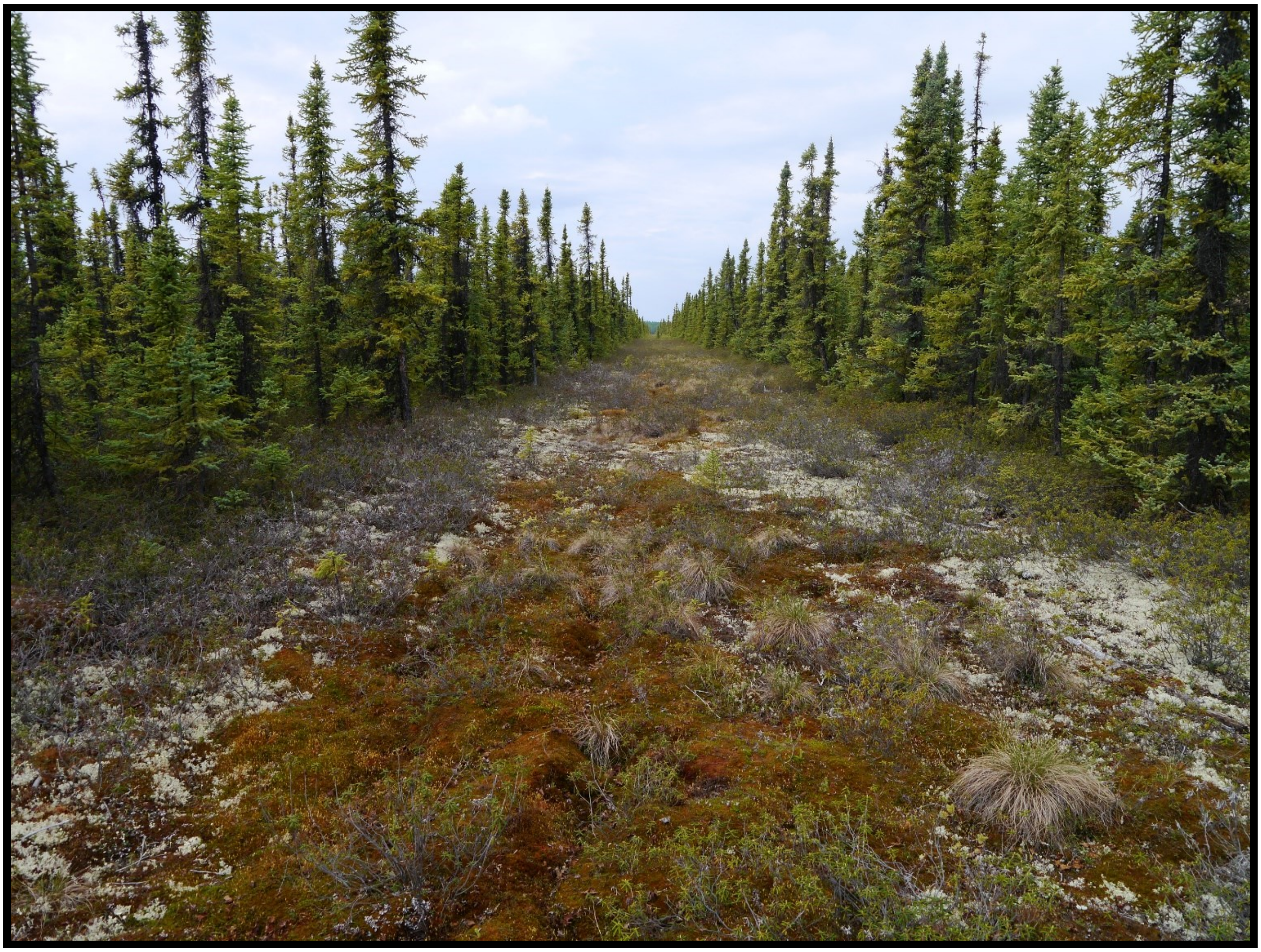
BC OGRIS
BC Oil and Gas
Research and Innovation Society

Testing Functional Restoration of Linear Features within Boreal Caribou Range

Phase 1: Revised Proposal



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BACKGROUND

Boreal caribou are provincially *Red-listed* in British Columbia and federally listed as *Threatened* due to population declines throughout much of their distribution. The main proximate cause of population decline is increasing predation[1,2], which ultimately has been linked to landscape disturbance within caribou range[3–5] and climate change[6,7]. Within western ranges of boreal caribou, linear features (LFs) such as seismic lines, pipelines and roads are a prominent form of disturbance and these features are thought to increase predation of caribou by increasing predator hunting efficiency [8,9] and facilitating predator movement into caribou range [10,11]. Because of these mechanistic links, limiting predator movement on LFs has become a management priority[12] for stabilizing caribou populations in the long-term.

In 2016, DeMars & Benesh[13] produced a proposal outlining a three-phase framework for developing and testing techniques for functionally restoring LFs with caribou range. Functional restoration refers to techniques that aim to limit predator use of LFs to ultimately restore historic caribou-predator encounter rates but such techniques do not necessarily result in the restoration of lined areas to their pre-disturbance structural state (i.e. ecological restoration). In the first phase of their framework, DeMars & Benesh[13]recommended a pilot study where promising techniques are tested on LFs highly used by wolves. The selection of highly used LFs was a focal part of their design as the relatively high rates of wolf encounter on these LFs would limit sample sizes necessary to detect treatment effects, thereby potentially increasing cost efficiency. While this approach held promise, recent analyses of remote camera data collected from May – August 2016 suggested that seasonal changes in wolf behaviour may impact the viability of this strategy. Specifically, two of the three wolf packs monitored did not return to use the previous year's den site; consequently, LFs highly used in one year had little to no use in the following year. Such behavioural changes would confound inferences regarding treatment effects. In a February 2017 update to the original proposal, DeMars suggested that alternative study designs needed to be considered, though his suggestion of randomly selecting LFs within caribou range would require significantly increased sample sizes and, accordingly, increased project costs.

To develop alternative testing designs more aligned with the cost-effectiveness sought by DeMars & Benesh [13], a scoping committee was convened on 12 April 2017. Representatives at the meeting included university-based researchers, the coordinator of BC's Research Effectiveness and Monitoring Board, and industry-affiliated biologists, all with expertise in linear feature mitigation. In this revised proposal, we outline a new study design based on the outcomes of this meeting. This design employs a new methodology for increasing wolf encounter rates on selected LFs to limit sample size requirements and also recommends the testing of two recently developed techniques for restoring LFs; specifically, tree-bending and tree-hinging.

Scope and Objectives

This revised proposal is specific to the first phase (i.e. *Year 1*) of the three-phase framework outlined by DeMars & Benesh[13] and therefore has a similar primary objective; that is, to cost-effectively test the efficacy of recently developed techniques for functionally restoring LFs. Assessing the efficacy of a particular technique in a controlled, smaller-scale experiment is a critical first step before deploying and testing techniques over a larger, more biologically meaningful area (e.g. a typical wolf pack's territory). Below, we discuss in further detail key components inherent to our primary objective.

1. Cost-effectiveness

   "Cost-effectiveness" has two meanings in our objective, one explicit and one implicit. For the former, evaluating potential techniques should be done in a cost-effective manner due to the high costs associated with deploying such techniques in remote locations. With the current design, a primary goal was to develop a methodology that would significantly reduce the sample size requirements suggested by DeMars in February 2017 (e.g. 60-70 sites) and more align with the smaller sample sizes suggested by DeMars & Benesh[13] (e.g. ≤ 30). Cost-effectiveness is also implicit in the types of treatments selected for testing. Perceived benefits of functional restoration over ecological restoration are that functional techniques should be faster to deploy, affect the targeted biological process in a shorter time frame, and be more cost-effective. In terms of cost, many ecological restoration techniques have costs in the range of $10,000 – 20,000 per km. We suggest that viable functional restoration techniques should have costs that are 50% lower.

2. Evaluate the efficacy of a given technique

   In the context of caribou conservation, the proximate goal for restoring LFs is to limit their use by predators. "Limit", though, is a qualitative term and an assessment of technique efficacy requires a more quantitative evaluation. Ultimately, the goal of restoration should be to limit predator use and movement rates to levels similar to those expected if the forest were intact. Determining such baseline levels, however, is difficult in areas where other LFs are still available for predators to use. Yet, without this information, quantitative comparisons and objectives are still possible. Recent literature suggests that wolves highly select LFs, often at rates 4-5 times higher than areas off of lines [9,11]. Given these differences, techniques should be considered effective only if large reductions in predator use are evident on treated LFs compared to controls. We suggest that an 80% reduction in predator use should be a reasonable target for a given functional restoration technique.

PROJECT METHODS

Similar to the study design proposed by DeMars & Benesh[13], our study design considers individual LFs as the sampling unit and uses a comparison between treated and control LFs to evaluate treatment efficacy. Our design, however, has two key differences. First, we suggest using lures or baits to increase encounter rates on LFs and potentially overcome the statistical difficulties that occur when a significant proportion of LFs have ≤ 1 encounter during a given monitoring period. Such lures could be strictly olfactory (i.e. wolf urine, beaver castor) or entail a small food reward or a combination of both, although food rewards may be impractical if bears are more likely to access bait stations before wolves. ***Because the assumption that lures will significantly increase encounter rates on LFs is a critical component to our study design, we recommend a small pilot study be conducted prior to the deployment of LF treatments to evaluate the degree to which lures change encounter rates and to assess the rate at which lure stations would need to be replenished.*** This pilot study could be as simple as deploying baited camera stations on a small sample of LFs (e.g. n = 10) and comparing encounter rates with a similar sized sample of control LFs where unbaited cameras are deployed.

In terms of general framework, we suggest that LF treatments (see below) be deployed in winter or early spring, then lures deployed and LFs monitored during the snow-free season. This timing allows for monitoring when LFs generally have the highest selection by wolves[10] and avoids having to access bait stations in winter, which could potentially confound inferences by packing snow and consequently facilitating wolf movement[9]. Unlike DeMars & Benesh[13], we suggest that a before-after-control-imapct (BACI) design is not necessary if lures are used, particularly if long-term lure use causes habituation of the animals to the lure (i.e. lure effectiveness decays with time). Rather, in our approach the response metric is the degree to which animals use LFs to reach the bait stations. If the restoration is effective, LF use by predators should be markedly lower (e.g. a reduction of 80%) on treated LFs versus controls (see *Site-level Design* below).

The second key difference in our design is we recommend testing a new technique for functionally restoring lines, which is explained in the following section.

Proposed Restoration Techniques

In the project's original proposal, tree felling and fencing were suggested to be the most promising techniques for functionally restoring LFs[13]. Here, we propose testing two techniques recently developed in Alberta: tree-bending and tree-hinging. In tree-bending, the tree stem is bent and fixed to the ground such that majority of the stem is perpendicular to the LF yet still elevated off the ground. A similar effect can also be achieved by mechanically tipping the root ball (e.g. with a bulldozer). Note that tree-bending is best accomplished in non-frozen conditions (e.g. summer and fall). With tree-hinging, the tree stem is partially cut anywhere from waist- to chest-high off the ground such that the stem falls perpendicular to the LF and the stem remains elevated off the ground (Fig. 2). In some instances, this type of cut may result in the tree continuing to live. In Alberta, treatment intensity for tree-bending or hinging has averaged approximately three stems per 10-m (Andrew Carpenter, Reclaimit, pers. comm.), though treatment intensity could be varied and further tested in an adaptive management framework.

In comparison to tree felling – where the fallen stem lies on the ground, the main advantages to tree-bending and tree-hinging are that: i) barrier effects remain in place for a longer time period because rates of decomposition are slower with the stem elevated above ground; and ii) the increased height of the barrier should further limit animal use of the LF. Potential drawbacks are that: i) tree bending costs could be high if extensive bulldozer use is required and site access is difficult; and ii) tree-hinging is an inherently more dangerous logging technique requiring special skills and, as a result, some contractors may be unwilling to perform this type of work. However, we know of at least two contractors in Alberta that have experience in these types of restoration techniques.

Site-level Design

To assess the degree to which tree-bending or hinging limits predator use of LFs, we propose a site-level design where a lure or bait is placed in the center of two treated segments (Fig. 2). We suggest that each treated segment be at least 200-m in length. At each site, three remote cameras are deployed. One camera is deployed at the bait station to record animal encounters with the bait. Each of the other two cameras are placed on the LF approximately 50-m away from the bait station to record potential animal movements along the LF. At each camera site, a treatment gap of 10 – 15-m is left so the ability of the cameras to capture animal movement is not impaired. At control sites, the same three-camera arrangement is deployed (Fig. 2). All control and treatment sites should be monitored for at least three months, as recommended by DeMars & Benesh[13]. Further, a pilot study should be conducted to determine the rate at which bait stations need to be replenished (as suggested previously).

The bait station camera and the two LF cameras have differing functions. The bait station camera will record animal encounters with the bait but will give no indication as to whether the LF was used to reach the bait. The two LF cameras, on the other hand, should capture any animal movement along the LF. This design can allow for stronger comparisons to evaluate treatment effects. For example, if an animal occurs at a bait station but is not captured at the LF cameras, then such a result provides strong evidence that the treatment is effective at pushing animals off of LFs. More specifically, the three-camera can answer the question “given an animal is in the area, does it use the LF?” The bait camera helps address the first component of this question by potentially capturing animal presence in the area. Without the bait cameras, it may be difficult to isolate treatment effects because rates of use at treatment and control sites may differ due to random chance or spatial variability in an animal’s movement behaviour. The use of the bait cameras can help to control the influence of these potential confounds.

Figure 1: Tree-hinging techniques for functionally restoring linear features. These techniques result in the cut stem being elevated off the ground, thereby significantly slowing the rate of decomposition. (Photos courtesy of M. Cody).

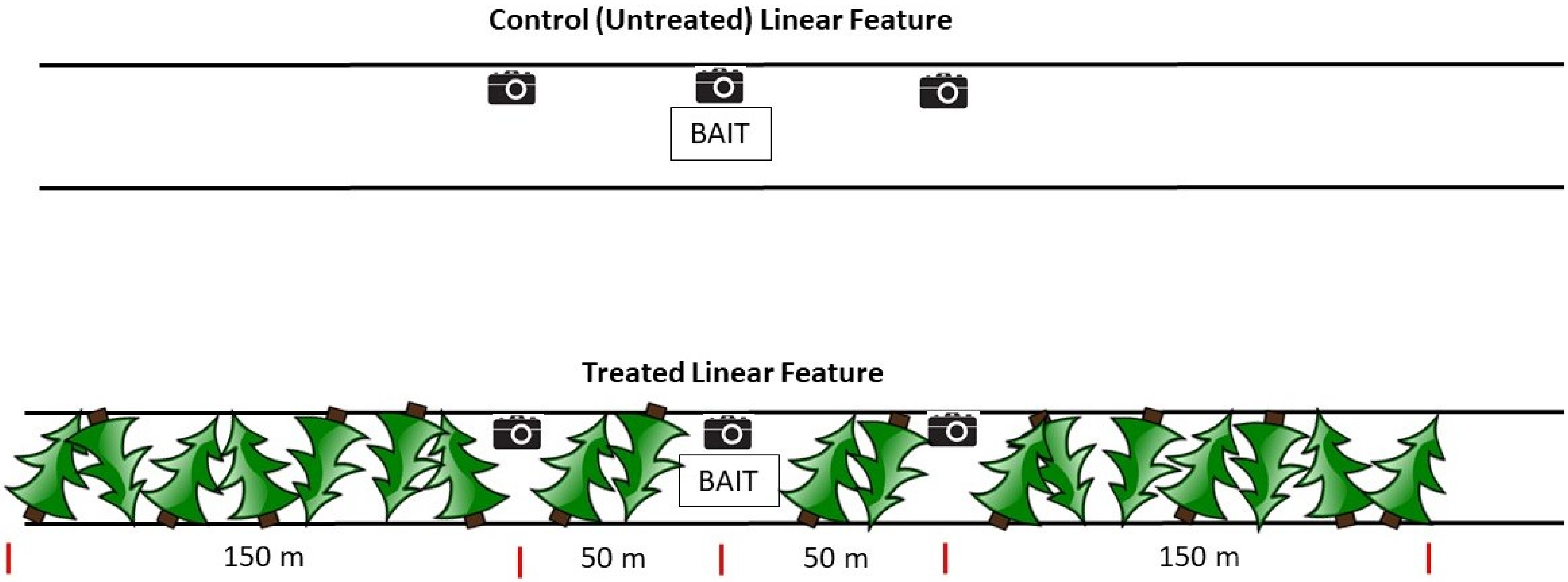

Figure 2: Proposed design for deploying tree-hinging or tree-bending treatments on selected linear features (LFs) and the three-camera array used to animal movement along the LF and at the bait station.

Site Selection and Sample Size

To assess potential sample size requirements, we conducted statistical power analyses using data collected from 43 remote cameras deployed on LFs in northeast BC between end-May to end-August 2016 (Appendix A). Analyses were conducted for two scenarios, one where no lures were used and one with lures. For the latter scenario, we assumed that lures resulted in all sites receiving at least one wolf encounter during the monitoring period. Results from these analyses showed that power is affected by the estimated size of the treatment effect and by the use of lures. In general, if the estimated treatment effect is large (i.e. > 60%) and cameras are lured, then sufficient power (i.e. an 80% probability of rejecting a false hypothesis of no treatment effect) may be possible with as few as 10 treatment and 10 control sites. Alternatively, if cameras are unlured, sample sizes need to be at least 30 and preferably larger if the treatment effect is < 80%. Because the true effect of luring is unknown (and may not result in all sites receiving at least one encounter), we suggest that 20 treatment sites and 20 control sites be considered a minimum sample size.

Selecting potential sites should take into account several factors. First, treatment and control sites do not need to be paired and, in fact, sites should be spatially separated (e.g. >1 km apart) to maintain relative independence among sites. Such separation may be important if animals habituate to the lure (i.e. after investigating one site, animals may become more or less likely to visit a nearby site). Second, treatment and control sites should be equally distributed with respect to land cover type (e.g. peatlands versus uplands). Third, sites should be distributed among at least 3 wolf packs to capture potential variation in wolf behaviour toward LFs and the bait stations. Moreover, sites should be selected within the territories of packs with existing wolf GPS data to ensure LFs have a higher probability of wolf use. Potential areas include wolf packs situated within the Clarke and Prophet caribou core areas (Fig. 3). Fourth, selected LFs should have baseline structural conditions that are similar. We suggest that to best evaluate treatment effects, all selected LFs should have baseline conditions that are conducive to wolf use (e.g. low vegetation height – see Dickie et al. [9]). Fifth, to reduce costs and facilitate access, we suggest selecting LFs along all season roads (Fig. 3). Selecting sites near roads may be particularly advantageous if baits need to be replenished frequently. Finally, we recommend that LFs not be selected within large peatland complexes as the use of lures in these areas could increase caribou-predator encounters.

Statistical Analyses

Remote camera data tends to be highly skewed, with many sites receiving few (or no) animal encounters. Although we suggest that the use of lures should increase encounter rates, we still expect that the resulting data will be skewed. As a result, comparisons between treatment and control groups can be accomplished using a Wilcoxon rank sum test, which is non-parametric and does not require that data be normally distributed. Generalized linear regression models with a negative binomial distribution could also be used to compare treatment and controls and such an approach may be advantageous for incorporating site-specific variables such as land cover type or line density to control for their potential confounding effects.

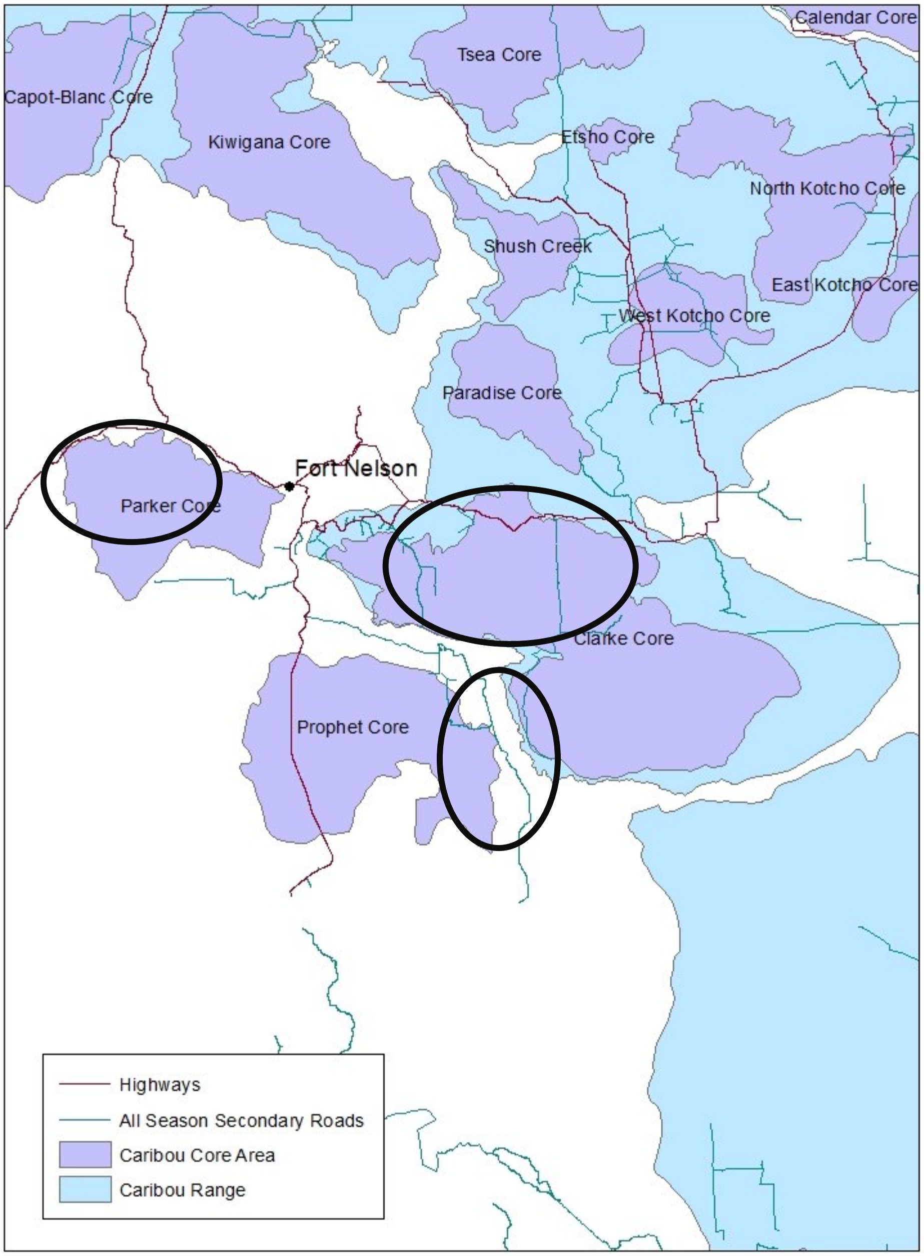

Figure 3: Potential study areas (black circles) for assessing the efficacy of techniques for functionally restoring linear features in northeast British Columbia. All areas are in close proximity to Fort Nelson and can be reached by all season roads. Circled areas in the Prophet and Clarke caribou core areas have pre-existing GPS data from radio-collared wolves.

Estimated Costs

We estimated project costs to be **$418,000** (Table 1). To estimate costs for tree-bending and hinging, we contacted Reclaimit Ltd. In Red Der, AB as they have experience in these techniques. They estimated costs at $6,000 / km, which includes a labour team of three. Although our design suggests treating only 400-m per site, we kept costs at the 1-km level for each site to account for the added logistics of moving site to site. A key uncertainty affecting our overall budget is the frequency at which bait stations will need to be replenished. This uncertainty will affect labour costs associated with the research technician and First Nations monitor. While we encourage involving the local community as much as possible in the project, the need to replenish baits frequently will place an increased emphasis on having a qualified research technician permanently located in Fort Nelson for the duration of the project. We estimated the project as having an 18-month time frame, beginning with site selection and ending with the completion of final project report. Within this estimated budget, there are opportunities for cost savings. For example, approximately 45 remote cameras may be available from previous projects and costs associated with a research technician may be reduced if a graduate student is brought on to the project.

Table 1: Estimated budget for testing tree-bending and tree-hinging techniques to functionally restore linear features in northeast British Columbia.

Cost Description	Item	Cost / Unit	Units	Amount
Equipment	Remote cameras	$800	120	$96,000
	SD cards and batteries	$50	120	$6,000
	Lure or bait	$5,000	1	$5,000
Flight costs	Recon flight for site selection	$12,000	1	$12,000
Travel / Accommodations	Trucks (1 per week)	$600	16	$9,600
	Mileage (per km)	$0.31	15000	$4,650
	Housing (per day)	$200	90	$18,000
	Meals (per day)	$45	90	$4,050
Labour	Tree-hinging labour & associated costs (per km)	$6,000	20	$120,000
	Research technician (per month)	$2,500	15	$37,500
	Project manager (per year)	$20,000	1	$20,000
	First Nations monitor (per day)	$600	60	$48,000
Subtotal				$380,000
Contingency	10% of total			$38,080
Total				$418,880

PROJECT STRUCTURE

Deploying treatments on LFs in NE BC will require significant planning and engagement from all potential stakeholders in the area of interest. Consequently, a necessary first step will be the formation of a project steering committee that includes members from government, First Nations, industry and academia (Fig. 4).

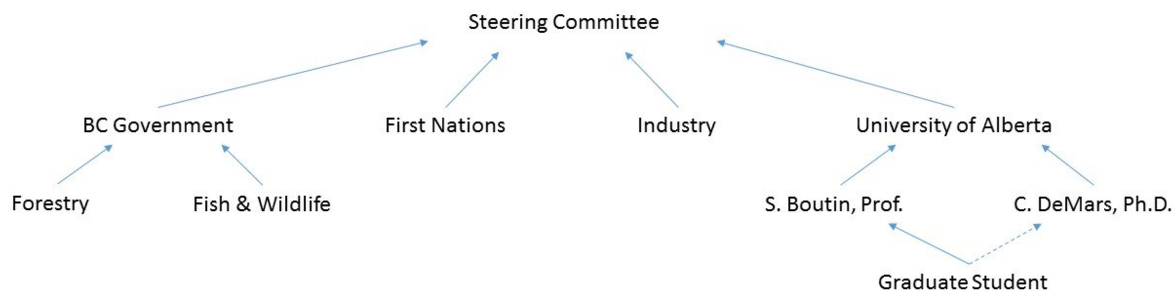

Figure 4: Potential structure of the project's steering committee.

The mandate of the steering committee will be to: i) provide financial oversight,; ii) offer expertise on project methodology; and iii) assist in negotiating the inevitable logistical hurdles that will occur when attempting to change landscape structure over large spatial scales. After each project milestone (see below), status reports will be delivered to the steering committee and a subsequent meeting will be coordinated to discuss project progress.

OUTCOMES AND DELIVERABLES

Outcomes of this project will directly inform management strategies aimed at stabilizing caribou populations by restoring habitat within caribou range. In particular, project outcomes will address a fundamental concern regarding caribou conservation in the boreal forests of western Canada; that is, can the vast network of existing linear features be effectively restored or deactivated at spatial and temporal scales that are biologically meaningful to caribou?

Throughout the project, we will produce status reports after major milestones, culminating with a final report at project completion (Table 2). We also anticipate disseminating project results through webinars and in-person presentations to all project stakeholders as well as at professional conferences. BC OGRIS will be credited as a funding source in the Acknowledgements section of all technical papers and publications as well as in the opening slides of all public and professional presentations.

Table 2: Estimated schedule for the completion of project reports

Deliverable	Milestone	Completion Date
Interim Report I	Pilot study to assess lure effects	October 1, 2017
Interim Report II	Deployment of LF treatments	March 31, 2018
Final Report	Data collection and analysis	December 31, 2018

Estimated Timelines

We estimated project timelines assuming that the primary treatment will be tree-hinging, which can be done during the winter (Fig. 5). If tree-bending is used, project times may be delayed by one month on 2018 as these treatments would likely be deployed in May when the ground has thawed.

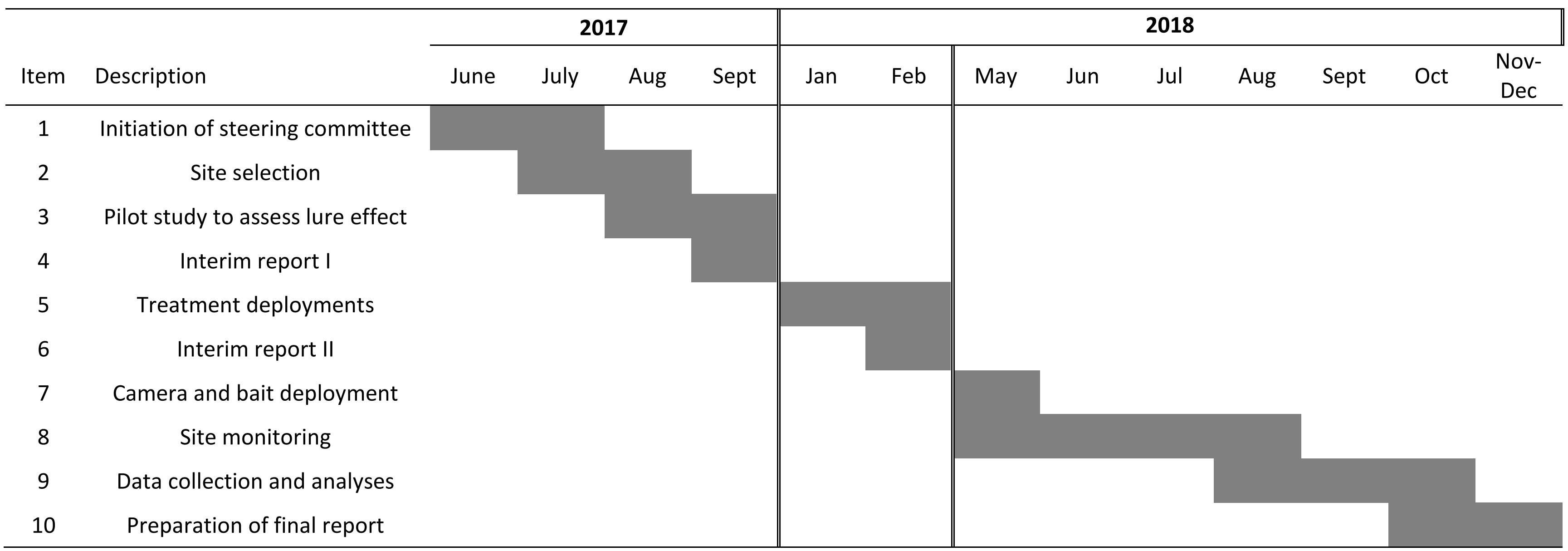

Figure 5: Gantt chart showing the projected workflow from project initiation to submission of the project's final report.

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APPENDIX A: POWER ANALYSES

We conducted power and precision analyses to assess sample size requirements for evaluating treatment effects of functional restoration techniques applied to LFs. These analyses used empirical data from remote cameras deployed on 21 LFs in northeast British Columbia from the end of May 2016 to the end of August 2016 (Bohm et al. 2016). To increase the probability of wolf encounters, cameras were deployed on LFs within close proximity of suspected den sites of three wolf packs. For each LF, a three-camera array was deployed to capture wolves moving along the LF (total number of cameras deployed = 63). Because of camera failures, we only used data from 43 cameras in these power analyses. Per camera encounters with wolves varied from 0 – 27 with the data having a highly skewed distribution due to many cameras (*n* = 20) recording no wolves (Fig. A1).

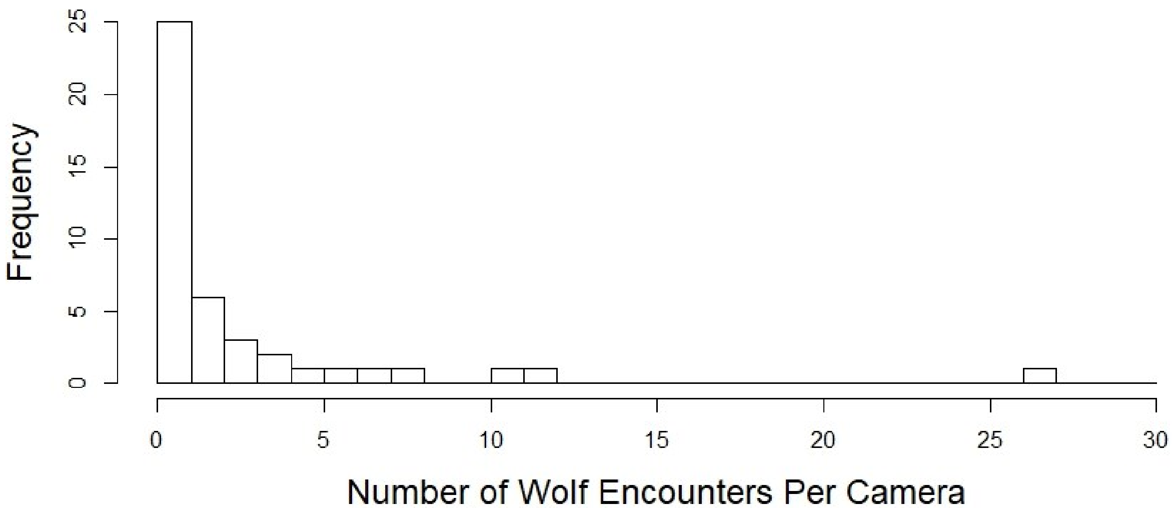

Figure A1: Frequency of wolf encounters per remote camera. Data were collected from 43 remote cameras deployed on 21 seismic lines in northeast British Columbia from the end of May 2016 to the end of August 2016. On average, cameras collected data for 95 days.

In the February 2017 update to this project (DeMars 2017), power analyses were conducted under the assumptions of a paired experimental design (i.e. a treated LF is matched to an untreated LF) and the data having a more normal (i.e. Gaussian) distribution because LFs were "highly used". Here, we conducted power analyses using an approach that more closely matches the distribution of the observed data and we assume that LFs are not paired (i.e. LFs are grouped as treatments and controls). Prior to analyses, we capped the maximum number of encounters per camera to 10 to reduce the influence of outlying data points (e.g. the camera with 27 encounters – see Fig. A1). To account for the data's skewed distribution and the high number of zeros among sites, we fit the data to a negative binomial distribution, which yielded an estimated distributional mean of 2.09 and a dispersion parameter of 0.48. Randomly drawing from this parameterized distribution yields simulated encounter data that might be reasonably expected from remote cameras deployed for a period of three months. For example, the following is one random draw of simulated encounter data for 30 remote cameras:

0 0 5 4 0 2 3 3 10 0 3 0 0 2 0 0 0 5 5 9 0 0 5 11 0 1 8 1 0 2

Note that this simulation approach yields data where a varying number of sites will still have no wolf encounters (i.e. zeros). In this proposal, we recommended the use of lures (or bait) to increase the probability of wolf encounters and reduce the statistical difficulties associated with zero-inflated data. Although the actual effect of lures is unknown, we simulated a 'lure effect' by assuming that the use of lures will result in at least one wolf encounter per camera during the monitoring period. To do so, we sampled from a zero-truncated negative binomial distribution. As a comparison to the above simulated data for 30 unlured cameras, a random draw of 30 from this truncated distribution yielded the following encounter data

1 2 1 3 3 2 1 2 1 1 1 1 7 3 5 2 3 1 3 4 3 3 3 1 3 3 6 5 5 1

Using these two distributions, we conducted power analyses for lured and unlured cameras using a Monte Carlo simulation approach similar to DeMars (2017). We assessed the statistical power associated with sample sizes varying from 10 to 200 in increments of 10. In our framework, a sample size of 10 equates to 10 treated sites and 10 control sites. For each simulation iteration, we drew a control sample from the relevant parameterized distribution then reduced the encounter data by 20%, 40%, 60% and 80% to simulate treatment samples under varying effect sizes. We compared treatment and control groups using a Wilcoxon rank-sum test and estimated *p*-values and 95% confidence intervals for each test. To account for uncertainty in the shape of the parameterized distribution from which encounter data were sampled, we varied the mean of the distribution by one standard error in each direction (i.e. from 2.09 to 1.58 and 2.60). For each combination of distribution mean, sample size, and effect size, we ran 1000 iterations. To estimate statistical power for each combination, we report the proportion of Wilcoxon rank-sum tests having *p*-values < 0. 05. Because 95% confidence intervals are difficult to compute from Wilcoxon tests when ties are present in the data (i.e. many sites with the same value), we do provide an evaluation of statistical precision with these analyses.

Results suggest that power is affected by the estimated size of the treatment effect and by the use of lures (Fig. A2). If the estimated treatment effect is large (i.e. > 60%) and cameras are lured, then sufficient power (i.e. an 80% probability of rejecting a false hypothesis of no treatment effect) may be possible with as few as 10 treatment and 10 control sites (Fig. A2 (B)). However, if cameras are unlured, sample sizes need to be at least 30 and preferably larger if the treatment effect is < 80%. Note that these analyses assumed that the use of lures resulted in all sites having at least one wolf encounter. The true effect of luring on encounter rates may be lower (or higher) than this assumption. Regardless of whether luring is used, sample sizes will need to be very large if treatment effects are < 20%.

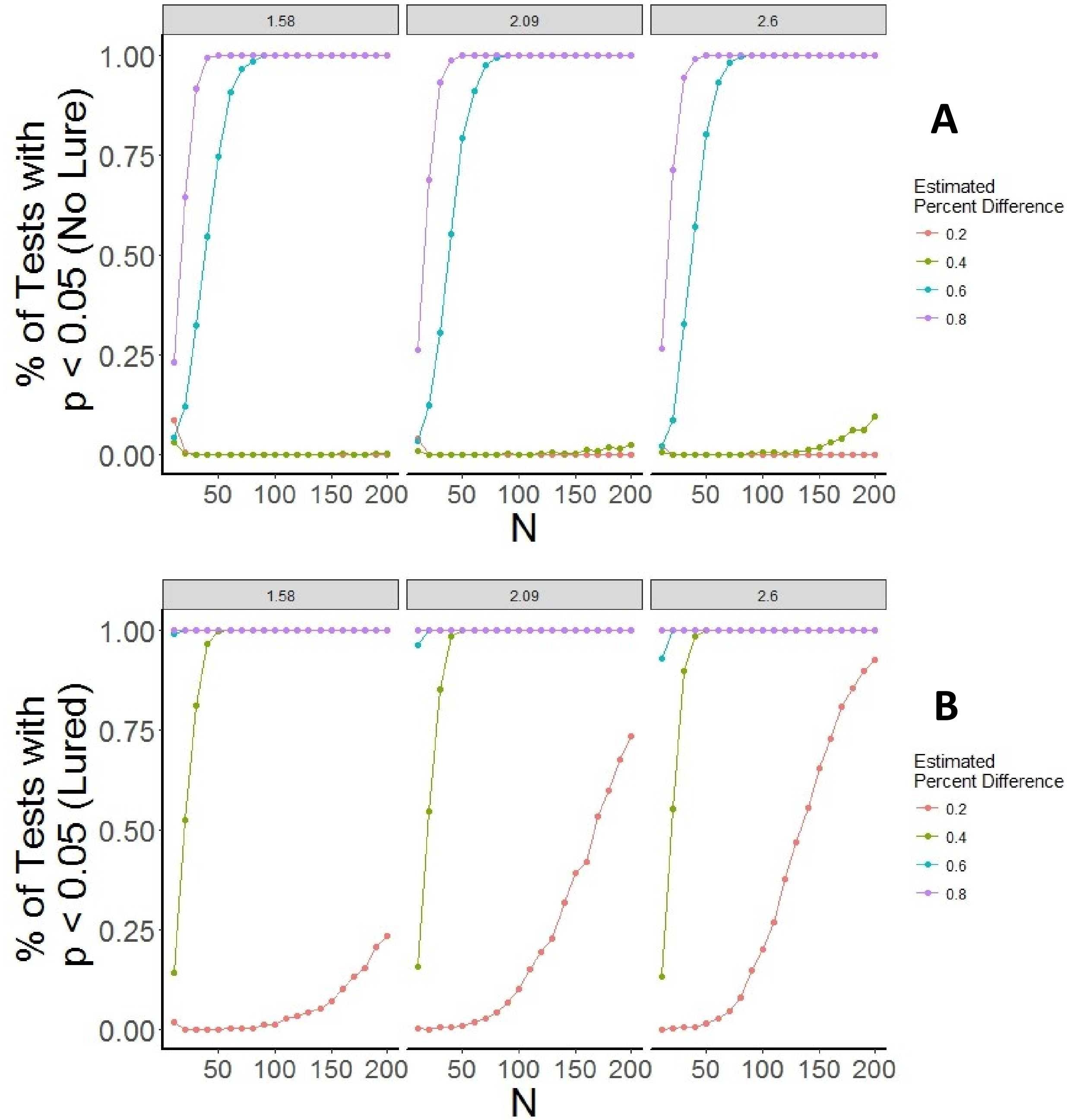

Figure A 2: Power analyses evaluating sample size requirements (N = number of treatment sites and number of control sites) necessary to identify potential effects of functional restoration treatments applied to linear features (LFs) in northeast British Columbia. Analyses used Monte Carlo simulations (n = 1000) of Wilcoxon rank-sum tests comparing the differences in wolf encounters between remote cameras deployed on treated and control LFs. In the top graph (A), cameras are unlured while in the bottom graph (B) camera sites have lures (baits) to increase overall encounter rates. Each facet represents different means in the negative binomial distribution from which simulated encounter data were drawn. Different line colors represent expected differences in encounter rates on treated linear features.