POWER ANALYSIS OF BAIT STATION SURVEYS IN IDAHO AND WASHINGTON

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Abstract: We evaluated statistical power for detecting trends of specified magnitude in visitation rate for American black bear (*Ursus americanus*) bait stations in Idaho and Washington. We found evidence for lack of independence due to multiple visits when bait stations were 0.8 km apart and no evidence for this with stations 1.6 km apart. Based on the variability observed in Idaho, we assessed power for several sets of criteria. The minimum criteria were a relative decline of 50% over 3 years at $\alpha = 0.20$ and power = 0.80. These criteria were met for many of the Idaho surveys, but were generally not met in Washington. More stringent criteria of a decline of 25% over 3 years at $\alpha = 0.10$ and power = 0.90 were not met in either state. The initial visitation rate had a predominant influence on power, and in areas such as western Washington, where visitation was low but bear populations thought to be substantial, an effective monitoring program is contingent on improving the visitation rate through changes in survey methods. For long-term monitoring (5, 10, or 20 years), we estimated sampling requirements for declines of 50%, 25%, and 10% with $\alpha = 0.10$ and power = 0.90 and estimated the costs of this sampling. Due to the inherent variability of bait station surveys, substantial sampling is required for detecting trends, and this method is likely to be cost effective only where visitation rates are relatively high. Although power analysis appears to be objective, determining the values for parameters used in its calculation is quite subjective and the results should be interpreted accordingly.

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For effective management of animal populations, some measure of the relative or actual abundance of the population is needed. This measure should be independent of harvest because of potential biases in harvest-based procedures and because not all populations are harvested. Black bears present many problems in deriving population estimates (relative index or actual population estimate) because of their relatively low densities, dense habitats, and solitary nature. Some approaches, such as capture–recapture or resight, are useful, but may be too costly to be conducted routinely over wide areas.

Bait stations have been advocated as a potential solution to this problem (Carlock et al. 1983, Johnson 1990, Beecham and Rohlman 1994), and bait-station surveys have been conducted for a number of years in many states (e.g., Tennessee, Georgia, North Carolina, Idaho, Michigan, Wisconsin, Minnesota, and South Carolina; Johnson 1990). These surveys have been used to track populations over time, assess seasonal habitat use (Pelton 1984), and compare bear visitation according to site characteristics (Pitt and Jordan 1996). Despite this extensive use of bait stations, questions remain about the suitability of this technique for monitoring or comparing black bear populations.

Much of the evidence supporting the view that bait station visitation rates reflect bear population levels has been anecdotal where differences in visitation rates were comparable to perceived population differences over time or between areas (Johnson 1982, Pelton 1984, Carlock 1986, van Manen 1988, Johnson 1990). Carlock et al. (1983) reported a positive correlation between visitation rate and mark–recapture population estimates over 5 years in Tennessee ($r^2 = 0.83$), but a similar relationship was not evident in Minnesota (Garshelis 1990). There was, however a relationship between visitation and hunter success (Garshelis 1990). In Idaho, bait station visitation dropped from a 3-year average of 24% to 8% after 35 bears were removed from the Priest Lake study population of about 100 (Beecham and Rohlman 1994). At the Council study area (population ≈135, Beecham and Rohlman, Idaho Department of Fish and Game, unpublished data), the positive trend in visitation reversed following the removal of 19 and 33 bears in succeeding years (Beecham and Rohlman 1994).

Reservations about the relationship between bait station visitation and population levels have focused on 3 areas: lack of independence, the effects of confounding factors, and site-specific influences. Lack of independence is addressed later in this paper. Confounding factors include food availability, weather, and timing of the surveys relative to annual climatic variation and plant phenology. These influences likely affect visitation rate independent of bear population levels. In some cases, these effects can be included in the analysis if their levels are measured, as Garshelis (1993) advocated for food availability. Otherwise, these factors add variability to the visitation rates, which reduces power for the analysis of changes in visitation rate and complicates interpretation of rates for individual years. If a point estimate is desired for visitation rate, it would be better to use a running average over several years as is done in Wisconsin (B.E. Kohn, Wisconsin Department of Natural Resources, Rhinelander, Wisconsin, personal communication, 1998). Site-specific influences likely affect visitation rates. For example, trail versus road (LeCount 1982), elevation, type of road, and forest type (Carlock 1986), and distance from roads and trails (van Manen 1988; J. Mantey, and D.A. Immell, 1995. Influence of roads on black bear detections at bait stations, Department of Wildlife, Humboldt State University, Arcata, California, USA). Because of these potential influences, bait station routes should be fixed between years and trend analysis for visitation rates should employ an analysis of covariance design. These influences also make comparisons among areas problematic unless they are included in the survey design as was done by Powell et al. (1996).

We conclude that properly designed bait station surveys can provide useful information for trend analysis, but many extraneous factors add variability to the data. This added variability is likely to mask changes in visitation rate to due small changes in population density. The question is, to what extent is this true? What magnitude of change in visitation rate is likely to be detectable despite this variability? Statistical power analysis provides a framework within which to address these issues. Toward that end, we addressed the following questions:

Were sampling levels employed by a monitoring program in Idaho and a pilot survey in Washington sufficient to detect specified magnitudes of change in the visitation rates?

In cases where the sampling was deficient, what changes could be made to improve the survey's performance?

METHODS

Study Areas

We ran bait stations in the Council, Coeur d'Alene, and Priest Lake study areas in Idaho. On the Council study area in south-central Idaho (elevations 975–2,470 m), low elevation (<1,700 m) timber stands of ponderosa pine, (*Pinus ponderosa*), and Douglas-fir (*Pseudotsuga menziesii*) were confined to riparian areas, with open areas of big sagebrush (*Artemisia tridentata*), various grasses, and forbs. At higher elevations, grand fir (*Abies grandis*), subalpine fir (*A. lasiocarpa*), and Engelmann spruce (*Picea engelmannii*) were the dominant trees. The Coeur d'Alene study area in the Idaho panhandle (elevations 890–1,890 m), was comprised of 3 vegetative zones: the pine-fir zone at 790–850 m; the cedar-hemlock zone at 850–1,490 m; and the spruce-fir zone at >1,490 m. Tree species included subalpine fir, western red cedar (*Thuja plicata*), Engelmann spruce, lodgepole pine (*Pinus contorta*), white pine (*P. monticola*), and Douglas-fir. On the Priest Lake study area, also located in the Idaho panhandle (elevations 700–2320 m), lower elevation (<1,580 m) tree species were dominated by western hemlock (*Tsuga heterophylla*), Douglas-fir, and western redcedar. Higher elevation forests were dominated by subalpine fir and whitebark pine (*P. albicaulis*). Further details on these areas are given in Beecham and Rohlman (1994).

In Washington, we ran bait stations in each of the 8 black bear management units (BBMUs, Washington Department of Fish and Wildlife 1996, Table 1). In each

Table 1. Black bear management units in Washington State.

Unit	Location	Forest zones[a]
Coastal	The Olympic Peninsula and south including the Black Hills and Willapa Hills	Sitka spruce (*Picea sitchensis*), western hemlock, silver fir (*Abies amabilis*), mountain hemlock (*Tsuga mertensiana*), Douglas-fir, and alpine–parkland
Puget Sound	Base of the Cascade foothills west to Hood Canal and the San Juan Islands	Douglas-fir, western hemlock, silver fir, mountain hemlock, and alpine–parkland
North Cascades	Western base of the Cascade foothills east to eastern base, north of Interstate 90	Ponderosa pine, Douglas-fir, subalpine fir, and alpine–parkland
East Cascades	Lake Chelan south between the eastern Cascade foothills and the Columbia River	Ponderosa pine, Douglas-fir, and grand fir
South Cascades	Western base of the Cascade foothills east to eastern base, south of Interstate 90	Oak (*Quercus garryana*), Douglas-fir, grand fir, subalpine fir, and alpine–parkland
Okanogan	Eastern Cascade foothills east to the Okanogan River and Lake Chelan to the Canadian border	Ponderosa pine, Douglas-fir, subalpine fir, and alpine–parkland
Northeastern	Okanogan River east to Idaho	Ponderosa pine, Douglas-fir, redcedar, western hemlock, subalpine fir, and alpine–parkland
Blue Mountains	The Blue Mountain Range within Washington State	Ponderosa pine, Douglas-fir, grand fir, subalpine fir

[a]after Cassidy (1997).

BBMU, routes were placed in locations judged to be typical good bear habitat.

Field Methods

Each bait station consisted of 2 half-opened cans of sardines suspended 2–4 m above ground. After 5 nights, we checked stations for evidence of bear visits (claw marks on tree trunks and canine punctures in the cans). In Washington, we operated 27 routes in 1996 and 29 in 1997. Each route consisted of 40 stations 0.8 km apart (total of 2,240 stations). In Idaho, each of 643 routes consisted of 5 stations 1.6 km apart (3,295 stations, 1985–95).

From 1989 to 1995, hunting was allowed in the Council study area, thus we separated the analysis into hunted and not hunted periods.

Analysis

For bait station results to be meaningful, visits should be independent. Sargeant et al. (1998) noted 2 likely causes of lack of independence in scent station visitation which are applicable to bait stations. These are visits by individuals to more than one station on a route (behavioral dependence), and localized variation in population density (heterogeneity dependence). Behavioral dependence imparts a positive bias to the visitation rate because 1 animal makes >1 visit which is interpreted as visits by more than one animal. This is a methodological problem and is resolved by providing sufficient spacing between stations.

We tested for behavioral dependence by assuming that multiple visits would be to adjacent stations. We then used Resampling Stats (Bruce et al. 1995) to generate 500 sets of n -1 pairs of station scores (where n = the number of stations in the route) for each route with the probability of a visit equal to the visitation rate observed for that route. Each pair represented adjacent stations and were represented by visit–visit, visit–no visit, no visit–visit, and no visit–no visit. Each set was scored as to whether the number of visit–visit pairs was <, =, or > the observed number of visit–visit pairs. The proportion of sets which had less than the observed number of pairs was the probability of fewer than the observed number of pairs occurring by chance. One minus this proportion was the probability that the observed number of pairs was greater than expected by chance, or P. For example, on the Minot Peak route in Washington in 1996, 4 of 40 stations were visited, which contained 2 visit–visit pairs. One of 500 random samples of 39 pairs had >2 visit–visit paris, and 14 had 2 visit–visit pairs, the proportions being 0.002, 0.970, and 0.028, respectively. Thus, the probability of ≥2 pairs occurring was 1- 0.970 = 0.030, providing substantial evidence for behavioral dependence in this case.

This is a conservative test for dependence as it assumes independence and checks to verify it. This is because the visitation rate used to generate the sets may already have a positive bias, thus producing more pairs by chance than would have occurred with a lower (correct) visitation rate. To compensate for this effect, we set $\alpha = 0.20$ for these evaluations.

Population heterogeneity dependence results from uneven distribution of the bear population over the landscape. Thus, visitations to routes reflect the population densities from those particular locales and should vary accordingly. Although heterogeneity dependence is affected by methodology (distribution, length, and spacing of stations), it is primarily a sampling and statistical design problem. We did not test for population heterogeneity dependence, but addressed it by choosing a statistical model which incorporated the effects of this type of dependence. Specifically, we used analysis of covariance (ANCOVA), which effectively allows the mean visitation rates to vary among routes, but fits a common slope to the different routes within each study area. This reflects a conceptual population model which depicts population levels that vary across the study area but undergo a common rate of change over the monitoring period.

Statistical power is the likelihood of detecting a change or difference of a specified magnitude in a statistical test when such a difference actually exists. In other words, if we postulate a degree of change and a sample size, and given an estimated variability in the measurements, what is the probability of obtaining a significant P value in a statistical test (Cohen 1988, Steidl et al. 1997)? This probability is the statistical power, and as variability increases, power decreases, whereas increases in α, sample size, or the postulated degree of change increase power. Monitoring programs are usually designed to detect trends (increases or decreases) over several time units in which the degree of change is the steepness of the slope. However, the difference between measurements made at 2 different times is also sometimes considered, in which case the degree of change is the difference between the 2 measurements.

In statistical power analysis, one often selects an α level and degree of change and then estimates the sample size needed to achieve adequate power given the variability in the data. Determining what level of power is adequate is a management decision similar to determining the appropriate α value. In a statistical test, α represents the probability of concluding there was a significant difference when the difference observed was really just an artifact of the variability in the data. Similarly, power is the probability of detecting a given degree of change, and therefore is another assessment of the assurance the manager has of reaching an appropriate conclusion. Another way to view this is to consider the monitoring program as an alarm system which lets the manager know when the measured variables have changed. In this context, α is the probability of a false alarm. Power is the probability of the alarm sounding when a specified degree of change has occurred. For most of our evaluations, we used a power of 1 - α as our criteria. This indicates that we gave equal importance to both avoiding false alarms and ensuring detection of change, but in general this evaluation needs to be made in the context of every monitoring program.

In determining the magnitude of change that should be detected, an important consideration is whether this is stipulated as being relative to the initial index value, or an absolute value regardless of the initial index value. That is, do we wish the monitoring program to be able to detect a change that is a specified percent of the index, or simply a change in the index of a specified magnitude? The answer depends on how one thinks the index relates to population densities and the nature of change in the population density one wishes to detect. Detecting a population density change of a certain magnitude may be satisfactory regardless of the initial density (e.g. detecting the difference between a low population and a very low population may not be critical as long as one receives an indication of a change from low to medium levels, or from low to zero). However, in western Washington, we obtained low visitation rates in areas with substantial bear populations (as indicated from other information). We therefore chose to specify relative change in the visitation rate as the measure of change we wished to detect (e.g., 25% over 5 years, meaning the initial and final values over a 5-year period differed by 25% of the initial value).

To conduct power analysis, it is necessary to estimate the variability inherent in the data. The variance of the visitation rates can be estimated from binomial probabilities (Snedecor 1940, Zar 1996), and power can be estimated directly. However, this procedure is predicated on the assumption that each station is an independent observation, which does not match our statistical model (due to population heterogeneity dependence).

An alternative method of estimating the variation is by ANCOVA. Within each study area, we included routes as a factor in a regression of the visitation rates with year of the survey. Variation was then estimated from the residuals from the ANCOVA. This approach incorporates the inter-annual variability due to extraneous factors (e.g., weather, food availability) and uses the rate estimate predicted by the ANCOVA as the best approximation for that survey. This approach is also suitable given the relatively low reproduction potential and long life span of black bears, but could not be applied to species whose populations would be expected to fluctuate greatly among years. Variability assessed in this manner overestimates the true variability to the extent the actual (but unknown) population change is not linear but corresponds to the fluctuations in the visitation rate, and underestimates variability to the extent that the actual population change differs from the change estimated by the least-squares fit of the visitation rates.

Because the Idaho data were long-term and those from Washington were not, we performed the variability assessment on the Idaho data and used it to evaluate power in both states. We used each Idaho route (5 stations) as the sampling unit and considered the 40-station Washington routes as approximately equivalent to eight 5-station routes. To arrive at a common basis for both states, results are expressed in terms of the number of stations.

In the context of a given level of variability, power, α, sample size, and magnitude of change are interrelated. Using the power analysis facilities of JMP (SAS Institute Inc. 1995), we explored these interrelations in several scenarios. We considered the detection of a decline in the rate generally more important than the detection of an increase, so power was assessed for declines only. This does not mean that the monitoring program was intended to detect declines only, but that the criteria we used to assess its suitability were based on declines. Trend analysis would test for both increases and declines using 2-tailed tests, so the 2-tailed framework is maintained for our evaluations of power as well.

We judged that the minimal performance of a bear population monitoring program would be to detect a 50% decline in visitation rate over 3 years with $\alpha = 0.20$ and power = 0.80. (The 3-year period matches Washington's period of major review of harvest regulation). This is a large degree of decline, with substantial probability of a Type I error (α). While we would not be completely satisfied with a monitoring program that met these criteria, we felt that monitoring that did not meet these criteria would be clearly inadequate.

To evaluate our monitoring for more stringent criteria, we estimated sampling requirements for detecting a 25% decline over 3 years with $\alpha = 0.10$ and power of 0.90. We also evaluated the effect of accepting a higher probability of false alarms for 25% decline over 3 years by using $\alpha = 0.20$, but retaining power at 0.90.

We used the power estimates for these scenarios to evaluate the power to detect these declines given the sampling intensity and visitation rates observed in the Idaho study areas and for the Washington pilot surveys. In Washington, initial evaluations were made at the black bear management unit level. We also pooled management units into Westside (west of the Cascade crest — Coastal, Puget Sound, North Cascades, and South Cascades black bear management units), Eastside (east of the Cascade crest - Okanagan, Northeastern, and Blue Mountain black bear management units), and statewide. Although these lower levels of resolution would be far from optimum for bear management in Washington, we were interested to know if the consequent increase in sample size might make bait stations functional at that level in situations where they were not at the black bear management unit level.

For longer monitoring periods, we estimated power to detect declines of 10%, 25%, and 50% over 5, 10, and 20 years with $\alpha = 0.10$ and power = 0.90, and estimated the cost that would be incurred for the estimated number of stations based on our experience in Washington. This cost estimate was $26/station (all costs on 1997 basis) and included salaries for state biologists, transportation, and materials.

RESULTS

In Idaho overall, the number of sequential visits (pairs) observed was quite likely to occur randomly ($P = 0.884$), and none of the 406 routes showed a probability <0.20 of having occurred by chance ($0.404 \leq P \leq 1.000$). In Washington overall, the number of sequential visits (pairs) observed was also quite likely to occur randomly ($P = 0.622$). However, for 8 of 35 routes (23%), there was a <0.20 probability that the observed number of sequential visits occurred randomly.

Power to detect a 50% decline in visitation with $\alpha = 0.20$ was strongly influenced by initial visitation rate (Fig. 1). Except at very low sample sizes, initial rate was the predominant determinant of estimated power, e.g. a doubling of sample size increased power less than doubling initial visitation. This is because the actual decline is smaller for low initial visitation rates than for higher rates (50% of 0.40 is 0.20, while 50% of 0.04 is 0.02) and it is the actual, not relative, decline that is the basis for the power calculations.

This relationship is evident in assessing power to detect this level of decline in the Idaho study areas (Table 2). Power was above the criteria of 0.80 (= 1 - α) for visitation rates observed during all years in the Council study area prior to hunting. After hunting, visitation rates were sometimes high enough to detect this degree of decline in subsequent years, but often were not. In the Coeur d'Alene and Priest Lakes study areas, power met our criteria only during the years of higher visitation rate.

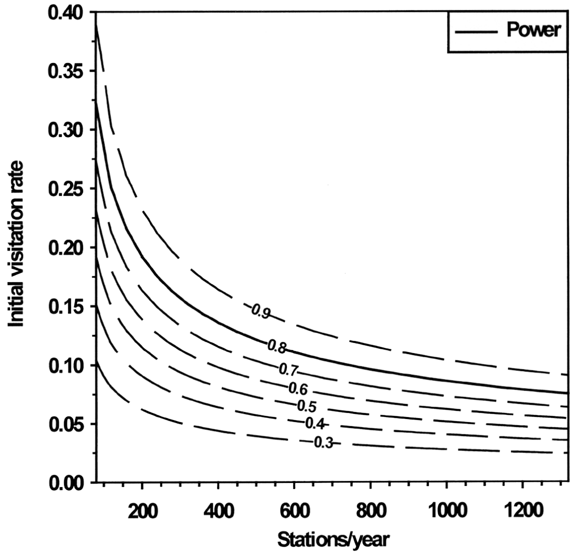

Fig. 1. Contour plot of power to detect a 50% decline in bait station visitation with a = 0.20. Solid line shows power = 1 - α = 0.80.

In Washington, power of 0.80 to detect a 50% decline over 3 years with $\alpha = 0.20$ was achieved for only the Northeastern black bear management unit in 1996 (Table 3). Power in other black bear management units was often well below this stipulated level. For black bear management units with low visitation rates (<0.10), no reasonable increase in sampling would yield the desired level of power (Fig. 1). Likewise, pooling black bear management units was not an effective strategy on the westside given the general low level of visitation in that area. On the eastside, the increased power resulting from pooling approached our criterion in 1996, as it did for both years for a statewide estimate.

For comparable initial visitation rates, sample sizes required to obtain a power of 0.90 for a 25% decline and $\alpha = 0.10$ (Fig. 2) were much higher than they were for a 50% decline with $\alpha = 0.20$ (Fig. 1). For the observed visitation rates in Idaho and Washington, this level of power was not achieved in any of the study areas or pooled estimates (Tables 2, 3). Under such circumstances, a manager might be willing to accept a higher rate of false alarms (higher α) in exchange for higher power. Although increasing α to 0.20 lowered sample size requirements con-

Table 2. Black bear bait stations survey results in Idaho, 1985–95.

Study area	Usual number of stations[a]	Visitation rate			Estimated power to detect: 50% decline $\alpha = 0.20$			Estimated power to detect: 25% decline $\alpha = 0.10$		
		Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Council — not hunted	160	0.41	0.35	0.47	0.99	0.98	0.99	0.64	0.53	0.74
Council — hunted	85	0.25	0.15	0.41	0.66	0.41	0.93	0.23	0.14	0.42
Coeur d'Alene	135	0.15	0.09	0.24	0.53	0.34	0.80	0.17	0.12	0.29
Priest Lakes	135	0.22	0.08	0.32	0.75	0.31	0.94	0.26	0.12	0.42

[a]The number of stations varied during early surveys.

Table 3. Black bear bait station survey results in Washington State, 1996–97.

	1996				1997			
			Estimated power to detect				Estimated power to detect	
Black bear management unit	Stations	Visitation rate	50% Decline, $\alpha = 0.20$	25% Decline, $\alpha = 0.10$	Stations	Visitation rate	50% decline, $\alpha = 0.20$	25% decline, $\alpha = 0.10$
Coastal	320	0.04	0.26	0.14	240	0.07	0.34	0.13
Puget Sound	120	0.13	0.44	0.15	120	0.03	0.21	0.10
South Cascades	80	0.04	0.21	0.10	80	0.04	0.21	0.10
North Cascades	240	0.04	0.24	0.11	360	0.07	0.38	0.14
Okanagan	120	0.11	0.37	0.14	120	0.13	0.44	0.15
Northeastern	120	0.27	0.84	0.31	120	0.18	0.61	0.20
Blue Mountains	80	0.01	0.20	0.10	80	0.03	0.20	0.10
East Cascades	0				120	0.00	0.20	0.10
Units pooled								
Eastside	320	0.14	0.76	0.26	440	0.10	0.63	0.20
Westside	760	0.05	0.44	0.15	720	0.06	0.46	0.16
Statewide	1,080	0.08	0.77	0.26	1,160	0.07	0.74	0.24

siderably for any given initial visitation rate (Fig. 2), these remain far above those in our study areas.

For longer term monitoring, power to detect a 50% decline with $\alpha = 0.10$ was determined by visitation rate and the total number of stations, regardless of the monitoring period (Fig. 3). Based on these calculations, we estimated the number of stations per year required to detect these magnitudes of decline (Table 4). For instance, a monitoring effort for a population with a visitation rate starting at 0.4 can detect a 50% decline over 5 years with 500 stations costing $13,000. If one is willing to wait 10 years to detect this decline, that would be 40 stations/year costing $1,100/year. Similarly, with a 25% decline over 5 years at a visitation rate of 0.2, 5,500 stations (1,100/year) at a cost of $142,000 ($28,400/year) would be required. A population with an initial visitation rate of 0.6 would be able to detect a 10% decline over the same period with about the same cost.

DISCUSSION

Randomization tests provided no evidence of behavioral dependence for the Idaho data, where stations were 1.6 km apart. In Washington, with stations 0.8 km apart, 23% of the routes showed evidence of behavioral dependence of adjacent stations. This percent is only marginally above the α level used for the test, indicating that nearly the same number of routes would be expected to provide such evidence under conditions of complete behavioral independence. Consequently, we conclude that to the extent that behavioral dependence occurred in the Washington routes, it was not at a high enough level to incur a significant bias or invalidate the sampling regime.

Power estimated for existing surveys in Idaho and Washington was not consistently above our minimum criteria (50% decline over 3 years, $\alpha = 0.20$, power = 0.80), indicating an unsatisfactory survey design. The obvious remedy of increasing sample size would fix this only for areas with higher visitation rates (above about 0.25). No reasonable increase in sampling would meet these criteria for areas exhibiting very low visitation rates (<0.10, Fig. 1). Such low rates were rare in Idaho but common in Washington, especially western Washington. Even so, a 50% decline is a large one over 3 years and an α of 0.20 with power at 0.80 represents a wide margin of error. Neither Idaho nor Washington areas approached our more stringent criteria of a 25% decline, $\alpha = 0.10$, and power of 0.90, although the unhunted Council study area would

Table 4. Stations per year required to detect declines of 10%, 25%, and 50% over 5, 10, and 20 years by initial visitation rate with $\alpha = 0.10$, power = 0.90.

		Initial visitation rate						
Decline	Years	0.1	0.2	0.3	0.4	0.5	0.6	0.7
10%	5				1,800	1,120	800	580
	10				1,000	650	450	330
	20					415	290	210
25%	5		1,100	500	280	200	140	
	10		690	320	180	120	90	70
	20		380	170	95	65	45	35
50%	5	1,100	280	140	100			
	10	690	180	90	40			
	20	370	95	50	10			

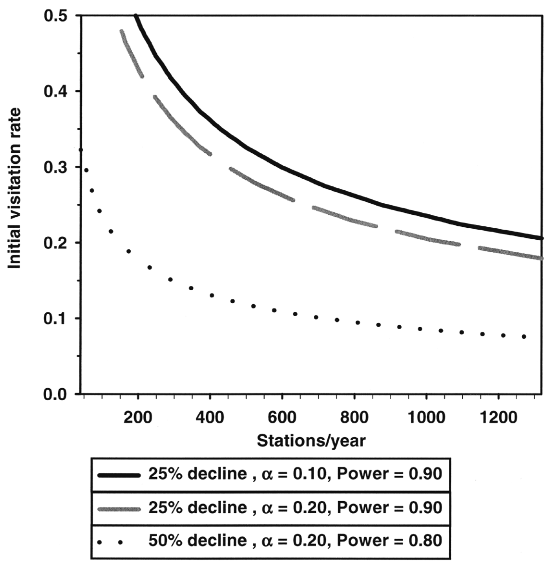

Fig. 2. Contour plot of power to detect declines in bait station visitation, 25% decline $\alpha = 0.10$, power = 0.90; 25% decline, $\alpha = 0.20$, power = 0.90; and 50% decline, $\alpha = 0.20$, power = 0.80.

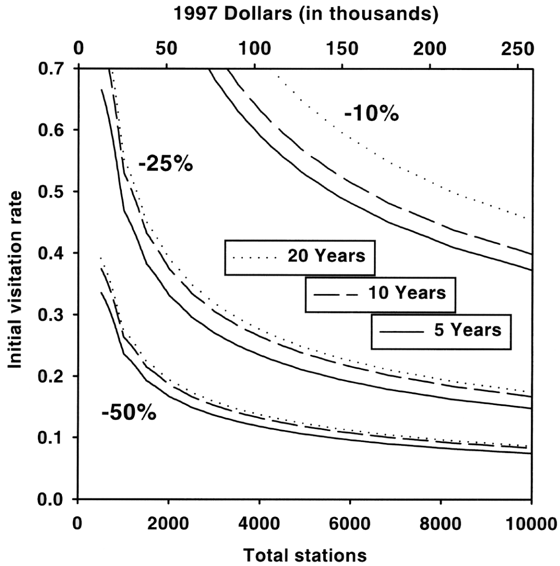

Fig. 3. Contour plot of 0.9 power for declines of 10%, 25%, and 50% over 5, 10, and 20 years by initial visitation rate, sample size, and approximate cost ($\alpha = 0.10$).

do so with an approximate doubling in sampling. To use bait stations for monitoring black bears in other areas in Idaho or in Washington, other strategies besides increasing sampling would be required: increasing the length of the analysis interval, adjusting our concept of relative change (see below), or achieving a higher visitation rate.

Increasing the length of the analysis interval is not a viable approach for areas with low visitation rates (Table 4). For those areas with a visitation rate <0.10, >370 stations would be required per management unit per year to detect even a 50% decline over 20 years, costing >$185,000. It is unlikely that this would be considered a cost-effective investment.

In considering our use of relative change, it is relevant to differentiate between 2 reasons for low visitation rates: low population density, or bears that are present do not visit the bait stations. If the population density is truly low (e.g., in areas which formerly had high visitation rates or other information indicates this — sightings, sign, harvest, nuisance and damage complaints), it may be sufficient to assess rates of change relative to a supposed or previously observed higher population density. For instance, if an area previously exhibited a visitation rate of 0.50 and currently has a rate of 0.25, the manager may judge it sufficient to detect a 50% decline based on the higher visitation rate. This would mean the manager could detect changes in population from high to medium (0.5 to 0.25), or medium to zero, but not medium to low (0.25 to 0.12). Thus, where long-term survey records exist, power may be acceptably assessed based on the maximum visitation rates observed, rather than the current visitation rate.

In areas where visitation is low but the population is thought, from other sources of information, to be substantial, a cost-effective monitoring program is only feasible if higher visitation rates can be achieved. Previous investigations have shown that visitation rate is affected by a number of factors: time of year, elevation, distance from roads and trails, interval between bait placement and checking, and the bait itself. Future efforts in western Washington will work toward achieving higher rates. We surmised that these low visitation rates were due to the failure of sardines to function as a long range attractant in the dense, moist forests in that area, so an option to remedy this is to use a more effective bait. Miller (1993) and Miller et al. (1995) found fresh fish to be a more effective bait than sardines (or meat) in Mississippi, and G. Koehler (Washington Department of Fish and Wildlife, Ocean Shores, Washington, USA, personal communication, 1998) achieved higher visitation in western Washington with fish emulsion fertilizer and raspberry extract. Another possible way to increase visitation rate is to increase the time before checking the station (Abler 1988, Miller 1993, Miller et al. 1995). Visitation is also strongly affected by the timing of the surveys due to the presumed relationship between bait station visitation and the availability of natural foods (Abler 1988, Miller 1993, Miller et al. 1995), and modifications in timing will also be tested. Because food availability is a function of plant phenology, which varies with weather conditions from year to year, consistently sampling in the optimum period is likely to be difficult to achieve. Documented site effects include LeCount's (1982) finding that bear visitation to scent stations was higher on a trail than a road (despite similar bear densities), Carlock's (1986) evidence that visitation to bait stations varied with type of road, van Manen's (1988) evidence of effects of distance from roads and trails, and Mantey and Immell's (1995, Influence of roads on black bear detections at bait stations, Department of Wildlife, Humboldt State University, Arcata, California, USA) finding that visitation increased with distance from roads (up to 100 m). Thus, placing bait stations along trails rather than roads, or >100 m from roads could be expected to increase visitation rate. However, because personnel time is the greatest component of the cost of running bait stations and these modifications would be more time consuming, they would likely increase costs substantially as well as increasing visitation rate.

CONCLUSIONS

In general, our evaluation indicates that using bait stations for monitoring black bear populations is feasible, but a substantial sample size is likely to be required. That is, large samples are required to overcome the variability introduced into bait station visitation rates due to extraneous factors. Less intensive sampling is likely to detect only large effects, and it is noteworthy that statistically significant differences reported to date have been large (i.e., change ≥42%; Carlock et al. 1983, Johnson 1990, Pitt and Jordan 1996, Powell et al. 1996), and large differences may not be statistically significant (change ≥39%; Carlock 1986, Miller 1993, Miller et al. 1995). Whether the costs incurred in meeting the sampling requirements are justified is a decision that must be made in the context of the requirements of each monitoring program.

Other approaches may also be appropriate for analysis of bait stations and estimating power of the surveys. Kendall et al. (1992) used a beta-binomial model as a basis for simulations to estimate power of track surveys, while Beier and Cunningham (1996) employed a Poisson model for simulations. Roughton and Sweeney (1982) advocated the use of the Fisher randomization test and the Wilcoxon signed rank test for analyzing scent station data. These approaches could be employed with bait-station surveys, although the Fisher randomization test or the Wilcoxon signed rank test are only suitable for comparing 2 rates, not assessing trend over several measurements. Sargeant et al. (1998) advocated the use of rank-transformed visitation rates for scent-station surveys, although this approach leaves the magnitude of the change in question. In addition, Thomas and Krebs (1997) reviewed statistical power analysis software.

Statistical power is calculated mathematically, and thus seems to be an objective means of assessing surveys. However, with the exception of sample size, all of the parameters used in calculating power incorporate a high level of subjectivity. To arrive at a power estimate, variability must be assessed, and deciding how to do this is subjective. Likewise, decisions on the minimum rate of change that needs to be detected are usually quite subjective, as are specifications of acceptable Type I (α) and II (1 - power) error rates. Because all these elements interact to provide the power estimate, the result, while having the appearance of numerical certainty, is highly subjective in nature and this should be borne in mind when interpreting the results. Our power analysis also presumes that surveys carried out in the future will exhibit the same variability as those done in the past.

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