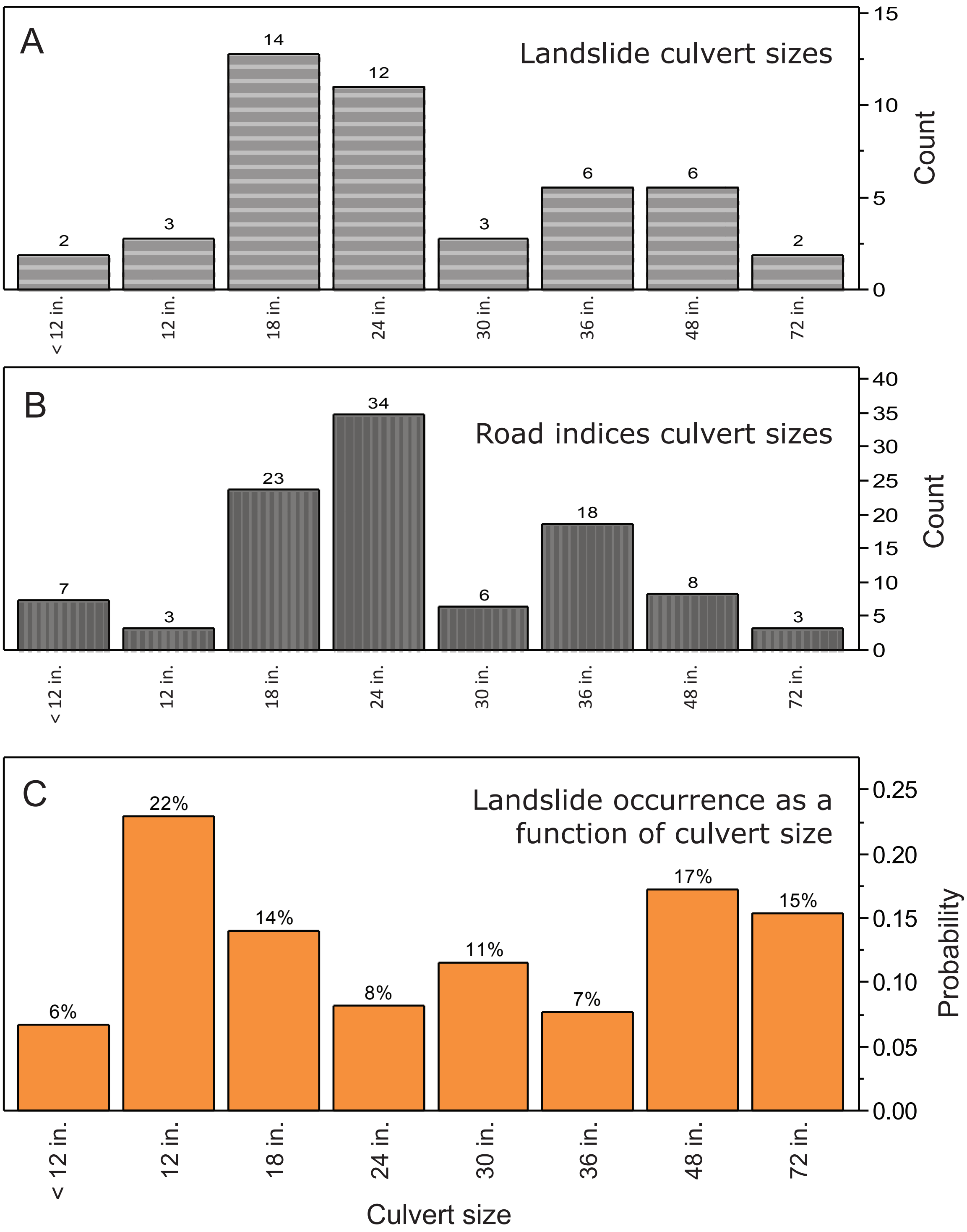


Figure C-4: Landslide occurrence as a function of culvert size. **Culvert sizes associated with stream-crossing landslides (A) were inversely weighted to their estimated distribution on the landscape (B) to determine landslide occurrence as a function of culvert size (C). Differences among culvert sizes are unlikely to be significant.**

Initial analysis of this hypothesis examined the frequency of landslides associated with each of the drainage factors listed above. Whether there is an increased risk of landslides associated with any of the drainage factors listed above is indeterminate due to an incomplete knowledge of the relative distribution of each of the factors.

R4: Poor tread maintenance or inappropriate road geometry will be associated with landslides.

Similar to micro hypothesis R3, this hypothesis evaluates field evidence regarding a correlation between road drainage geometry (e.g., crowned, insloped, outsloped), road tread condition with respect to surface drainage (e.g., adequately graded, potholed, rutted), and landslide frequency. Local road drainage conditions may, in some circumstances, cause concentration of runoff that could contribute to slope failure.

Data preparations for evaluation of this hypothesis include:

- Limited data to landslides where a road geometry was observed (i.e., not blank);
- Limited data to landslides where a road tread condition was observed (i.e., not blank).

Initial analysis of this hypothesis was accomplished by examining the frequency distribution of landslides associated with road geometry and tread condition (Figure C-6). Results of the initial analysis were indeterminate. For example, a high proportion of landslides were at sites with adequately graded road surfaces. This does not suggest that adequately graded road surfaces are more susceptible to failure but that a high proportion of the roads are adequately graded. In the absence of data on the distributions of road geometry and tread condition throughout the study area, the relative risk for landsliding cannot be assessed and this hypothesis cannot be tested.

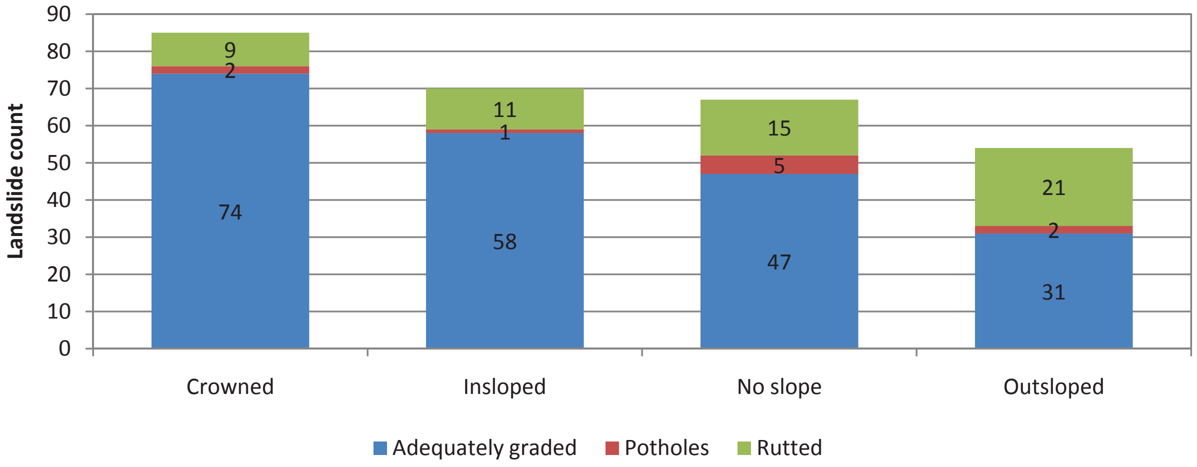


Figure C-5: Distribution of road landslides as a function of road geometry and tread condition.

C.6 Landslides in riparian areas

Landslides in riparian areas may be triggered by hillslope failure processes or they may be initiated by in-channel processes. The extent to which in-channel processes are related to forest management activities is not completely understood and therefore, in this study, it was considered important to include riparian landslides in the sample design in order to evaluate overall buffer effectiveness. The following analysis was completed to determine the number of landslides that initiated in riparian areas in order to evaluate their potential impact on the results of this study.

For this analysis, landslides that initiated within 115 feet of Type F or Type S streams on the WDNR hydro layer were identified as riparian landslides. The choice of 115 feet as the Type F riparian buffer width was based on a review of typical buffer widths measured from aerial photos.

Table C-3 contains the summary data for delivering riparian landslides. The number of riparian landslides was similar in all treatments. The percentage of the total number of landslides that initiated in riparian areas was lowest in the Partial Buffer treatment (12%) and highest in the Full Buffer treatment (36%). The proportion of riparian landslides initiating in RIL ranged from 70% in the Partial Buffer treatment to 85% in the Full Buffer and Mature which is larger than the 55% for all delivering landslides.

The effect of riparian landslides on total landslide density in treatments was evaluated by determining the number of landslides and the area of each treatment within riparian buffers. Landslide densities in the each treatment were then adjusted accordingly. The change in landslide density was relatively similar in all treatments (Figure C-7). Full Buffer had the largest reduction in landslide density from 5.9 to 4.0 (slides / sq. mi.). Given the relatively even distribution of riparian landslides in all treatments, we conclude their inclusion does not significantly influence the results of this study. Further, we argue that their inclusion is critical to completely evaluate buffer effectiveness, particularly in light of the high percentage of riparian failures that occurred in RIL.

Table C-4: Summary data for Type F riparian landslides by harvest treatment and the percentage of riparian landslides initiating in RIL.

Treatment	Type F Riparian landslide count	Total delivering landslide count	% riparian landslides	% riparian landslides in RIL
No Buffer	30	160	19%	77%
Partial Buffer	30	244	12%	70%
Full Buffer	26	72	36%	85%
Submature	48	258	19%	81%
Mature	47	204	23%	85%

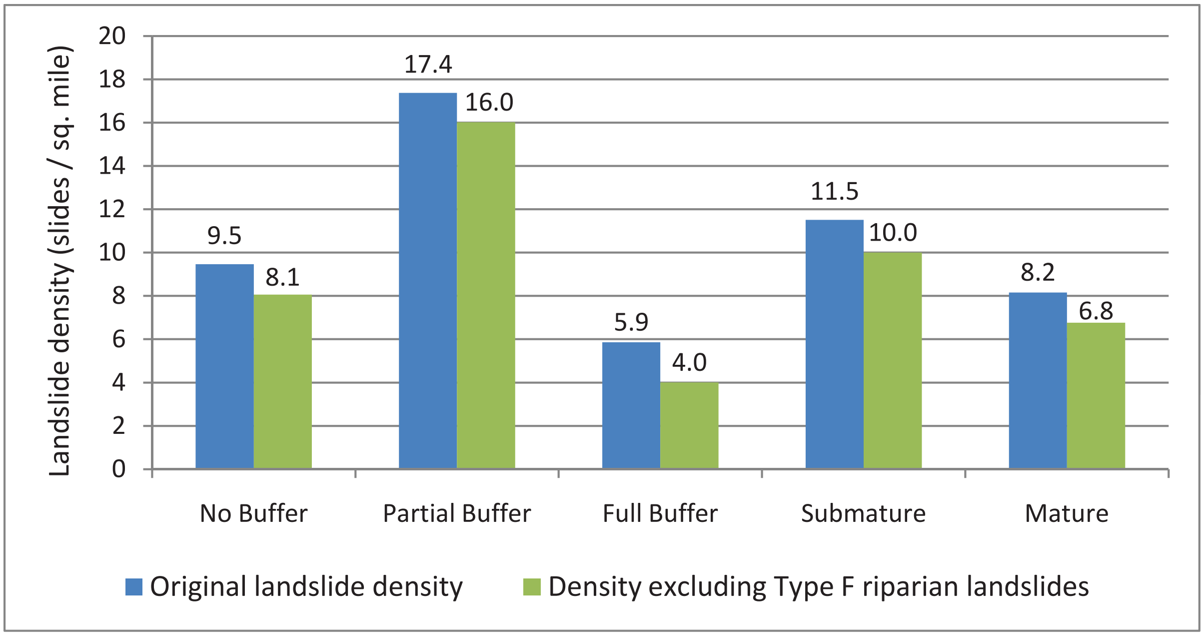


Figure C-6: Histogram of landslide density (landslides per square mile) by harvest treatment for all delivering landslides compared with landslide density excluding those originating in Type F riparian buffers. Landslide density does not include block replication or any normalization other than area (e.g., does not account for precipitation or topography).

APPENDIX D: PROPOSALS FOR FUTURE RESEARCH

Some of the results of this study raise questions that require additional analyses that are beyond the scope of this study to address. Those that UPSAG is most interested in pursuing as future research topics are discussed below in a brief scoping-style of presentation where the issue is presented, the additional data analysis is explained and the expected outcomes are conceptually described.

D.1 Testing the Accuracy of Unstable Landform Identification

<u>Issue</u>: It was predicted in the Post-Mortem study design that there would be some Partial Buffer of RIL under current Forest Practices Rules; it was expected to occur in limited acreage where operational limitations required that a yarding corridor be cut across an RIL or where an RIL had been missed during engineering and regulatory review. It is a surprising result that 50% of the study area harvested since 2001 was stratified as Partial Buffer.

<u>Proposed project</u>: UPSAG has scoped and written a study design titled "Testing the Accuracy of Unstable Landform Identification." The study is designed to randomly select completed harvest permits across Washington State subject to the current Forest Practices Rules to determine if RIL are being accurately identified and protected. In cases where RIL that have the potential to deliver sediment to streams were partially or completely harvested, the study is designed to identify where in the process (harvest unit layout, application review, or harvest operations) problems are occurring. As this study goes through CMER and ISPR review, UPSAG intends to modify the study design to simplify the field data collection. This project is described in the 2011 CMER Work Plan.

<u>Expected outcomes</u>: Testing the Accuracy of Unstable Landform Identification will quantify the occurrence of RIL protection and non-protection and will illuminate the reasons for the non-protection. This will occur across Washington State and will place the occurrence of the Partial Buffer treatment in the Post-Mortem Project in a broader context (but may not provide a detailed or statistically rigorous answer for the Post-Mortem Project area itself).

D.2 Testing the Assumption of Evenly Distributed RIL

<u>Issue</u>: Statistical differences in landslide density between harvest treatments have been questioned during the UPSAG review process; in particular, because it is unknown whether RIL are distributed equally in each treatment. An uneven distribution of RIL could lead to an unequal risk of landsliding which might influence the results. The signal of buffer effectiveness is small compared with basic factors like slope gradient and precipitation, and it has been determined that the mean slope of the Full Buffer treatment is 6-7% lower than the mean slopes of the other treatments.

<u>Proposed project</u>: It may be possible to test the assumption of evenly distributed RIL by conducting analyses similar to the characterization of the Partial Buffer treatment for other treatments. The

Partial Buffer analysis was accomplished through a several day effort to map in GIS both buffers and RIL (by type) in randomly selected polygons of the Partial Buffer treatment. The effort to map RIL would need to be extended to randomly selected polygons of the Mature and Full Buffer treatments. Accurate mapping of RIL in the Mature treatment is particularly confounded by canopy closure on the 2007 aerial photography, a bias that might be avoided by utilizing older aerial photography.

Expected outcomes: Validation of the assumption of even distribution of RIL would allow us to more strongly assert that it is the buffering of RIL that causes Full Buffer to have lower landslide density than No Buffer or Partial Buffer. If the assumption is not validated, then the resultant data may assist in the multivariate logistic regression analysis of site-specific factors to better understand landslide susceptibility and differences between the treatments.

D.3 Analysis of Landslide Occurrence Outside of RIL

Issue: Delivering landslides that occurred outside of RIL in the Post-Mortem Project area were 47% of the total delivering hillslope landslides. This result is higher than expected and based on previous Watershed Analyses and Landslide Hazard Zonation (LHZ) products for many Watershed Administrative Units (WAU) across western Washington.

Proposed project: The landslides that occurred outside of RIL should be characterized by hillslope gradient and form, by detailed lithology and geomorphology (e.g., relationship to larger scale features such as ancient earthflows), by process and, to the extent possible, by contributing factors. These results should be analyzed for susceptibility in the broader (local) landscape context, including with respect to the occurrence of landslides within RIL.

Expected outcomes: Analysis of the landslides that occurred outside of RIL may identify shared characteristics or processes that may explain their occurrence and distribution. This, coupled with a basic understanding of landform distribution and risk, may help Policy make informed decisions.

D.4 Multivariate Logistic Regression Analysis of Site-Specific Factors

Issue: Post-Mortem was not designed as a mechanistic study and it did not require identification of the physical processes initiating each landslide to address the research questions related to the effectiveness of Forest Practices Rules. However, the extensive field-based nature of the study resulted in the creation of a powerful data set which could be potentially valuable to increase our overall understanding of landslide processes and risk.

Proposed project: To conduct multivariate logistic regression analysis for site-specific factors such as contributing drainage area for all landslides in the Post-Mortem Project area.

Expected outcomes: This analysis could provide a more complete understanding of the relative importance of the many factors that contribute to landslide occurrence, putting into a broader context our knowledge of any individual set of landslides. The output from this analysis could also be used to compare Post-Mortem landslides with other related studies and/or to test slope stability models.

D.5 Evaluation of Road Fillslope Landslides for Susceptibility

Issue: One hundred sixty-eight fillslope failures were sampled. When fillslope gradient is plotted against hillslope gradient, the result is a scatter diagram that fails to validate two simple beliefs about fillslope failures: 1) that perched fill (fillslope steeper than hillslope) is a common cause of fillslope failures; and 2) that most fillslope failures occur on natural hillslope gradients that exceed 70%.

Proposed project: To attribute the Post-Mortem road network by gradient category, by topographic position, by perch, and by hillslope shape so that we can better characterize fillslope susceptibility.

Expected outcomes: To better inform forest managers about where it is most critical to accomplish mitigation measures such as better water control and sidecast pullback. Although few details are available in the current scientific literature, landowners are expected to do this work to reduce landslide occurrence on existing roads.

D.6 Evaluation of Buffer Effectiveness by Landform

Issue: If buffers are effective at limiting landslide occurrence, then this result brings to bear a basic question about relative buffer effectiveness by landform. Post-Mortem results do show fewer landslides in buffered bedrock hollows than in buffered inner gorges, but the reasons for this difference cannot be explained without additional work.

Proposed project: Directly linked to three other projects described above, this project would utilize the RIL distribution data through the multivariate logical regression analysis to detect differences between landform susceptibility in the context of buffer effectiveness.

Expected outcomes: May understand at least relative levels of buffer effectiveness between RIL and potentially between other landform types.

APPENDIX E: DATA SUMMARIZED BY BLOCK

The following are the block summaries used in the statistical analysis of treatments.

E.1 Hillslope data summarized by block and treatment

Row	Block	Treatment	Treat_ Abbr	Count	Area_ SqMi	Med-High_Slp-Stab	PctGT65	AreaWt-StandAge	AreaWt-MeanSlope	AreaWt-MedSlope	Area-WtHigh-Haz	AreaWt-MedHigh	InitSed-Vol_ft3	Deliver-Vol_ft3	Init-SedTPA	DeliverSEDT-PA
1	1	Full Buffer	FB 0-20	0	0.1493	1236	0	22.651	20.352	19.648	11.716	33.936	0	0	0	0
2	1	Mature	M 41+	12	2.1143	24946	1.856	67.263	27.428	25.935	20.107	48.470	437735	71671	20.22	3.31
3	1	No Buffer	NB 0-20	15	1.2572	17255	0.763	15.659	29.021	28.160	24.035	56.391	1065464	225188	82.76	17.49
4	1	Partial Buffer	PB 0-20	13	0.3131	4530	4.054	20.348	32.697	32.341	25.536	59.465	236020	244125	73.62	76.15
5	1	Submature	SM 21-40	0	0.4567	2803	1.313	26.506	18.485	15.836	7.655	25.189	0	0	0	0
6	2	Full Buffer	FB 0-20	7	0.3011	2449	0.374	5.289	20.751	18.291	10.813	33.368	627099	31389	203.37	10.18
7	2	Mature	M 41+	8	0.8261	9042	7.619	45.666	32.256	30.454	12.107	44.945	151764	75822	17.94	8.96
8	2	Partial Buffer	PB 0-20	49	2.0472	21610	3.445	9.979	27.911	25.723	13.263	43.374	916190	539405	43.70	25.73
9	2	Submature	SM 21-40	15	0.8172	8806	5.292	36.204	33.362	31.515	10.863	44.248	1791899	1963840	214.13	234.67
10	7	Full Buffer	FB 0-20	11	0.5827	5999	11.659	5.850	39.917	38.325	6.525	42.254	200236	183399	33.56	30.73
11	7	Mature	M 41+	26	1.4963	17541	16.655	47.775	46.057	46.400	10.456	48.131	1379472	2054407	90.03	134.08
12	7	No Buffer	NB 0-20	10	0.1998	2602	20.971	11.092	53.312	52.685	12.301	53.424	355950	1438125	173.93	702.74
13	7	Partial Buffer	PB 0-20	21	0.8007	9311	14.510	7.558	43.643	43.122	8.628	47.788	579897	485160	70.73	59.17
14	7	Submature	SM 21-40	29	1.1464	14325	20.471	35.036	48.209	48.443	11.894	51.298	2347536	3805276	199.98	324.15
15	28	Full Buffer	FB 0-20	2	0.1836	2080	0.571	11.630	29.848	29.483	20.132	46.711	67800	8592	36.07	4.57
16	28	Mature	M 41+	3	1.1899	13657	1.786	49.905	26.755	25.024	19.012	47.139	2688	840	0.22	0.07
17	28	No Buffer	NB 0-20	15	0.8682	13104	2.155	13.679	31.645	31.023	27.081	61.922	138030	531932	15.53	59.84
18	28	Partial Buffer	PB 0-20	13	0.4722	4811	0.605	9.977	24.637	23.335	16.127	41.893	268028	259297	55.43	53.63
19	28	Submature	SM 21-40	7	1.0453	14716	1.779	29.419	29.279	28.499	24.769	57.873	15684	7172	1.47	0.67
20	35	Full Buffer	FB 0-20	2	0.2252	2706	3.454	7.560	27.516	25.163	19.413	49.291	37840	36800	16.41	15.96
21	35	Mature	M 41+	1	0.0889	1060	13.297	44.465	34.497	32.301	13.415	48.686	6480	5830	7.12	6.40
22	35	Partial Buffer	PB 0-20	6	0.5147	5596	8.601	13.454	32.535	30.720	14.166	44.626	113172	235835	21.47	44.74
23	35	Submature	SM 21-40	65	3.6977	52419	24.494	34.770	47.017	47.417	16.147	58.242	20100018	16676721	530.84	440.43
24	36	Full Buffer	FB 0-20	0	0.2684	2313	0.086	12.634	20.906	19.767	11.770	35.386	0	0	0	0
25	36	Mature	M 41+	0	0.1584	1128	0.493	41.478	20.799	17.143	6.775	29.303	0	0	0	0
26	36	No Buffer	NB 0-20	4	0.2802	4855	43.257	17.119	60.953	60.951	22.548	71.088	97775	93304	34.08	32.52
27	36	Partial Buffer	PB 0-20	0	0.2156	3045	7.243	11.455	33.546	29.789	23.227	57.921	0	0	0	0
28	36	Submature	SM 21-40	10	3.7702	58625	19.904	31.884	43.754	42.994	21.657	63.871	72679	67038	1.88	1.74
29	47	Full Buffer	FB 0-20	0	0.4940	3857	0.602	16.496	25.513	24.875	6.638	32.134	0	0	0	0
30	47	Mature	M 41+	16	2.9320	18501	0.442	64.416	21.806	20.014	3.706	25.916	759699	467568	25.30	15.57
31	47	No Buffer	NB 0-20	3	0.6481	5534	0.351	16.268	22.490	21.114	11.404	35.091	9454200	901365	1424.61	135.82
32	47	Partial Buffer	PB 0-20	3	0.1024	1617	5.053	16.982	38.851	38.247	28.610	64.777	49810	35500	47.52	33.87
33	47	Submature	SM 21-40	0	0.3993	5155	2.131	35.288	29.956	30.177	18.991	53.090	0	0	0	0
34	61	Full Buffer	FB 0-20	0	0.5343	4320	3.964	5.962	31.759	30.321	5.247	33.190	0	0	0	0
35	61	Mature	M 41+	0	0.9569	10691	7.957	46.150	39.671	39.377	9.616	45.840	0	0	0	0
36	61	No Buffer	NB 0-20	0	0.4933	4559	5.221	15.299	29.687	28.376	9.433	37.968	0	0	0	0
37	61	Partial Buffer	PB 0-20	6	1.5268	15746	6.988	8.411	33.346	32.119	12.109	42.403	40623	18363	2.60	1.17
38	61	Submature	SM 21-40	0	0.3039	3584	9.396	33.324	39.222	38.306	8.461	48.230	0	0	0	0
39	65	Full Buffer	FB 0-20	6	0.3044	3705	15.613	11.852	43.846	43.306	10.026	50.031	74347	52233	23.85	16.76
40	65	Mature	M 41+	44	3.1286	46360	27.856	46.876	50.642	50.583	17.782	60.860	396792	555420	12.39	17.34
41	65	No Buffer	NB 0-20	2	0.1664	2254	17.603	9.911	45.507	44.086	13.586	55.744	15936	6045	9.35	3.55

Row	Block	Treatment	Treat_ Abbr	Count	Area_ SqMi	Med-High_Slp-Stab	PctGT65	AreaWt-StandAge	AreaWt-MeanSlope	AreaWt-MedSlope	Area-WtHigh-Haz	AreaWt-MedHigh	InitSed-Vol_ft3	Deliver-Vol_ft3	Init-SedTPA	Deliv-erSEDT-PA
42	65	Partial Buffer	PB 0-20	18	0.8508	10913	14.283	8.838	43.672	42.939	12.360	52.664	468166	473700	53.74	54.37
43	65	Submature	SM 21-40	0	0.2245	2730	17.186	36.810	46.579	47.000	10.590	50.021	0	0	0	0
44	71	Full Buffer	FB 0-20	2	0.3979	3144	1.526	9.664	20.652	19.600	10.409	32.427	1296	1104	0.32	0.27
45	71	Mature	M 41+	0	0.3826	2227	0	56.304	15.428	13.862	7.466	23.882	0	0	0	0
46	71	No Buffer	NB 0-20	18	1.9493	21648	0.725	14.741	22.856	21.858	16.874	45.619	53672	20267	2.69	1.02
47	71	Partial Buffer	PB 0-20	12	0.6283	8320	1.200	9.957	27.695	26.370	22.449	54.370	76318	40614	11.86	6.31
48	71	Submature	SM 21-40	16	0.5498	4720	1.124	27.900	19.354	17.836	13.324	35.294	44862	41155	7.97	7.31
49	72	Full Buffer	FB 0-20	4	0.5045	4224	0.815	8.785	24.329	23.194	8.387	34.346	428832	234589	83.00	45.41
50	72	Mature	M 41+	9	1.4063	19613	17.220	45.388	44.010	43.478	15.519	57.278	98866	90090	6.87	6.26
51	72	No Buffer	NB 0-20	1	0.4219	5120	6.884	7.927	34.081	30.889	14.337	49.910	22050	20250	5.10	4.69
52	72	Partial Buffer	PB 0-20	5	0.4218	5416	21.672	10.173	43.772	40.272	11.820	52.750	37926	36430	8.78	8.43
53	72	Submature	SM 21-40	18	0.9827	16207	36.715	32.730	55.986	56.294	19.686	67.778	99923	282578	9.93	28.08
54	73	Full Buffer	FB 0-20	7	0.2297	2131	2.303	7.405	27.828	26.781	6.110	38.082	371308	283117	157.87	120.37
55	73	Mature	M 41+	11	0.9645	8752	4.590	49.739	34.685	34.427	6.121	37.302	640246	3449880	64.83	349.31
56	73	No Buffer	NB 0-20	17	0.8895	9254	2.325	10.114	31.274	30.656	10.549	42.731	340915	307635	37.43	33.77
57	73	Partial Buffer	PB 0-20	33	1.4086	11632	1.490	7.648	30.112	29.255	4.122	33.973	4245607	2655280	294.34	184.08
58	73	Submature	SM 21-40	0	0.3284	1030	0.459	36.814	12.839	11.949	2.154	12.840	0	0	0	0
59	78	Full Buffer	FB 0-20	1	0.5263	3252	1.371	10.722	22.374	21.599	2.819	25.443	1728	1512	0.32	0.28
60	78	Mature	M 41+	4	0.5186	8236	9.350	42.306	37.790	35.455	26.391	65.296	39819	17260	7.50	3.25
61	78	No Buffer	NB 0-20	6	1.7560	11767	0.721	18.404	23.433	21.357	4.705	27.536	97129	10615	5.40	0.59
62	78	Submature	SM 21-40	2	1.0399	13291	9.576	29.988	34.630	33.382	18.712	52.476	121860	99217	11.44	9.32
63	80	Full Buffer	FB 0-20	12	0.4878	4638	1.140	10.196	22.841	21.790	11.636	39.031	33366	27528	6.68	5.51
64	80	Mature	M 41+	8	0.4187	4983	1.626	49.710	25.168	23.449	18.875	48.855	52065	29010	12.14	6.77
65	80	No Buffer	NB 0-20	14	3.4734	33497	3.870	16.322	27.919	25.658	9.651	39.622	1090507	1010442	30.66	28.41
66	80	Partial Buffer	PB 0-20	2	0.1722	2035	6.468	8.122	34.577	32.826	14.308	48.630	12150	5580	6.89	3.17
67	82	Full Buffer	FB 0-20	8	0.3626	2034	1.122	12.317	20.715	18.905	0.901	23.043	102240	97505	27.54	26.26
68	82	Mature	M 41+	14	0.8768	7666	3.178	52.256	32.670	31.477	2.391	35.908	145968	147140	16.26	16.39
69	82	No Buffer	NB 0-20	16	0.7303	5722	4.610	11.851	31.286	30.051	2.799	32.224	1357593	198174	181.55	26.50
70	82	Partial Buffer	PB 0-20	21	1.5944	12259	1.512	7.837	28.557	27.006	1.759	31.577	3114592	2202932	190.77	134.93
71	82	Submature	SM 21-40	5	0.3944	3102	2.682	24.170	29.114	27.389	1.600	32.285	6663	6665	1.65	1.65
72	83	Full Buffer	FB 0-20	1	1.0440	5649	0.703	16.992	20.433	20.125	0.839	22.215	2970	2376	0.28	0.22
73	83	Mature	M 41+	7	2.1445	19304	2.604	75.007	31.453	30.612	2.843	36.977	257748	278062	11.74	12.66
74	83	No Buffer	NB 0-20	5	0.5206	4729	1.542	13.993	30.048	28.551	1.293	37.334	356454	245787	66.86	46.10
75	83	Partial Buffer	PB 0-20	0	0.1733	1389	0.313	18.000	30.538	32.000	0.380	32.969	0	0	0	0
76	83	Submature	SM 21-40	3	0.2293	1208	0.051	31.232	19.395	18.094	1.313	21.653	10452	10452	4.45	4.45
77	101	Full Buffer	FB 0-20	1	0.6592	5942	4.305	9.604	27.769	25.654	7.792	37.057	420000	420000	62.22	62.22
78	101	Mature	M 41+	32	1.7206	20608	14.729	51.864	44.197	43.918	9.072	49.196	16620361	13286463	943.34	754.12
79	101	No Buffer	NB 0-20	21	0.7754	8147	11.710	10.438	40.983	40.354	6.929	43.133	1100873	721574	138.64	90.88
80	101	Partial Buffer	PB 0-20	27	1.0533	12658	15.538	10.537	44.090	43.094	8.638	49.383	1099606	1079517	101.95	100.08
81	101	Submature	SM 21-40	0	0.1189	1451	13.214	36.792	44.727	44.618	8.402	50.174	0	0	0	0
82	105	Full Buffer	FB 0-20	0	0.2928	2893	2.934	15.567	32.293	30.872	10.182	40.601	0	0	0	0
83	105	Mature	M 41+	1	1.2679	12994	3.303	58.037	33.109	32.208	8.825	42.107	648	200	0.05	0.02
84	105	No Buffer	NB 0-20	1	0.3815	4820	1.842	12.420	33.824	33.271	18.265	51.763	450000	20000	115.18	5.12
85	105	Partial Buffer	PB 0-20	1	0.1796	2575	5.546	12.228	38.836	37.790	20.170	58.870	18480	12012	10.05	6.53
86	105	Submature	SM 21-40	1	1.5688	19392	3.262	24.440	28.916	26.506	21.246	50.823	2520	2016	0.16	0.13
87	109	Full Buffer	FB 0-20	0	0.1476	1822	19.621	9.684	42.127	37.258	10.345	50.868	0	0	0	0
88	109	Mature	M 41+	2	0.1436	2368	33.843	56.682	50.127	48.728	20.503	67.805	6125	1225	4.16	0.83

Row	Block	Treatment	Treat_ Abbr	Count	Area_ SqMi	Med-High_Slp-Stab	PctGT65	AreaWt-StandAge	AreaWt-MeanSlope	AreaWt-MedSlope	Area-WtHigh-Haz	AreaWt-MedHigh	InitSed-Vol_ft3	Deliver-Vol_ft3	Init-SedTPA	Deliv-erSEDT-PA
89	109	No Buffer	NB 0-20	3	0.0801	1991	67.008	14.626	72.385	75.742	37.259	101.897	45960	102000	56.02	124.34
90	109	Partial Buffer	PB 0-20	11	0.2249	3570	33.565	13.866	54.725	55.696	19.308	65.402	678729	819666	294.69	355.89
91	109	Submature	SM 21-40	78	3.8306	72666	48.207	30.844	61.857	62.776	24.776	77.924	4728949	7687194	120.56	195.97
92	116	Full Buffer	FB 0-20	4	1.7273	16691	2.995	16.255	28.730	27.180	11.526	39.705	17885	34765	1.01	1.97
93	116	Mature	M 41+	3	0.8831	12015	10.646	55.839	35.263	32.205	20.987	55.996	12798	12598	1.42	1.39
94	116	No Buffer	NB 0-20	2	0.3239	3012	0.438	14.819	25.282	24.759	13.325	38.151	25920	20000	7.82	6.03
95	116	Partial Buffer	PB 0-20	2	0.7621	6878	3.073	18.267	32.351	32.189	9.274	37.035	148800	14916	19.07	1.91
96	116	Submature	SM 21-40	3	0.3660	2173	0	22.767	20.020	18.827	2.445	24.366	3330	3013	0.89	0.80
97	123	Full Buffer	FB 0-20	1	1.4830	9299	0.315	8.389	17.922	16.459	8.317	25.791	364900	291920	24.03	19.22
98	123	Mature	M 41+	2	0.6672	4306	0.603	49.748	17.611	16.140	7.683	26.501	21426	19093	3.14	2.79
99	123	No Buffer	NB 0-20	2	0.8512	6496	0.617	14.665	21.426	20.027	8.898	31.387	20268	33893	2.33	3.89
100	123	Partial Buffer	PB 0-20	1	0.2589	1960	0	9.127	18.312	16.992	10.095	31.244	18750	8000	7.07	3.02
101	123	Submature	SM 21-40	4	0.8651	3970	0.640	28.270	16.000	13.794	4.653	18.815	46221	52800	5.22	5.96
102	154	Full Buffer	FB 0-20	3	1.3884	8284	0.476	12.998	23.471	22.713	0.344	24.556	5510	5350	0.39	0.38
103	154	Mature	M 41+	1	0.7185	5438	2.240	63.749	29.195	27.776	1.529	31.030	720	720	0.10	0.10
104	154	No Buffer	NB 0-20	5	0.8386	5630	3.189	16.924	29.402	27.493	2.064	27.587	36260	24694	4.22	2.88
105	154	Partial Buffer	PB 0-20	0	0.3210	2310	3.408	16.158	29.388	27.073	2.005	29.483	0	0	0	0
106	154	Submature	SM 21-40	2	0.2731	2000	4.293	26.754	29.016	24.321	2.848	30.121	8496	7963	3.04	2.85

E.2 Road data summarized by block and treatment

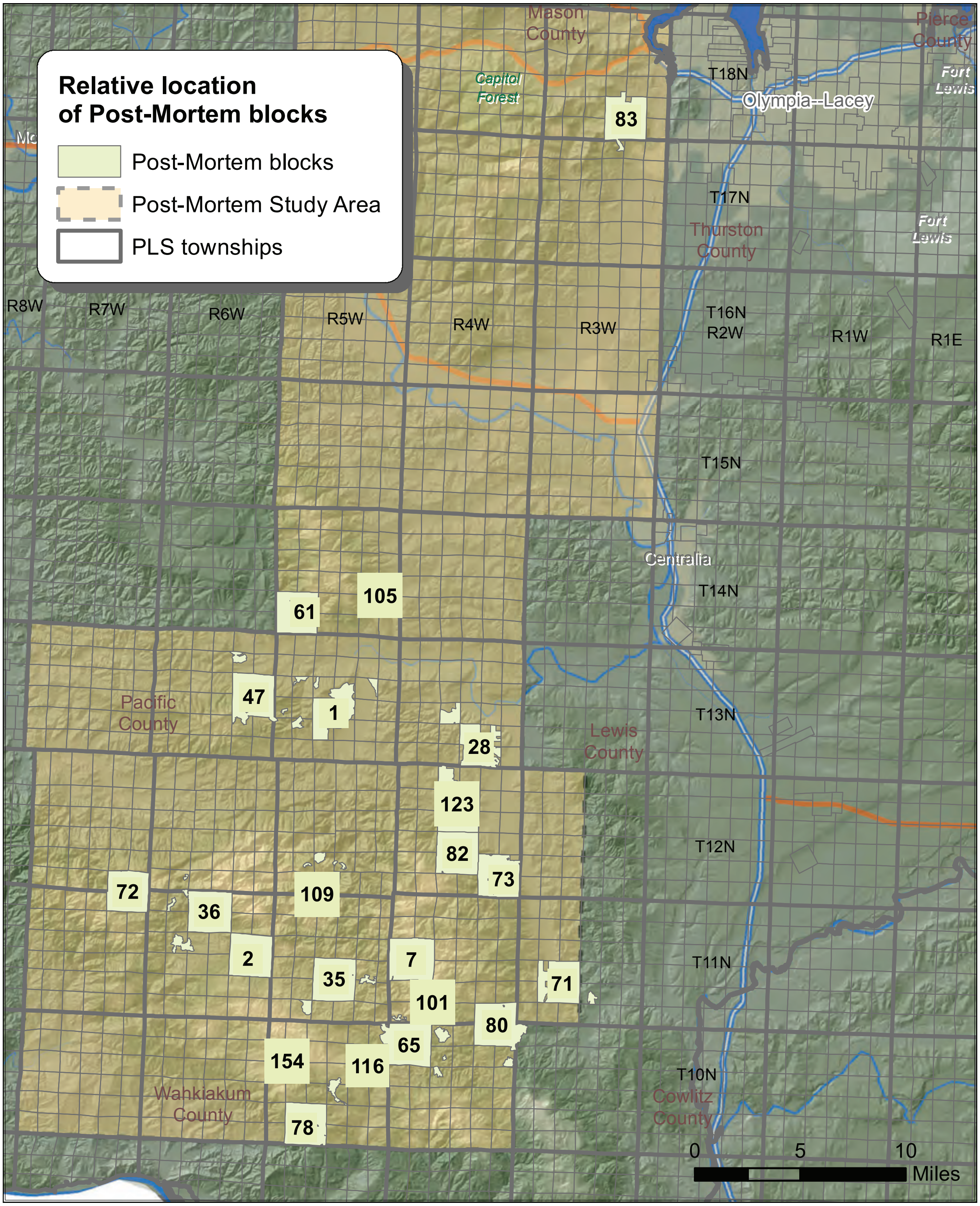
Row	Block	Treatment	Treat_Abbr	Road_length_mi	Count	Density_mi	Density_SqMi	InitVol_ft3	DeliverVol_ft3	InitSed_TPA	DelSed_TPA
1	1	Mitigated	Mit	2.69	0	0	0	0	0	0	0
2	1	Standard	Std	9.42	0	0	0	0	0	0	0
3	1	Substandard	Sub	2.35	0	0	0	0	0	0	0
4	2	Abandoned	Abd	0.16	0	0	0	0	0	0	0
5	2	Mitigated	Mit	1.65	0	0	0	0	0	0	0
6	2	Orphaned	Oph	5.83	7	1.20	105.57	210506	829623	310.03	1221.87
7	2	Standard	Std	16.42	1	0.06	5.36	49725	44000	26.03	23.03
8	2	Substandard	Sub	8.15	7	0.86	75.63	282522	214957	298.08	226.79
9	7	Abandoned	Abd	3.28	4	1.22	107.35	396375	167315	1038.88	438.52
10	7	Mitigated	Mit	2.33	5	2.15	188.96	282336	280136	1042.01	1033.89
11	7	Orphaned	Oph	4.32	7	1.62	142.48	1080786	1264936	2148.31	2514.34
12	7	Standard	Std	15.42	15	0.97	85.58	2917230	1691269	1625.31	942.28
13	7	Substandard	Sub	4.39	5	1.14	100.28	616482	1100267	1207.44	2154.98
14	28	Abandoned	Abd	0.35	0	0	0	0	0	0	0
15	28	Mitigated	Mit	0.93	0	0	0	0	0	0	0
16	28	Orphaned	Oph	0.18	0	0	0	0	0	0	0
17	28	Standard	Std	8.88	0	0	0	0	0	0	0
18	28	Substandard	Sub	6.09	0	0	0	0	0	0	0
19	35	Abandoned	Abd	3.28	5	1.52	134.15	339954	809571	890.73	2121.19
20	35	Mitigated	Mit	7.80	14	1.79	157.94	359020	597377	395.54	658.15
21	35	Orphaned	Oph	4.47	4	0.90	78.78	67705	269600	130.21	518.51
22	35	Standard	Std	11.36	16	1.41	123.95	1339595	1195412	1013.48	904.40
23	35	Substandard	Sub	7.64	16	2.09	184.29	824400	1183025	927.31	1330.70
24	36	Abandoned	Abd	1.22	0	0	0	0	0	0	0
25	36	Mitigated	Mit	2.08	1	0.48	42.27	1080	1080	4.46	4.46

Row	Block	Treatment	Treat_Abbr	Road_length_mi	Count	Density_mi	Density_SqMi	InitVol_ft3	DeliverVol_ft3	InitSed_TPA	DelSed_TPA
26	36	Orphaned	Oph	1.07	0	0	0	0	0	0	0
27	36	Standard	Std	22.40	2	0.09	7.86	66900	45750	25.67	17.55
28	36	Substandard	Sub	6.03	4	0.66	58.34	34094	45646	48.56	65.01
29	47	Abandoned	Abd	0.20	0	0	0	0	0	0	0
30	47	Mitigated	Mit	1.45	0	0	0	0	0	0	0
31	47	Orphaned	Oph	1.36	0	0	0	0	0	0	0
32	47	Standard	Std	10.07	1	0.10	8.74	400	400	0.34	0.34
33	47	Substandard	Sub	0.67	0	0	0	0	0	0	0
34	61	Abandoned	Abd	1.67	0	0	0	0	0	0	0
35	61	Mitigated	Mit	2.96	0	0	0	0	0	0	0
36	61	Orphaned	Oph	0.35	0	0	0	0	0	0	0
37	61	Standard	Std	20.26	0	0	0	0	0	0	0
38	61	Substandard	Sub	3.76	1	0.27	23.43	9792	7851	22.40	17.96
39	65	Abandoned	Abd	0.65	0	0	0	0	0	0	0
40	65	Mitigated	Mit	1.76	0	0	0	0	0	0	0
41	65	Orphaned	Oph	8.28	2	0.24	21.25	30259	29900	31.40	31.03
42	65	Standard	Std	3.71	0	0	0	0	0	0	0
43	65	Substandard	Sub	1.61	1	0.62	54.53	26040	23000	138.67	122.48
44	71	Abandoned	Abd	0.17	0	0	0	0	0	0	0
45	71	Mitigated	Mit	2.15	0	0	0	0	0	0	0
46	71	Orphaned	Oph	1.16	1	0.86	75.78	1728	1080	12.79	7.99
47	71	Standard	Std	15.51	0	0	0	0	0	0	0
48	71	Substandard	Sub	5.27	0	0	0	0	0	0	0
49	72	Mitigated	Mit	3.44	4	1.16	102.23	170210	131700	424.82	328.71
50	72	Orphaned	Oph	1.50	0	0	0	0	0	0	0
51	72	Standard	Std	16.83	9	0.53	47.06	172552	88542	88.11	45.21
52	72	Substandard	Sub	5.84	0	0	0	0	0	0	0
53	73	Abandoned	Abd	0.33	0	0	0	0	0	0	0
54	73	Mitigated	Mit	1.91	0	0	0	0	0	0	0
55	73	Orphaned	Oph	0.86	0	0	0	0	0	0	0
56	73	Standard	Std	20.49	19	0.93	81.61	350915	246063	147.19	103.21
57	73	Substandard	Sub	1.61	1	0.62	54.49	131820	129600	701.46	689.65
58	78	Abandoned	Abd	0.26	0	0	0	0	0	0	0
59	78	Mitigated	Mit	1.72	0	0	0	0	0	0	0
60	78	Orphaned	Oph	0.17	1	6.06	533.17	135000	135000	7029.11	7029.11
61	78	Standard	Std	15.65	0	0	0	0	0	0	0
62	78	Substandard	Sub	9.85	1	0.10	8.93	900	750	0.79	0.65
63	80	Abandoned	Abd	4.58	0	0	0	0	0	0	0
64	80	Mitigated	Mit	2.20	0	0	0	0	0	0	0
65	80	Standard	Std	16.46	0	0	0	0	0	0	0
66	80	Substandard	Sub	6.70	0	0	0	0	0	0	0
67	82	Abandoned	Abd	0.29	0	0	0	0	0	0	0
68	82	Mitigated	Mit	2.22	0	0	0	0	0	0	0
69	82	Orphaned	Oph	0.52	0	0	0	0	0	0	0
70	82	Standard	Std	23.66	6	0.25	22.32	203028	236292	73.75	85.83
71	82	Substandard	Sub	1.73	0	0	0	0	0	0	0
72	83	Abandoned	Abd	5.33	0	0	0	0	0	0	0
73	83	Mitigated	Mit	2.69	0	0	0	0	0	0	0
74	83	Standard	Std	6.86	2	0.29	25.67	18220	8792	22.84	11.02
75	83	Substandard	Sub	1.16	0	0	0	0	0	0	0

Row	Block	Treatment	Treat_Abbr	Road_length_mi	Count	Density_mi	Density_SqMi	InitVol_ft3	DeliverVol_ft3	InitSed_TPA	DelSed_TPA
76	101	Abandoned	Abd	0.31	0	0	0	0	0	0	0
77	101	Mitigated	Mit	5.01	0	0	0	0	0	0	0
78	101	Orphaned	Oph	1.10	0	0	0	0	0	0	0
79	101	Standard	Std	12.45	5	0.40	35.34	408096	422781	281.66	291.79
80	101	Substandard	Sub	1.88	0	0	0	0	0	0	0
81	105	Abandoned	Abd	1.91	0	0	0	0	0	0	0
82	105	Mitigated	Mit	1.68	0	0	0	0	0	0	0
83	105	Standard	Std	12.97	0	0	0	0	0	0	0
84	105	Substandard	Sub	2.55	0	0	0	0	0	0	0
85	109	Abandoned	Abd	12.08	11	0.91	80.12	1398275	1396294	994.63	993.22
86	109	Mitigated	Mit	3.47	3	0.86	76.03	3448750	4426250	8535.75	10955.09
87	109	Orphaned	Oph	0.28	0	0	0	0	0	0	0
88	109	Standard	Std	14.05	17	1.21	106.45	1722186	2405453	1053.07	1470.87
89	109	Substandard	Sub	6.26	3	0.48	42.17	41403	35053	56.84	48.12
90	116	Abandoned	Abd	0.35	0	0	0	0	0	0	0
91	116	Mitigated	Mit	1.24	0	0	0	0	0	0	0
92	116	Orphaned	Oph	2.39	2	0.84	73.76	18144	18144	65.35	65.35
93	116	Standard	Std	15.50	1	0.06	5.68	8400	8400	4.66	4.66
94	116	Substandard	Sub	1.49	0	0	0	0	0	0	0
95	123	Abandoned	Abd	2.33	0	0	0	0	0	0	0
96	123	Mitigated	Mit	2.05	0	0	0	0	0	0	0
97	123	Orphaned	Oph	1.15	0	0	0	0	0	0	0
98	123	Standard	Std	30.11	3	0.10	8.77	12000	12300	3.42	3.51
99	123	Substandard	Sub	2.38	0	0	0	0	0	0	0
100	154	Abandoned	Abd	2.50	0	0	0	0	0	0	0
101	154	Mitigated	Mit	2.39	1	0.42	36.89	162000	54000	583.55	194.52
102	154	Orphaned	Oph	0.89	0	0	0	0	0	0	0
103	154	Standard	Std	12.92	0	0	0	0	0	0	0
104	154	Substandard	Sub	3.13	0	0	0	0	0	0	0

APPENDIX F: BLOCK MAPS

This section includes a map for each block showing the relative location of all mapped landslides along with harvest treatment delineations.



Block 1

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

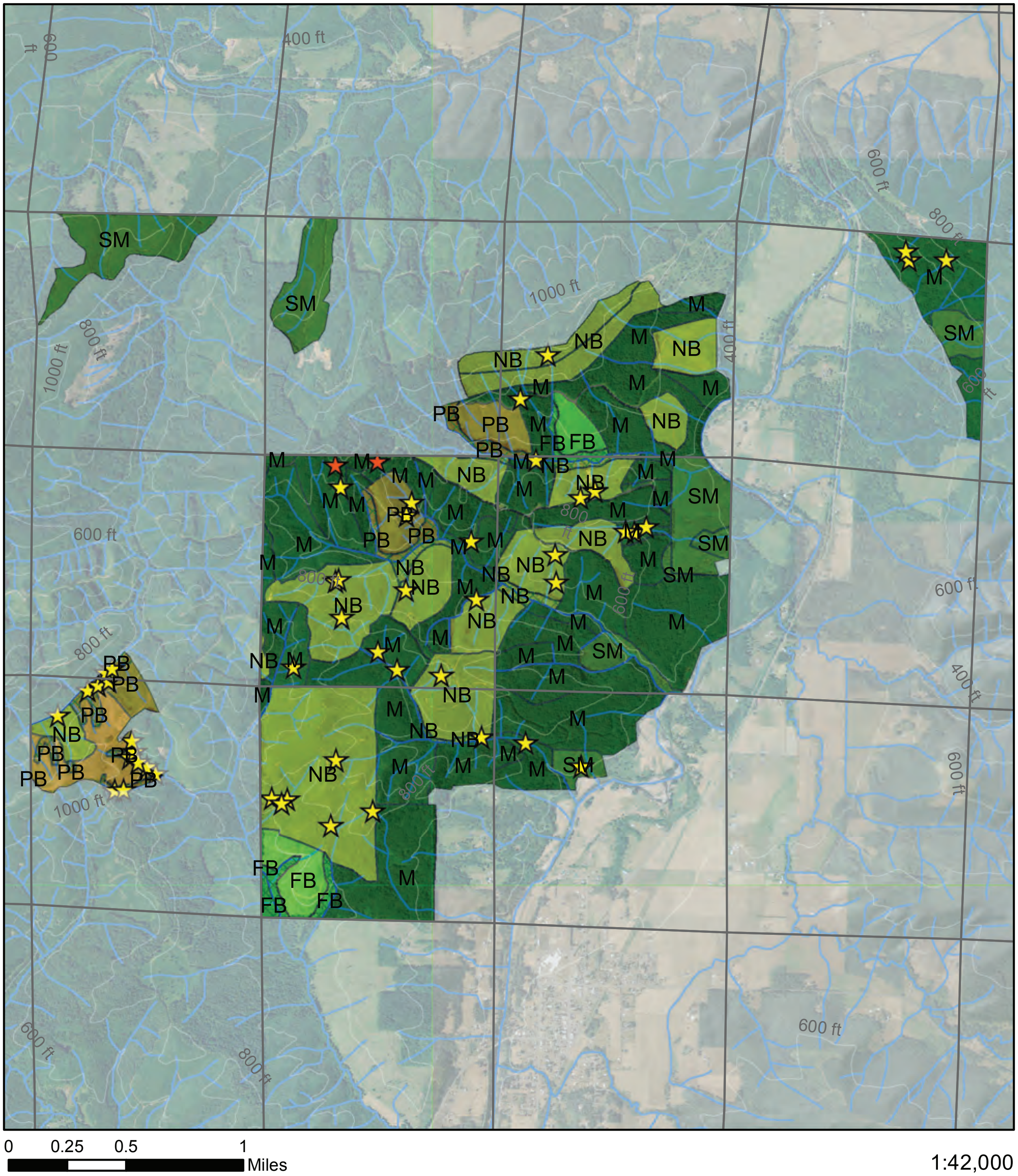
The core sections are 21, 22, 27 & 28 in T13N R5W

Landslide event location

- ★ Road
- ★ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



Block 2

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

The core sections are 13, 14, 23 & 24 in T11N R6W

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

🟨	No buffer (NB)
🟧	Partial buffer (PB)
🟩	Full buffer (FB)
🟩	Submature (SM)
🟩	Mature (M)

Block 7

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

The core sections are 21, 22, 27 & 28 in T11N R4W

Landslide event location

★ Road

☆ Hillslope

Harvest treatment:

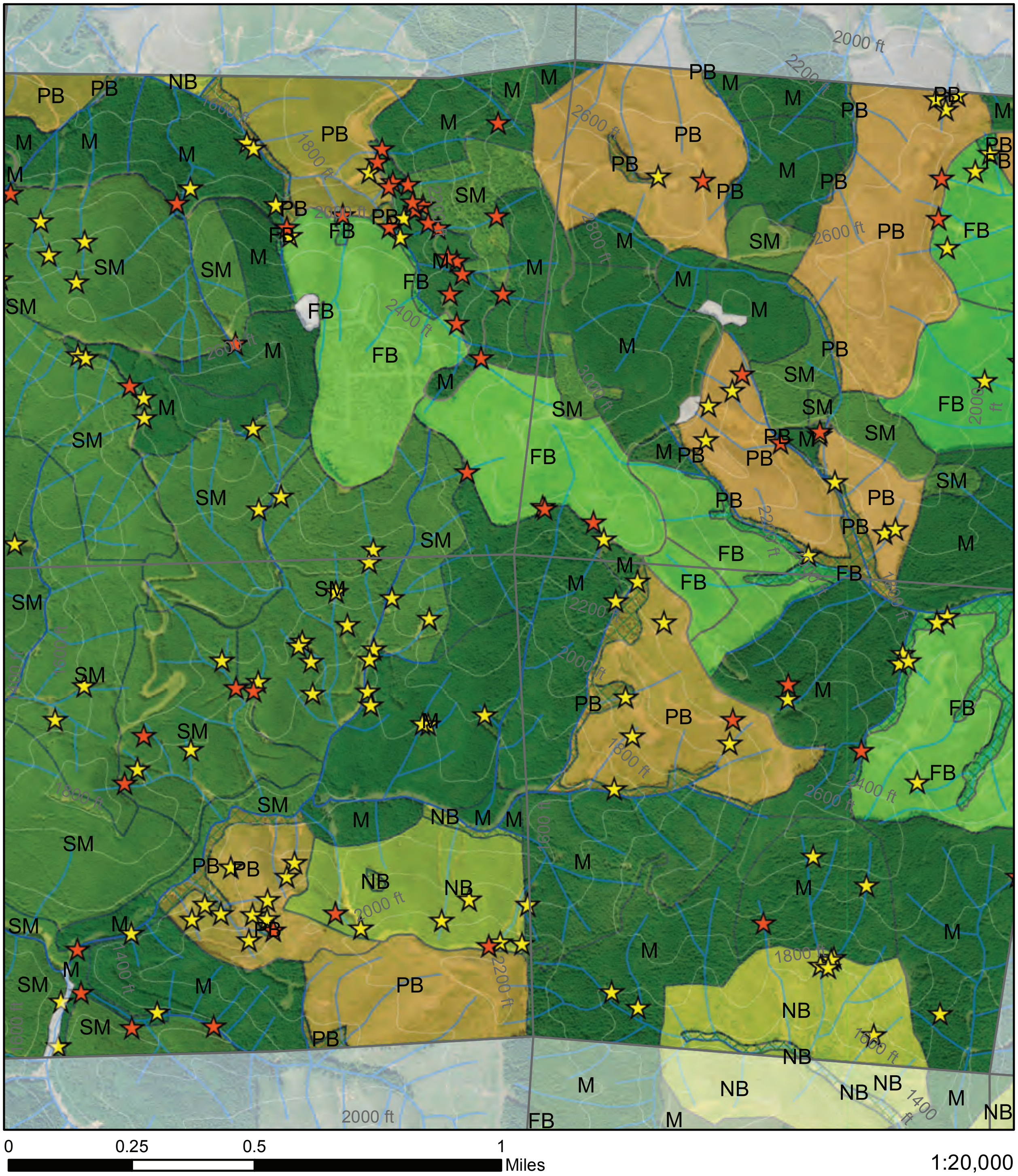
🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)



1:20,000

Block 28

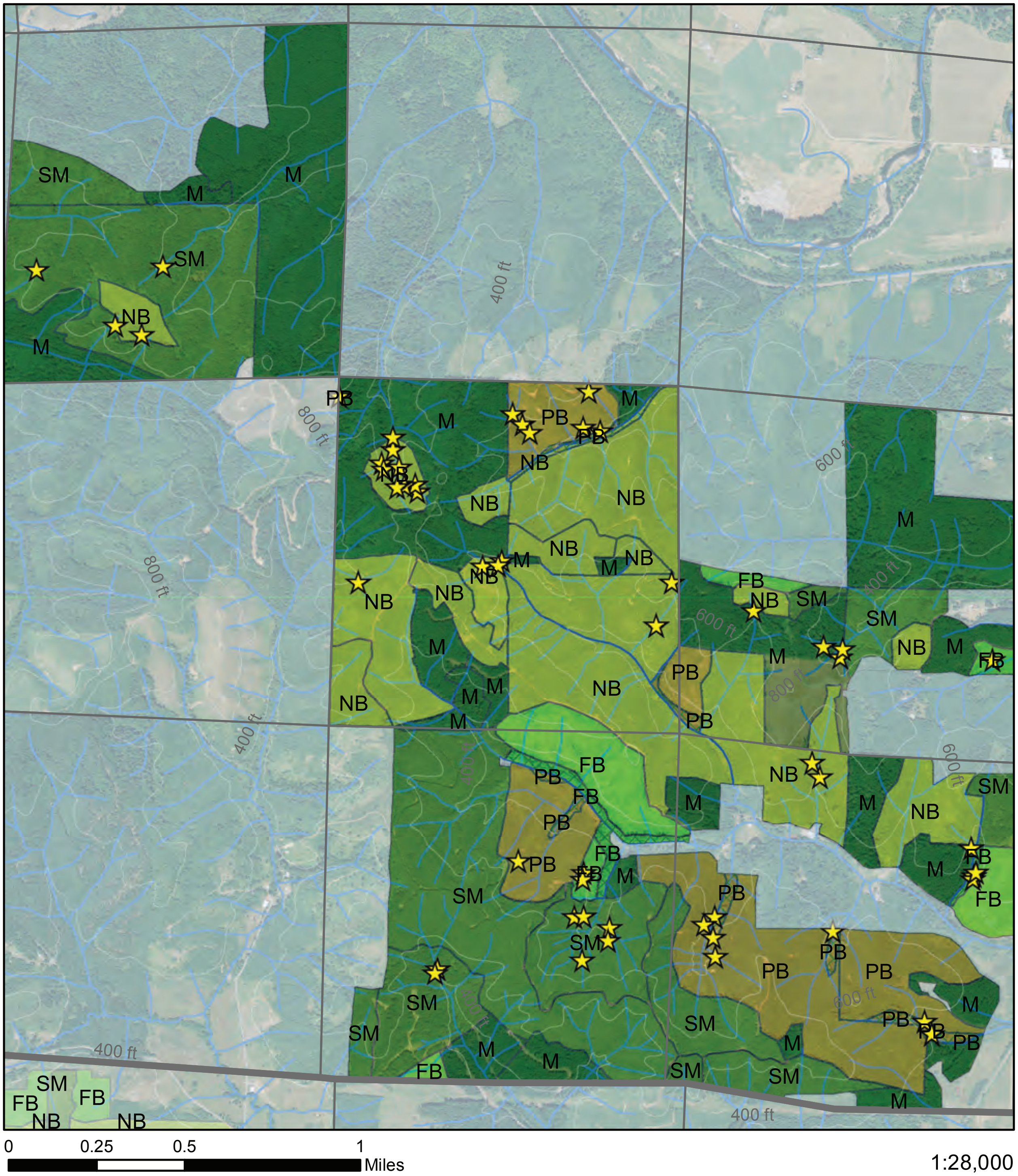
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



Block 35

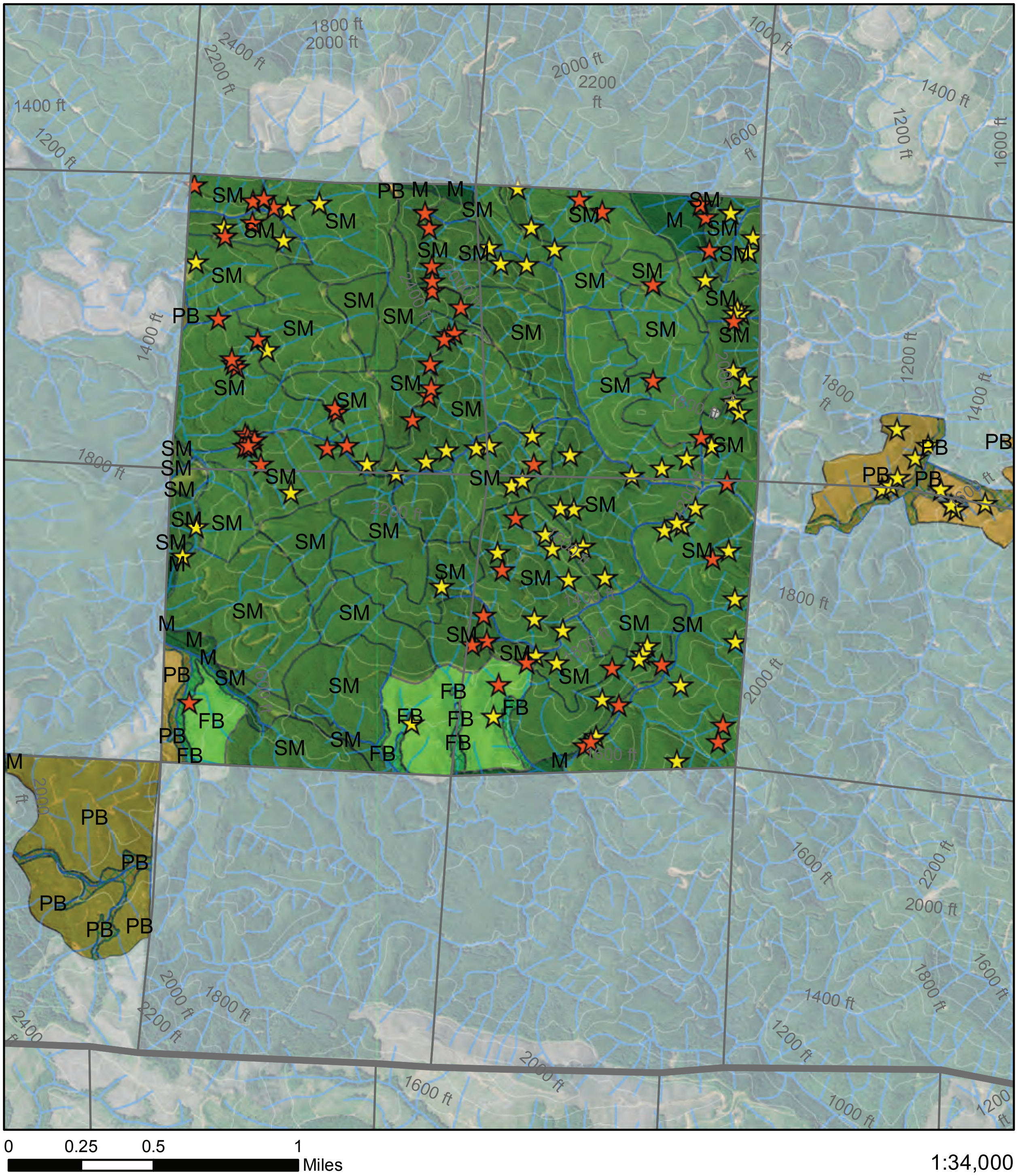
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



Block 36

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

★ Road

☆ Hillslope

Harvest treatment:

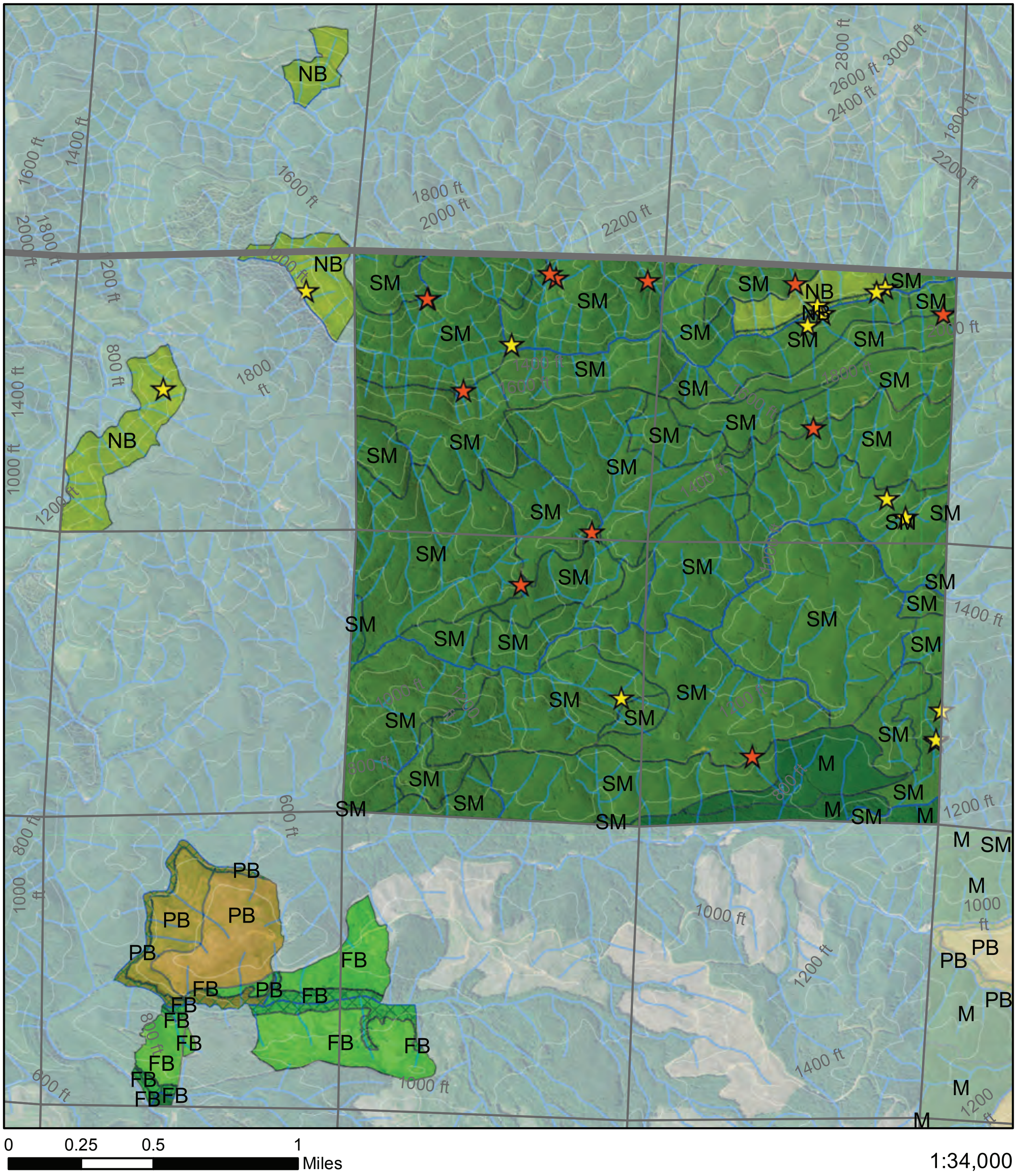
🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)



Block 47

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

★ Road

☆ Hillslope

Harvest treatment:

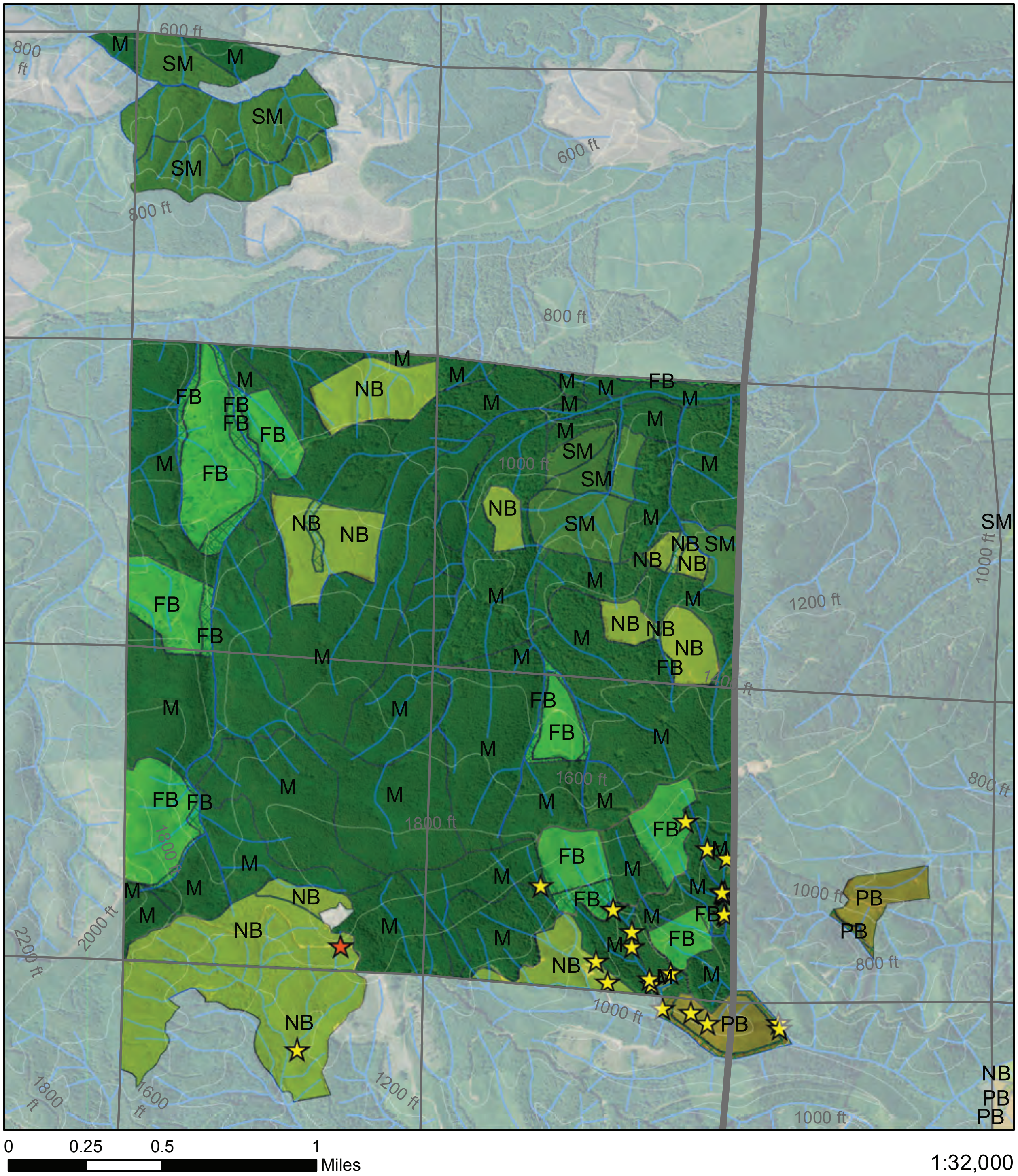
🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)



Block 61

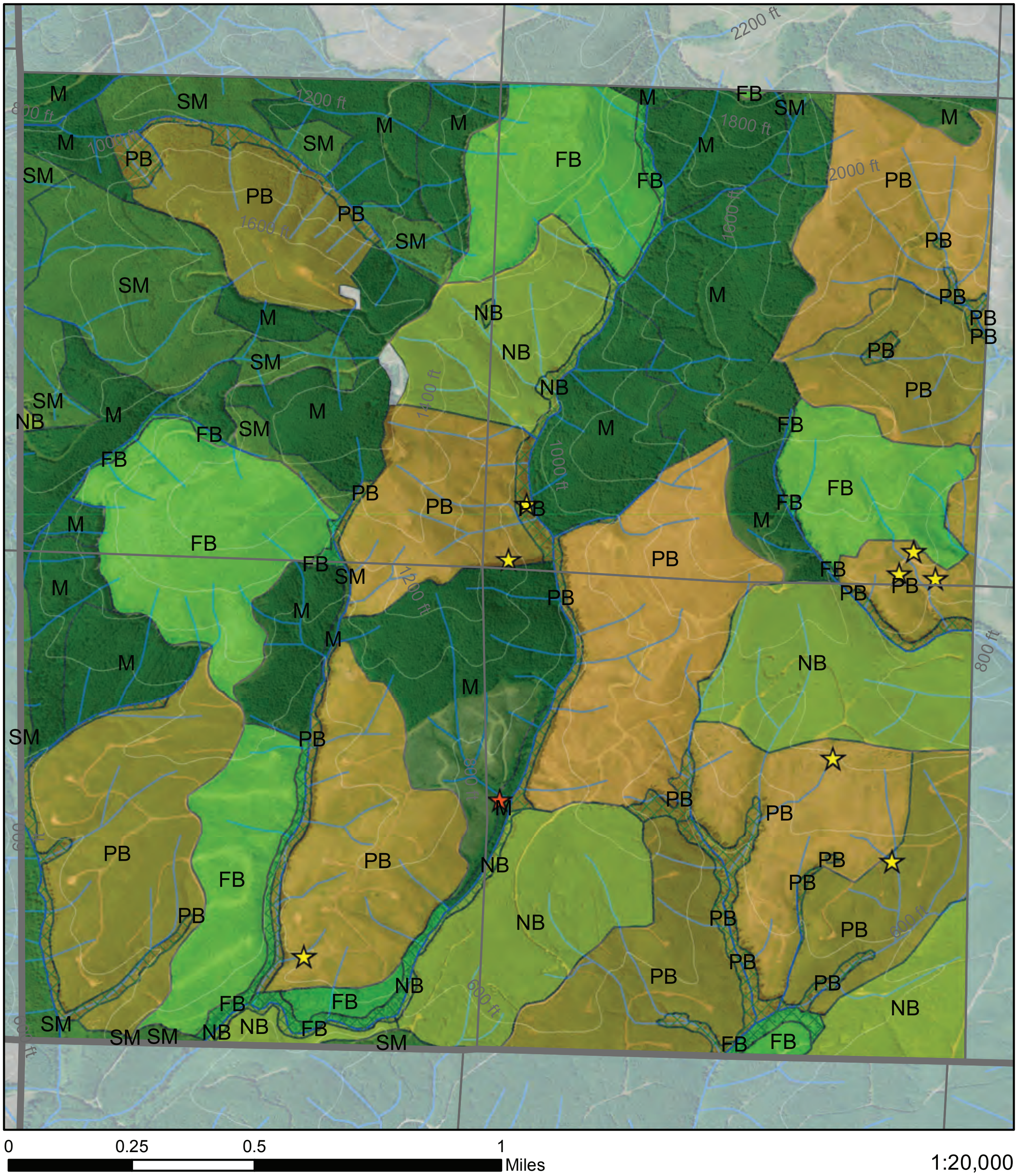
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



Block 65

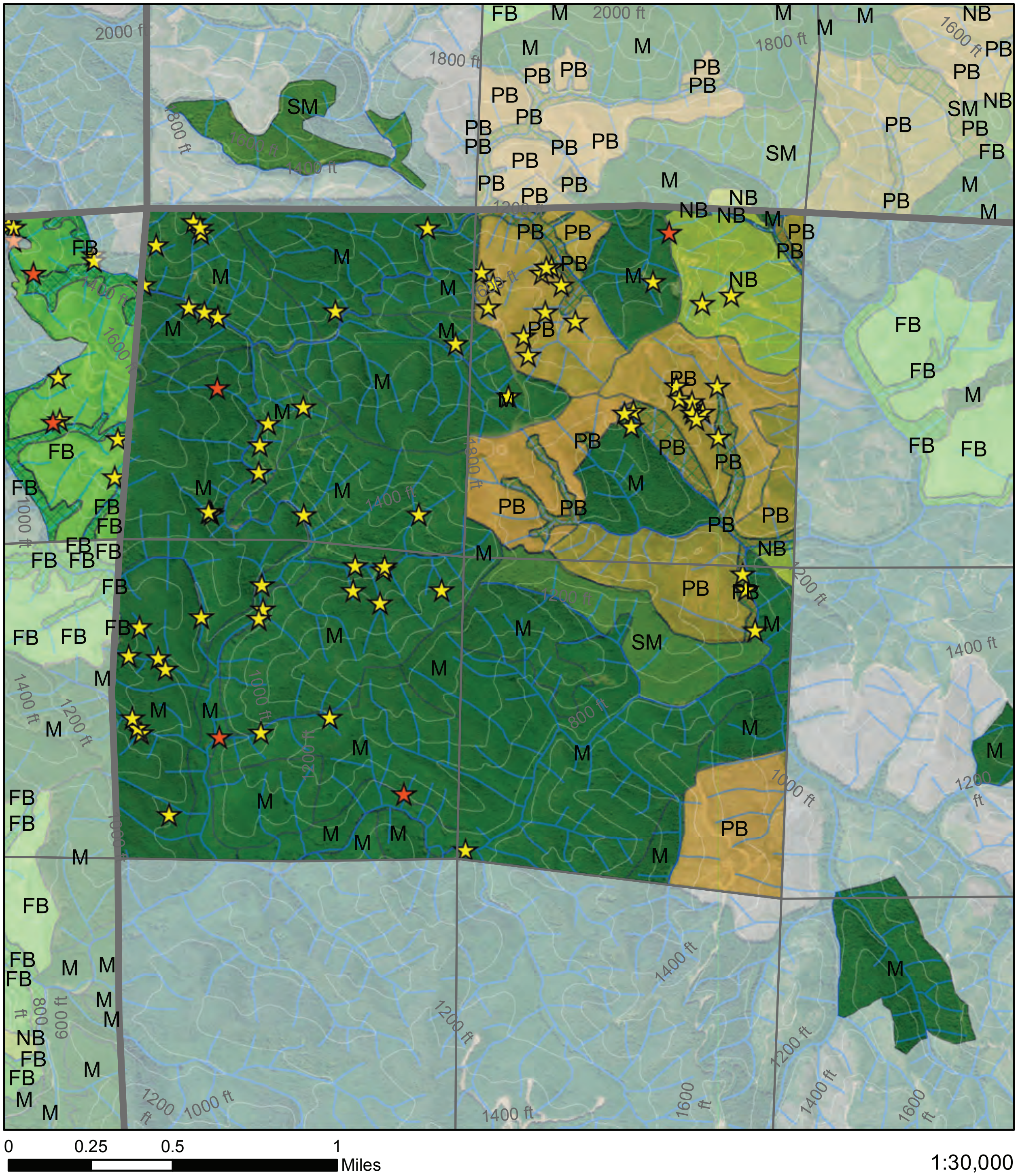
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



Block 71

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

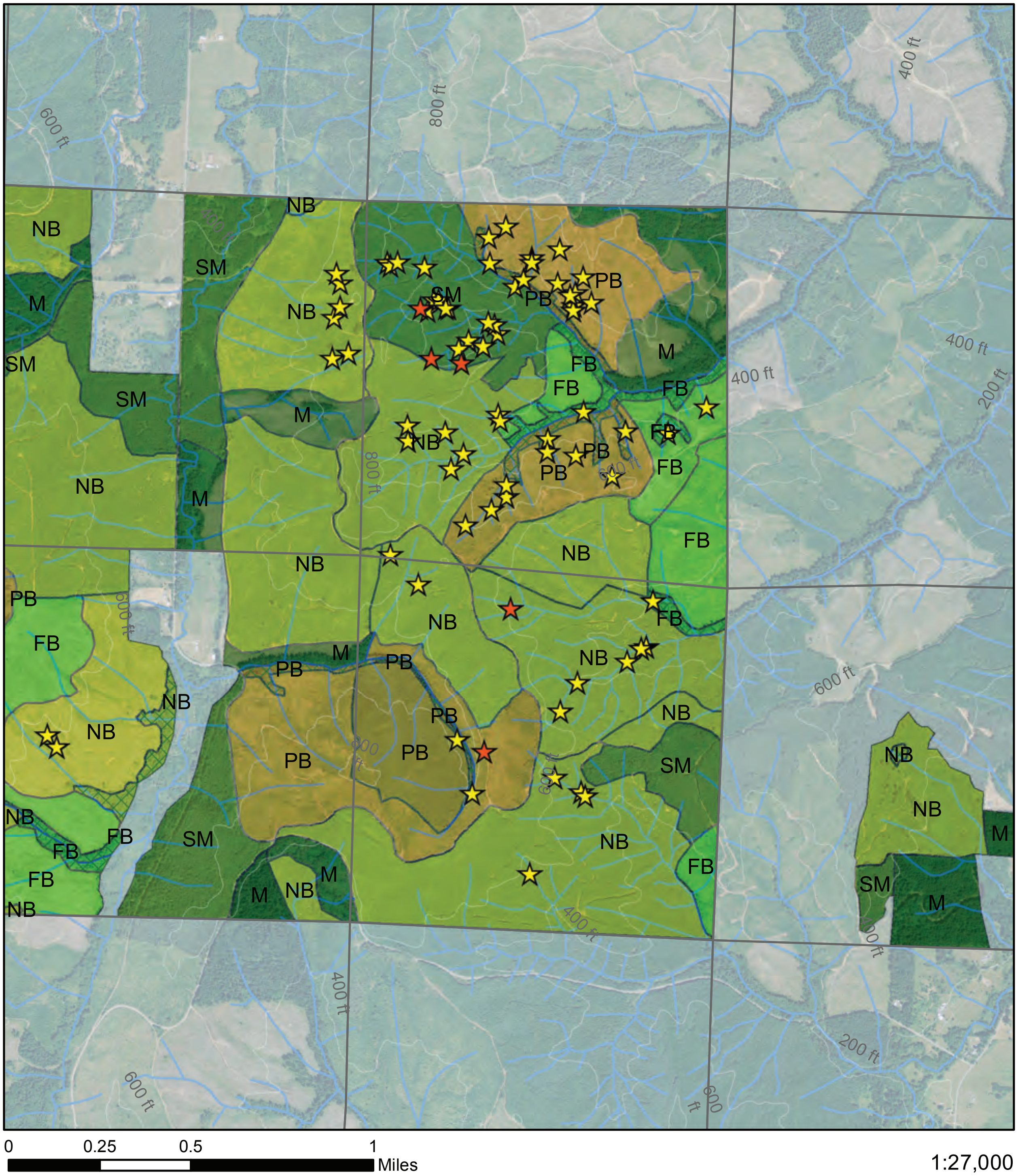
Landslide event location

★ Road

☆ Hillslope

Harvest treatment:

- No buffer (NB)
- Partial buffer (PB)
- Full buffer (FB)
- Submature (SM)
- Mature (M)



Block 72

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

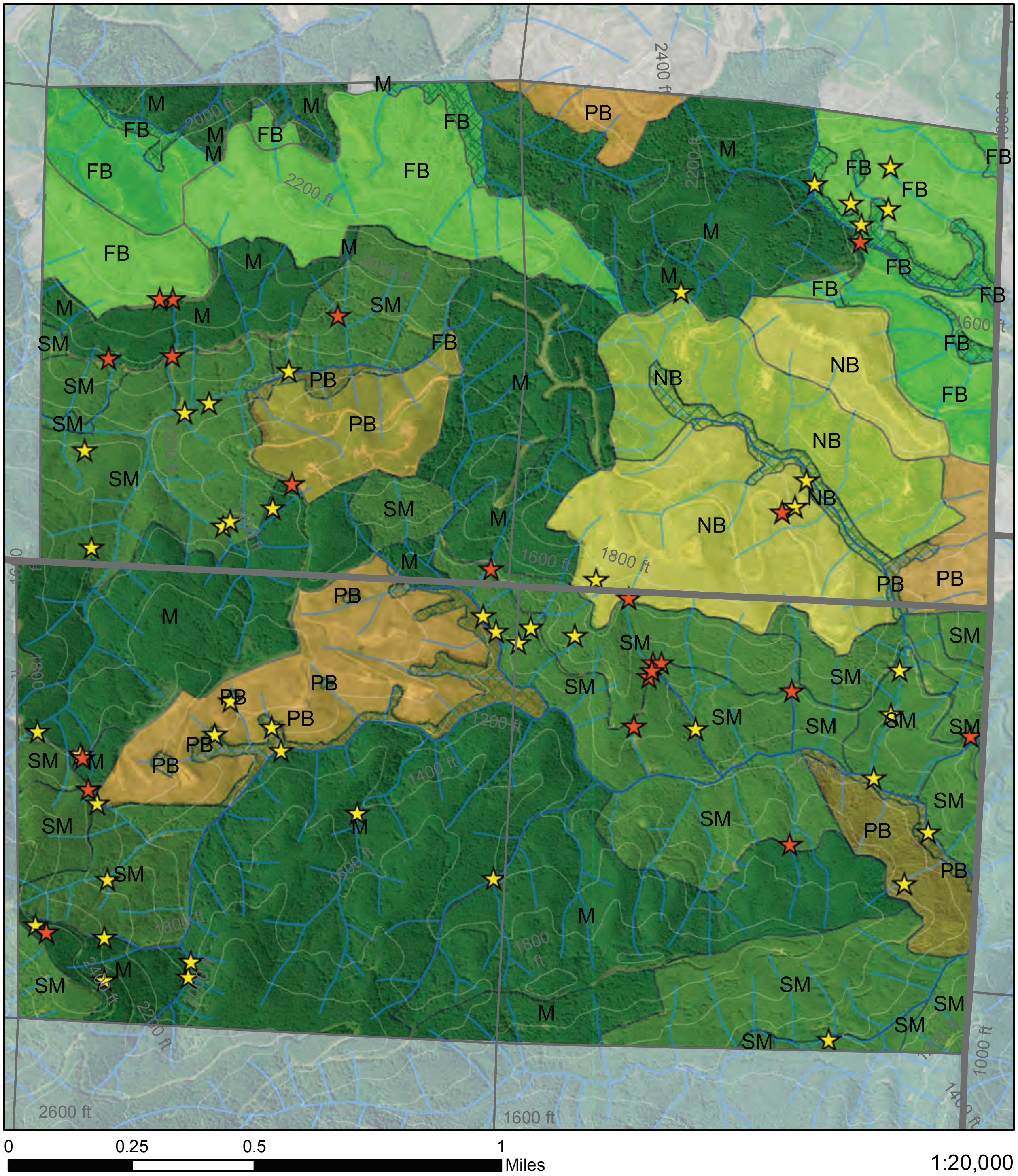
Landslide event location

★ Road

★ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



Block 73

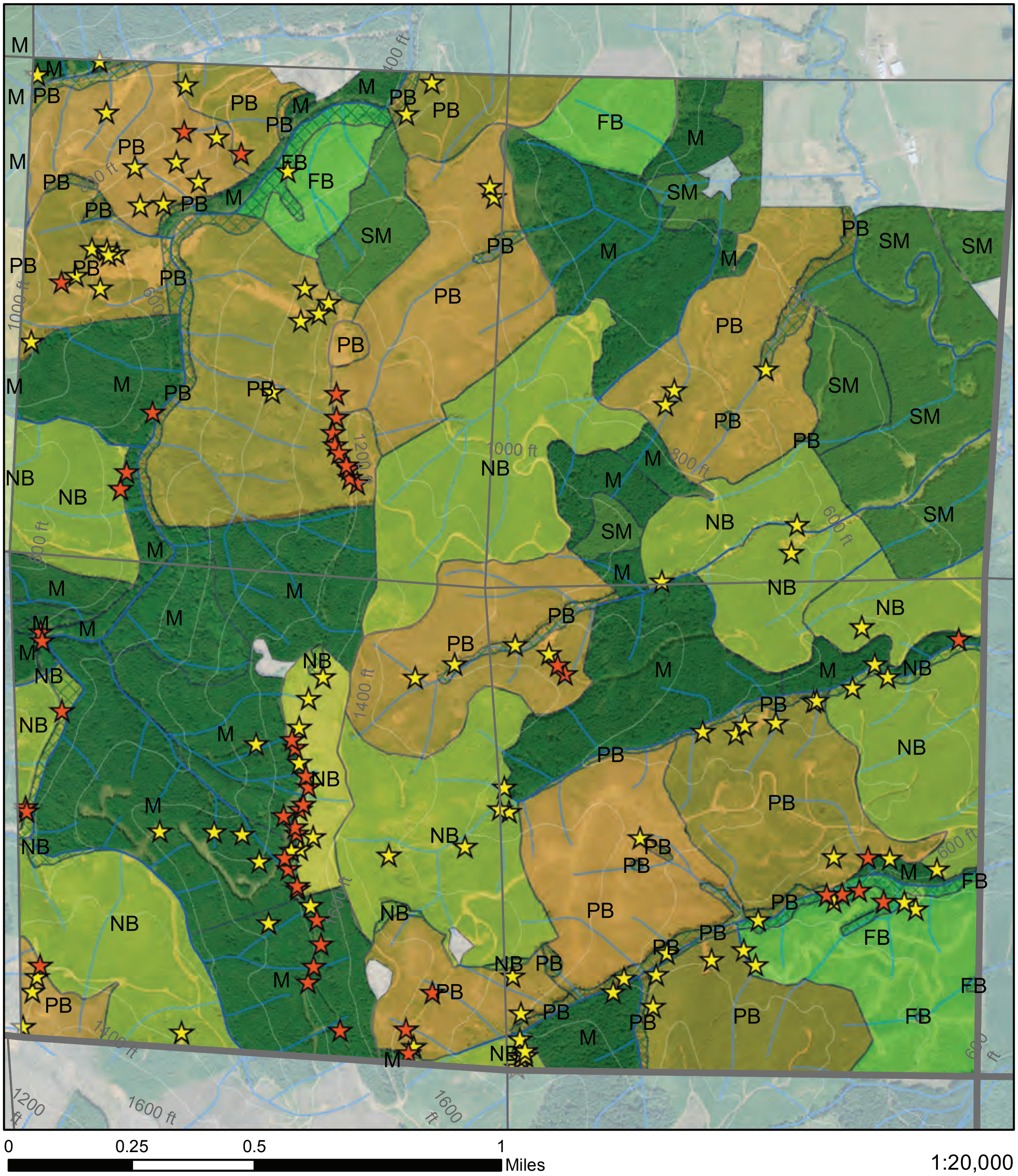
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- No buffer (NB)
- Partial buffer (PB)
- Full buffer (FB)
- Submature (SM)
- Mature (M)



Block 78

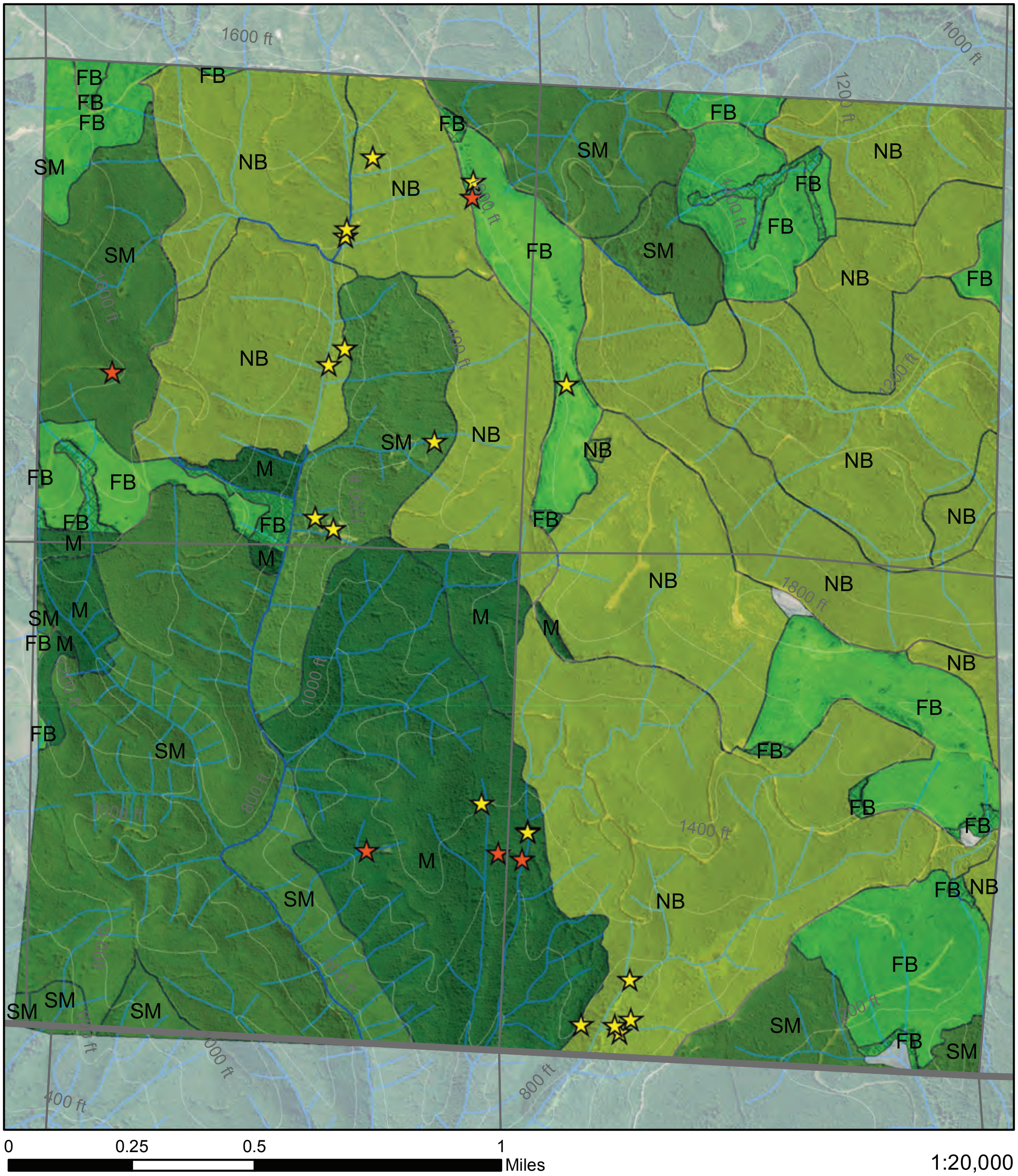
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



1:20,000

Block 80

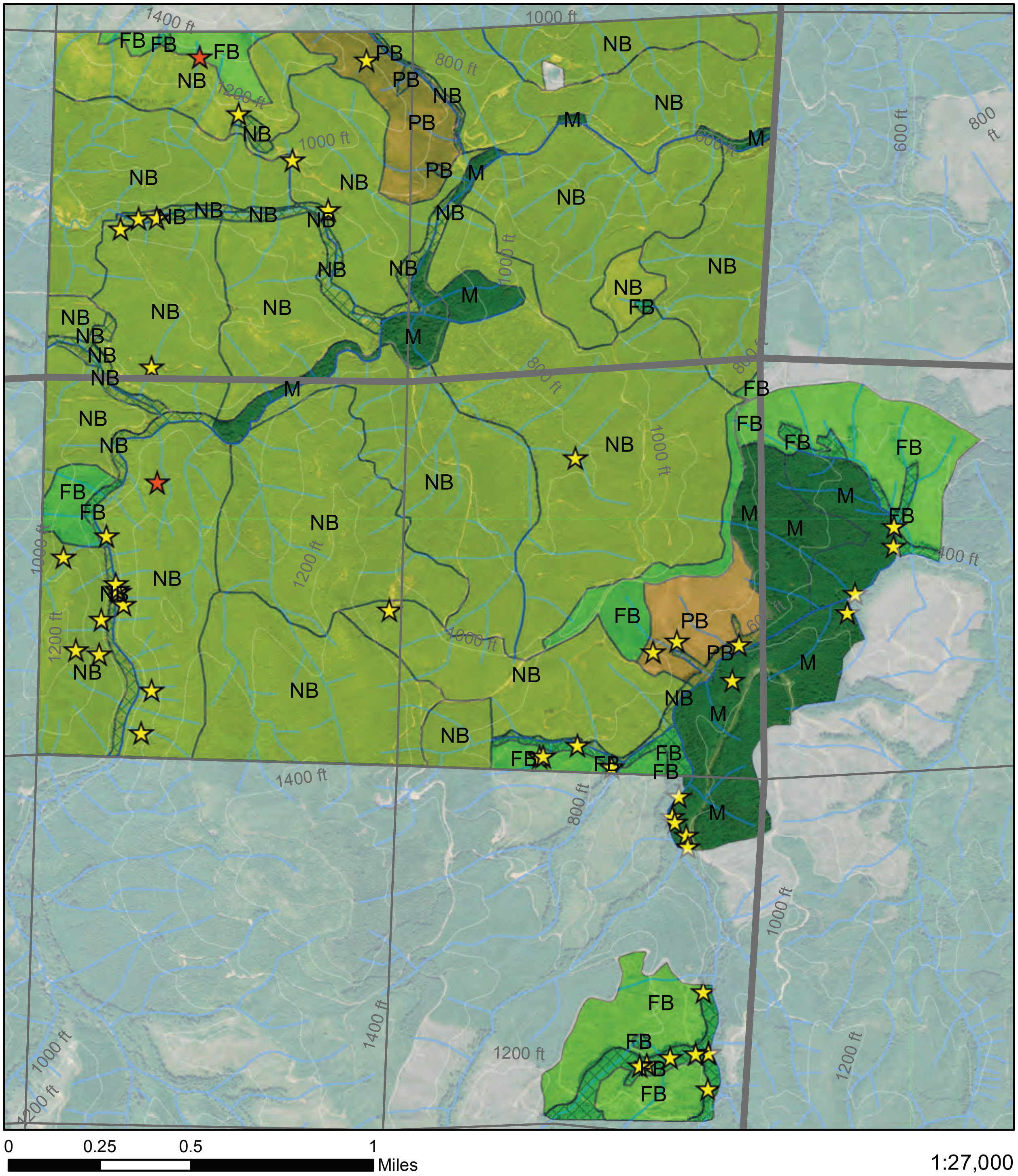
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



1:27,000

Block 82

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

★ Road

★ Hillslope

Harvest treatment:

🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)

Block 83

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

★ Road

☆ Hillslope

Harvest treatment:

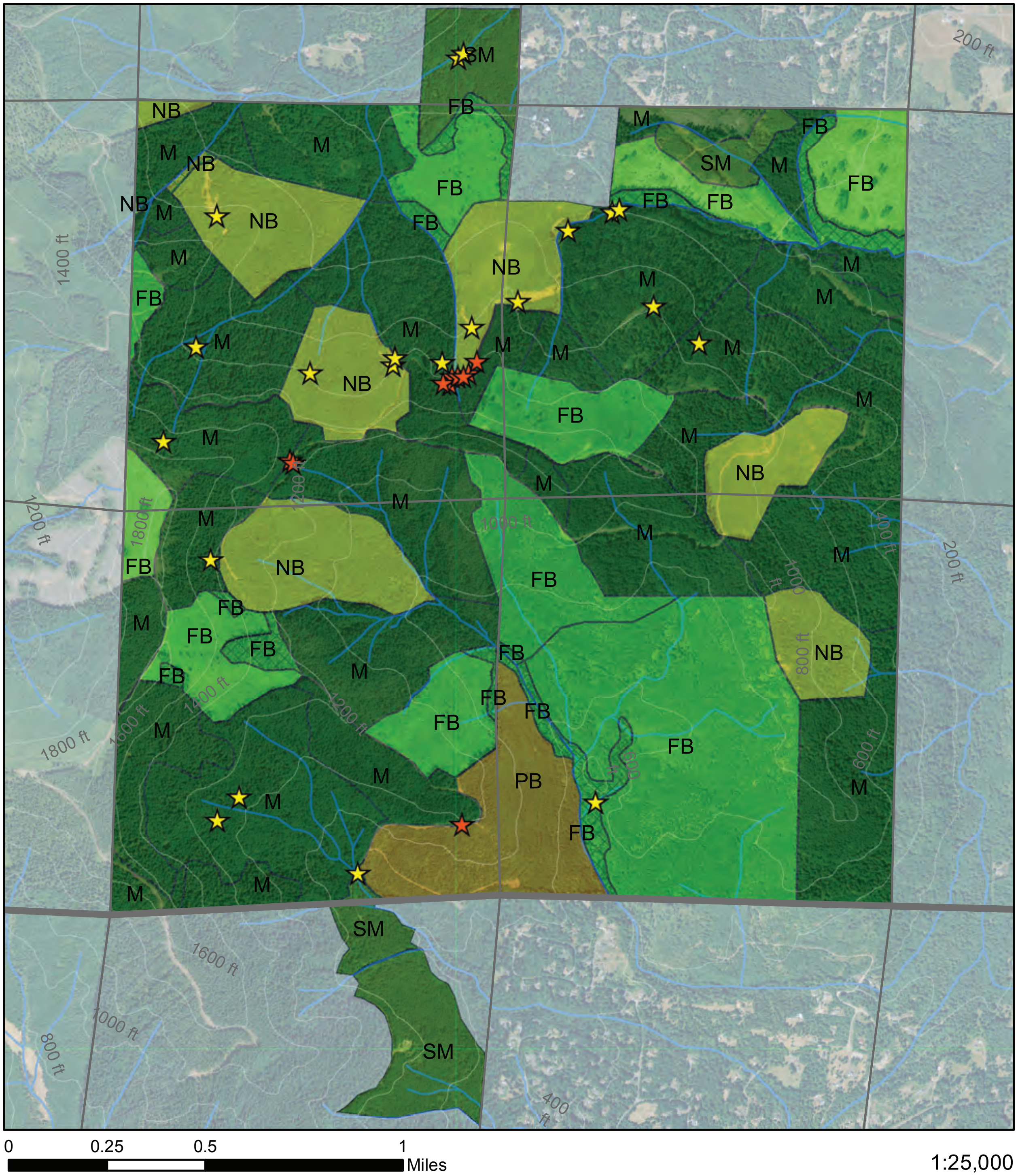
🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)



Block 101

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

★ Road

★ Hillslope

Harvest treatment:

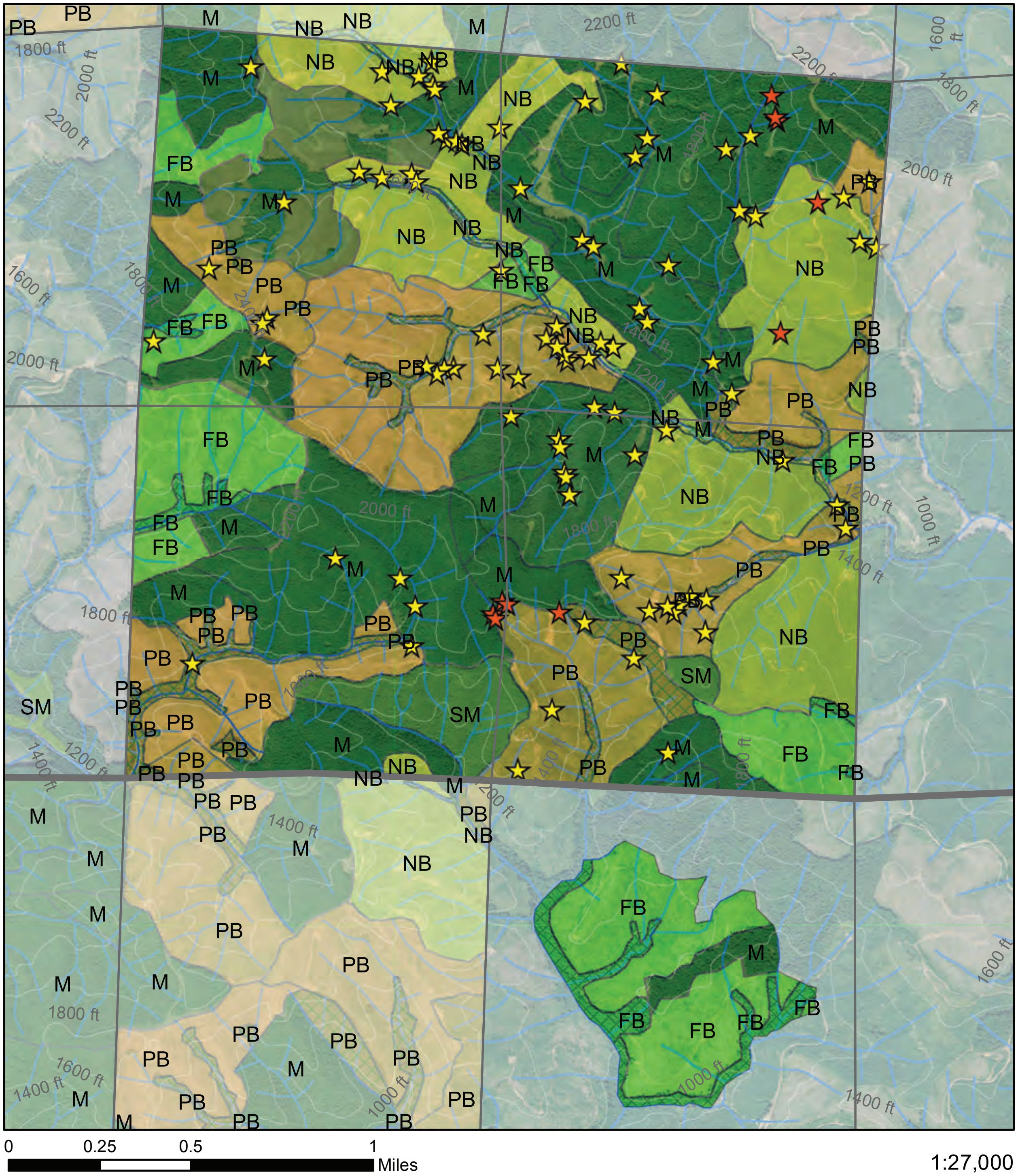
🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)



Block 105

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

★ Road

★ Hillslope

Harvest treatment:

🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)

Block 109

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)

1:28,000

Block 116

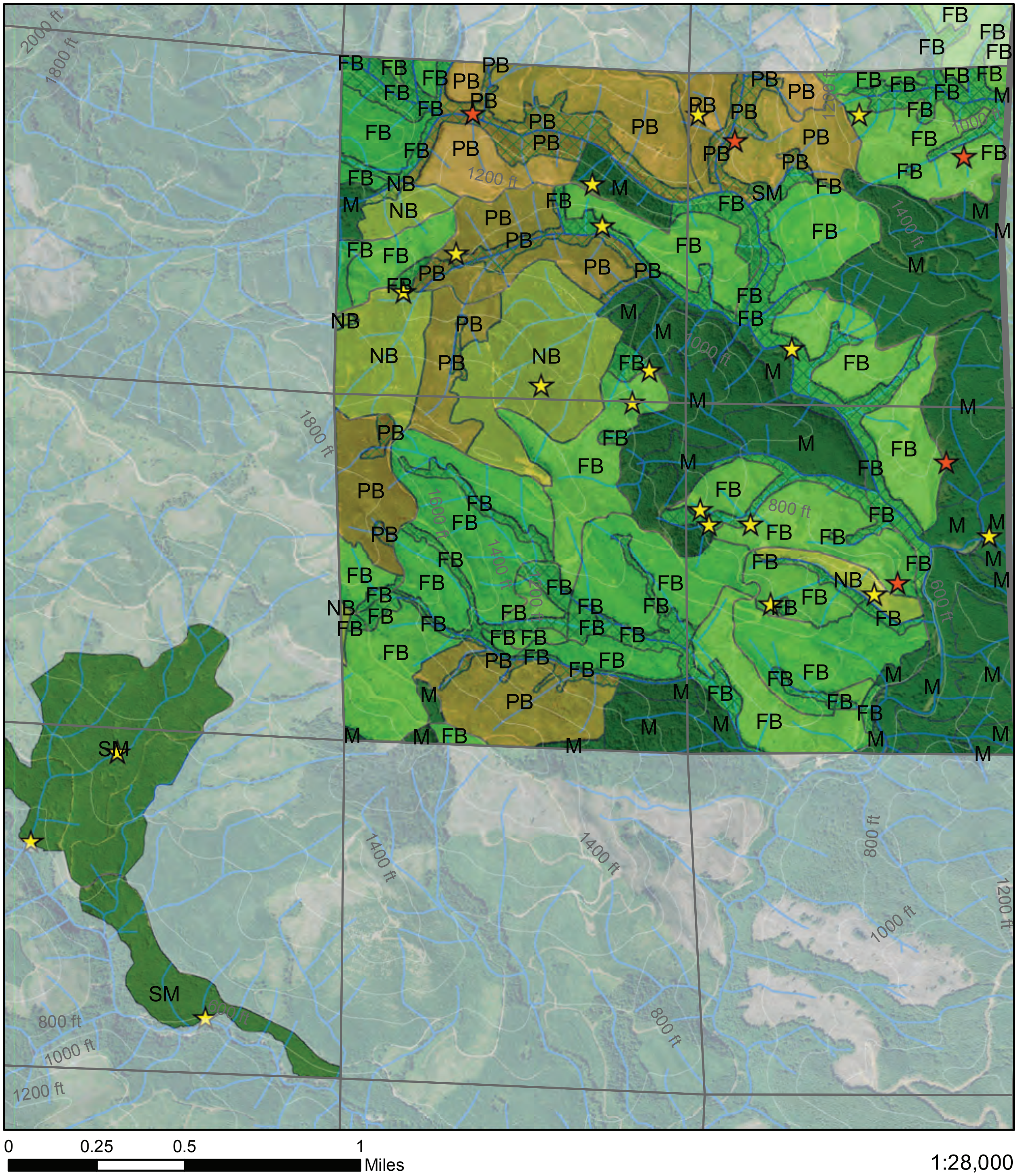
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ★ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



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Block 123

Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

★ Road

☆ Hillslope

Harvest treatment:

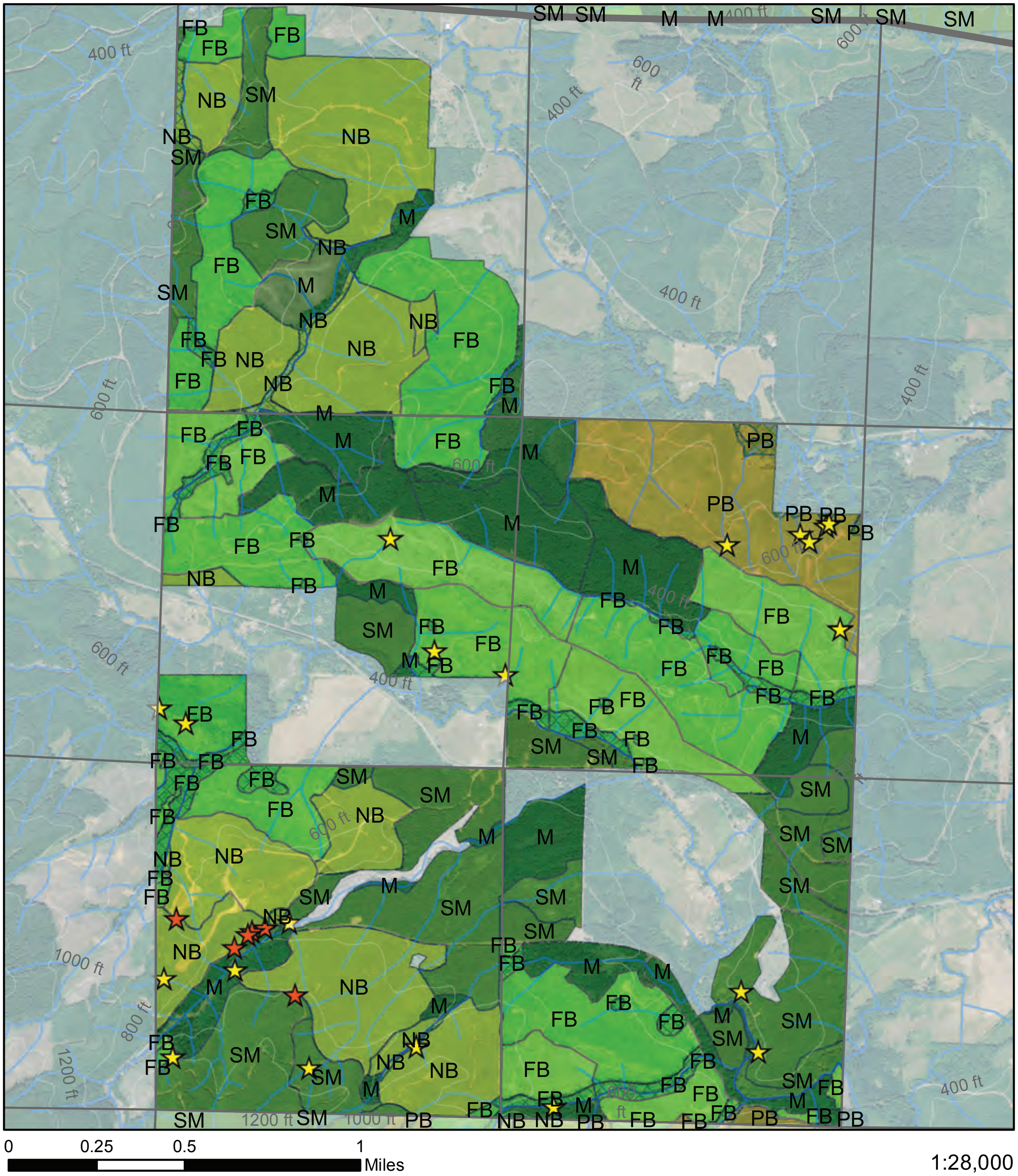
🟨 No buffer (NB)

🟧 Partial buffer (PB)

🟩 Full buffer (FB)

🟩 Submature (SM)

🟩 Mature (M)



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Block 154

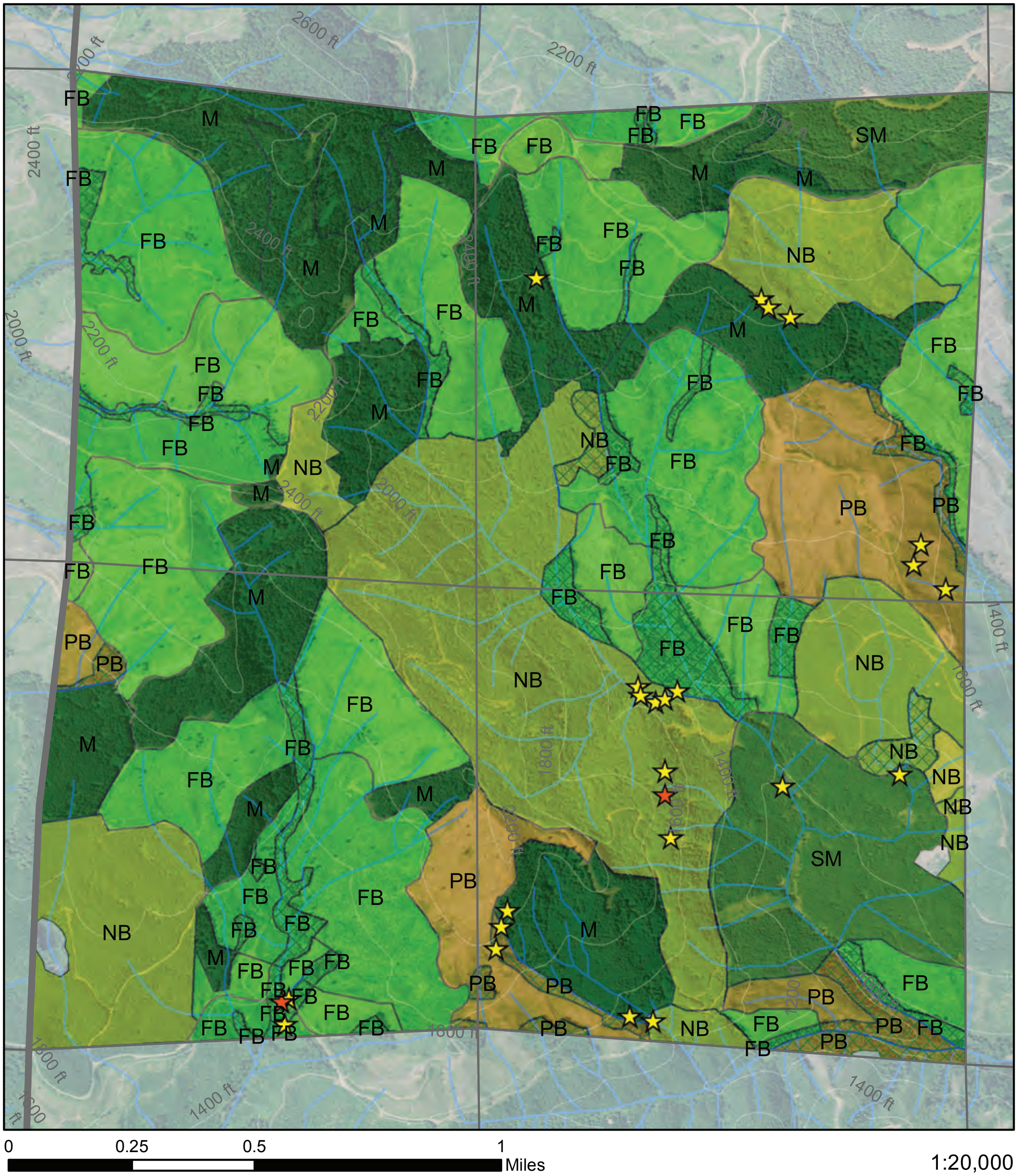
Landslides and harvest treatment groups by block. Cross-hatch used to indicate buffers, Ortho photography flown between April and May of 2008.

Landslide event location

- ★ Road
- ☆ Hillslope

Harvest treatment:

- 🟨 No buffer (NB)
- 🟧 Partial buffer (PB)
- 🟩 Full buffer (FB)
- 🟩 Submature (SM)
- 🟩 Mature (M)



1:20,000