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Mountain lion track. Photo by G. Andrejko, Arizona Game and Fish Department

Power of track surveys to detect changes in cougar populations

Paul Beier and Stanley C. Cunningham

Abstract Little is known about the ability, or statistical power, of track surveys to detect a change in abundance of cougars (*Puma concolor*). We examined monitoring schemes that would have 80% power to detect a 30% or 50% change in track abundance between 2 survey periods. We used data from track transects in southeastern Arizona to evaluate survey designs for 8-km transects in first- and second-order dry washes. Track density (number of 0.5-km segments with tracks along an 8-km transect) followed a Poisson distribution, with no serial correlation between consecutive surveys of a given transect. We used simulated Poisson data to determine how power varied in response to number of 8-km transects, risk of Type I error, direction of change (increase or decrease), magnitude of change (30% or 50%), and whether track density between surveys changed uniformly or patchily across transects. Power decreased only slightly when change in track density was patchy. Track transects had low power to detect increases in track density (e.g., about 190 transects would be needed to detect a 30% increase with 80% power and $\alpha = 0.05$), but somewhat more power to detect decreases (about 140 transects would detect a 30% decrease with 80% power at $\alpha = 0.05$). Managers can increase the power of surveys (or decrease the number of transects) if a 10–20% risk of Type I error is acceptable, i.e., about 140 transects would be needed to detect a 30% decrease in track density with 80% power at $\alpha = 0.05$, 110 transects at $\alpha = 0.10$, and 85 transects at $\alpha = 0.20$. If surveys need to detect only large decreases (50%), track surveys are more powerful, with only about 50 transects needed for 80% power at $\alpha = 0.05$, and 30 transects at $\alpha = 0.20$. Thus, track surveys usually will not detect small annual changes, but may reveal large changes more efficiently than other methods.

Key words cougar, mountain lion, Poisson distribution, population monitoring, *Puma concolor*, statistical power, track surveys

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Because cougars (*Puma concolor*) are nocturnal, secretive, and disperse at low densities, it is difficult to monitor changes in their populations. With intensive radiotelemetry, we can estimate the size of a population (Seidensticker et al. 1973, Van Dyke et al. 1986, Van Sickle and Lindzey 1992, Beier 1993, Cunningham et al. 1995), but this approach is expensive, requires handling a large proportion of a population, and yields an estimate for only a relatively small area. To index population trend over large areas at low cost, managers use hunter harvests, depredation rates, and track surveys. However, trend estimates from hunter kills and depredation rates are sensitive to hunting effort and reporting rates, respectively, and these parameters rarely are known. Furthermore, these 2 types of data yield only point estimates with unknown risk of Type I error (α, the risk of concluding that a population change occurred when it did not), unknown risk of Type II error (β, the risk of failing to detect a change that did occur), and unknown statistical power ($1 - \beta$, probability of correctly detecting a population change).

Track surveys may provide a better index than harvest or depredation data. Van Dyke et al. (1986) and Van Sickle and Lindzey (1992) reported positive correlation between track density and cougar population size. Tracking effort can be standardized easily. Smallwood and Fitzhugh (1992, 1995) and Smallwood (1994) suggested field methods for track surveys and used track surveys to index population trend in California. Similarly, Cunningham et al. (1995) used tracks to index cougar abundance in southeast Arizona.

We developed sampling methods that would allow managers to detect a 30-50% change in cougar abundance between 2 survey periods. We assumed that track density in an area was linearly related to number of cougars regardless of density (i.e., that the number of tracks/cougar/unit time is the same at high and low density). We used data from track surveys in southeastern Arizona to evaluate different survey designs. We analyzed how power varied with sample size, risk of Type I error, direction and magnitude of change, and pattern of change in track density (i.e., uniform or patchy).

Mountain lion. Photo by G. Andrejko, Arizona Game and Fish Department

Field survey methods and results

Cunningham et al. (1995) surveyed 28 dry washes on foot in the 4035-km^2 Aravaipa-Klondyke study area in southeastern Arizona during 4 sampling periods (Oct 1991, Apr 1992, Oct 1992, Apr 1993). Washes were chosen because tracks were identifiable only in dust, sand, or mud, which occurred mostly on dirt roads and washes; vehicular traffic and wind rapidly obliterated tracks on roads. Smallwood and Fitzhugh (1995) reported that first- and second-order washes had higher track densities than transects in other topographic settings. About half (13) of their transects were 8 km long; the others were 4.5-7.5 km long ($\bar{x}$ = 6.8 km). Because 5 precipitation-free days were required before starting a survey, not all transects were walked in each sampling period (Table 1).

On each transect, a single trained observer started at first light at the headwaters of the wash and walked downstream, completing the survey within about 4 hours. For each 0.5-km segment of a transect, the observer counted cougar scats and track sets (continuous set of tracks apparently made by 1 individual). The observer also estimated the number of different individuals detected in each segment and along the transect. In our analysis, we reduced these observations to 1 type of presence-absence data, i.e., whether each segment contained cougar tracks (excluding any track set which unambiguously extended from the previous segment). We counted total number of segments with tracks/8-km transect (for transects >8 km long, we used the number of tracks expected on an 8-km transect). We refer to this count as track density. We did not analyze numbers of track sets, numbers of scats, or estimated numbers of individuals for 2 reasons. First, presence-absence data were less susceptible to variation due to observer skills. Second, the most likely cause of a high count on a segment was that a cougar had a kill nearby; this high count would not reflect a larger number of animals.

The probability of track detection was not constant across each 0.5-km segment (Fig. 1). Tracks were rarely detected in the first kilometer of a transect because the uppermost portions of washes lacked a well-developed scour zone with suitable sandy substrates. In the lowermost portions of a transect, domestic cattle tracks tended to obliterate cougar tracks. Consequently, we could not model track density as a constant probability of track occurrence on all segments, as Kendall et al. (1992) did for sign of grizzly bears (*Ursus arctos horribilis*) and black bears (*U. americanus*).

On average, 7% of the 0.5-km segments contained cougar tracks (Table 1). Although there were more segments with tracks during the first survey period than during subsequent periods, only the difference between period 1 (12%) and period 2 (4%) was statistically significant (Wilcoxon matched-pairs signed-rank test, $P = 0.008$; matched-pairs *t*-test, $P = 0.007$).

Track density followed a Poisson distribution in each sampling period (Table 1) and across 78 transects in all sampling periods (Kolmogorov-Smirnov test, $P = 0.09$) and departed from a normal distribution in each sampling period (Table 1) and across all sampling periods (Kolmogorov-Smirnov test, $P < 0.0001$).

To investigate serial correlation of track occurrence on individual transects, we compared the proportion of each transect's segments that had tracks for each pair of consecutive surveys. These showed no correlation ($r^2 = 0.04$, $n = 51$ pairs). Therefore, in our simulations we assigned each transect a track density from a Poisson distribution with an appropriate mean, regardless of the track density simulated for any previous survey on that transect.

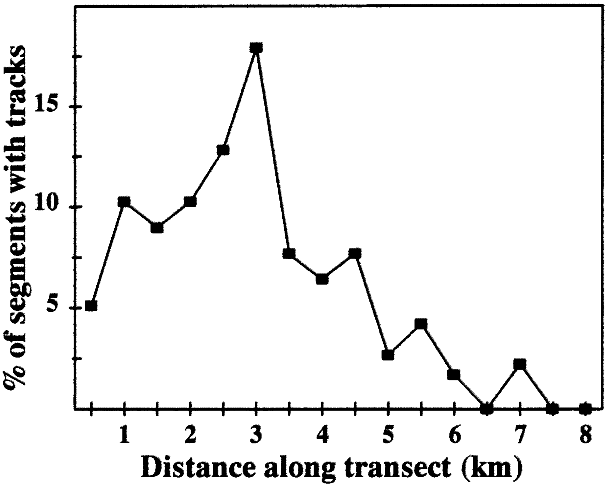

Fig. 1. Probability of track detection versus distance along dry stream washes during 78 transects for cougar tracks on the Aravaipa-Klondyke study area, Arizona, 1992–1993. Each 8-km transect was walked from high to low elevation.

Methods used to analyze survey power

Our field results indicated that track density followed a Poisson distribution and that track density on a transect was independent of previous counts on that transect. Therefore, we used a simple Poisson model to simulate observations of track density during 2 different years. We used our observed mean track density across all surveys (1.14) as the mean simulated track density for the first, or baseline survey. We assessed how power changed in response to 5 factors:

Sample size (n, number of 8-km transects). We simulated survey efforts from 30 to 270 transects in increments of 30 transects.

Risk of Type I error (α). When managing small threatened populations like those in southern California (Beier 1993) or Florida (Maehr 1990), the consequences of Type I error (incorrectly concluding that a population has declined) are less severe than Type II error (failure to detect a change). Therefore we determined power for 2-sided $\alpha = 0.1$ and 0.2, in addition to the conventional 0.05.

Magnitude of change. Because surveys designed to detect small increases (e.g., 10–20%) would be prohibitively expensive, and because management actions are unlikely unless larger changes are detected, we simulated track densities in the second survey that varied by 30% or 50% from those in the baseline survey.

Direction of change. Managers of a threatened bighorn sheep population subject to preda-

Table 1. Density of cougar tracks on 28 stream-bottom transects on the Aravaipa-Klondyke study area in southeastern Arizona during 4 sampling periods, period 1 (October 1991), period 2 (April 1992), period 3 (October 1992), and period 4 (April 1993).

		Track density[a]			
Transect	Length (km)	Period 1 $n = 19$	Period 2 $n = 25$	Period 3 $n = 17$	Period 4 $n = 17$
1	8	0	0	2	2
2	7.5	2	0	0	2
3	6.5	1	2	0	0
4	8	0	0	1	—
5	8	0	0	—	—
6	8	4	0	1	0
7	8	3	1	1	3
8	8	—[b]	1	1	0
9	8	2	1	—	0
10	8	5	0	—	—
11	8	2	0	2	0
12	5	2	0	—	2
13	6	—	1	3	1
14	5.5	—	1	—	0
15	5.5	—	0	—	0
16	6.5	1	2	—	2
17	8	0	0	1	1
18	7	1	0	0	0
19	8	—	—	0	—
21	5.5	6	1	6	3
23	5.5	3	—	0	—
25	8	—	0	0	—
26	6	0	0	—	—
27	5.5	—	—	0	—
28	4.5	0	0	—	0
29	5	—	0	—	—
30	8	—	4	1	—
31	6	4	1	—	—
P (fit normal)[c]		0.70	0.007	0.10	0.06
P (fit Poisson)[c]		0.67	1.00	0.99	0.90
$\bar{x}$	6.8	1.89	0.60	1.12	0.94
		(12%)	(4%)	(6%)	(7%)
SD		1.85	0.96	1.54	1.14

[a] Number of 0.5-km segments with tracks per 16-segment transect. For transects < 8 km long, we multiplied the proportion of segments with tracks by 16 to yield the number of segments expected on that route if the transect had been 8 km long.

[b] Transect was not run during this period.

[c] Probability that column values fit a normal or Poisson distribution (Kolmogorov-Smirnov test).

tion by cougars may be concerned with detecting an increase in a cougar population. In contrast, managers of a threatened cougar population may be more interested in detecting a decline. Because track surveys have greater power to detect a decrease than an increase in track density (the variance of a Poisson variable equals the mean), we simulated both increases and decreases (of 30% or 50%).

Patchiness of change in track density between the 2 simulated surveys. During a decline, cougars from low-quality habitat could shift their ranges to fill vacancies occurring in high-quality habitat. Thus track density might remain unchanged in the best habitat and decrease only in lower-quality habitat, increasing the standard deviation and lowering statistical power. Similar patchy changes during a population increase could reduce statistical power. Therefore we tested power under 2 conditions: (1) a uniform change in track density on all transects between the 2 survey periods, and (2) no change in track density on half of the transects and a 60% or 100% change in track density on the other half of the transects (to produce overall change of 30% or 50%, respectively).

For each combination of the 5 factors, we used Resampling Stats software (Simon 1995) to simulate 1,000 survey efforts, each consisting of a baseline survey (with mean track density 1.14, following a Poisson distribution) and a follow-up survey (with mean track density 30% or 50% lower or higher). We drew 500 independent samples (each of size n) from the pooled simulated observations ($2n$), tallied the number of samples whose mean was at least as extreme as the follow-up mean, and divided this tally by 500. This proportion is the exact 1-tailed significance level for a test of the hypothesis that the baseline and follow-up surveys reflect a population of constant size. We doubled this proportion to yield a 2-tailed P-value. Because this test makes no assumption about the distribution of the variates, it was appropriate for our simulated Poisson data and also for our simulations of patchy change in sign density. We then computed power as that fraction of the 1,000 computed significance levels that were less the specified α (0.05, 0.1, or 0.2). We used 80% as a minimally acceptable level of power.

In addition to simulating these combinations of conditions, we estimated the power of different sampling efforts to detect a non-patchy 10% decrease in track density at $\alpha = 0.20$. For this effort we simulated efforts of 300–1,000 transects in increments of 50 transects.

Simulation results

Power increased as number of transects increased (Fig. 2). For large (30–50%) changes, the number of transects needed to achieve 80% power varied from about 30 transects (to detect a 50% decrease with $\alpha = 0.20$) to about 190 transects (to detect a 30% increase with $\alpha = 0.05$). In simulations to detect a small (10%) decrease at $\alpha = 0.20$, 400 transects had only 48% power, and 700 transects were needed to achieve 80% power. Most power curves had an inflection point at 75–90% power; large increases in number of transects were necessary to make small marginal gains in power beyond that level.

By accepting an increased risk of Type I error, the power of a given survey effort could be increased, or alternatively, the number of transects needed to achieve 80% power could be reduced. The gain in efficiency in increasing α from 0.1 to 0.2 was greater than when increasing α from 0.05 to 0.1. For example, about 140 transects would be needed to be 80% confident of detecting a 30% decline at $\alpha = 0.05$, 120 transects for $\alpha = 0.1$, and 90 transects for $\alpha = 0.2$.

Power increased dramatically as the magnitude of change increased from 30% to 50%. For example, about 50 transects were needed to detect a 50% decline versus about 140 transects needed to detect a 30% decline, with 80% power at $\alpha = 0.05$. As expected, track surveys also had more power to detect a population decrease than an increase (Fig. 2).

However, power decreased by ≤2% when a change in track density was concentrated on half of the transects instead of occurring uniformly over all transects. Patchiness of change was the only factor that had a negligible impact on survey power, and therefore its impact is not illustrated in Fig. 2. Thus spatial patchiness in population increase or decrease will not compromise power in a survey effort that otherwise meets a manager's needs.

Discussion

We estimate that 30–190 transects would be needed to detect a 30–50% change in a cougar population on the basis of 8-km track transects in dry washes. Because 700 transects are necessary to detect a 10% decrease at $\alpha = 0.20$, we conclude that track surveys to detect small changes at any reasonable α would be prohibitively expensive. This supports Van Sickle and Lindzey (1992) in that a track index would detect only "relatively large changes in cougar population size" reliably. Similarly, Kendall et al. (1992) used a combination of field data and simulations to conclude that surveys using bear scat on trails would not detect small annual fluctuations in bear populations, but that such surveying on many long trails could detect substantial, potentially threatening declines. Smallwood and Fitzhugh (1995) recommended 44 quadrats, each with 2 or 3 transects (11.3 km long) for monitoring state-wide trend of cougar populations in California.

Although we found that considerable sampling effort is required for track surveys to detect large population changes, we believe that track surveys are better than the alternatives. Track surveys can

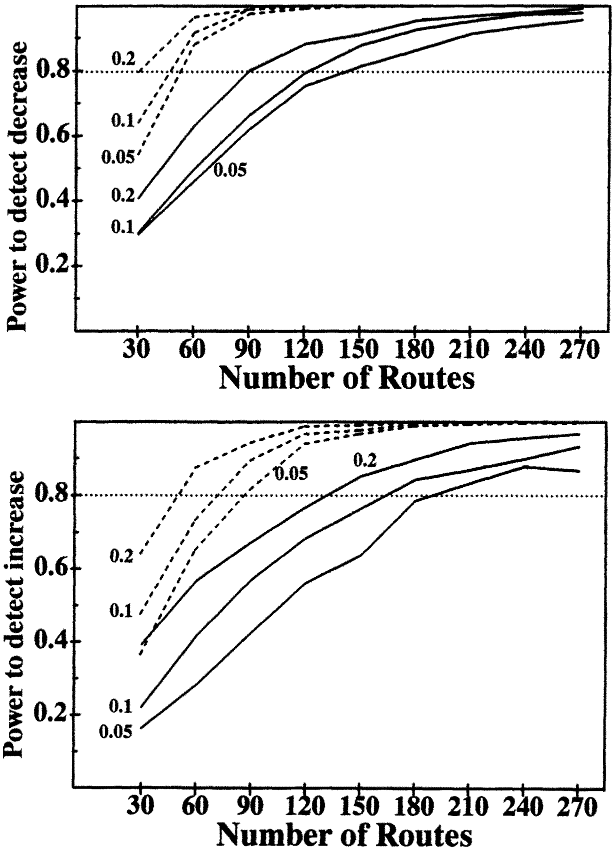

Fig. 2. Power of track surveys to detect decreases (upper panel) or increases (lower panel) in track density in relation to number of transects, magnitude of change, and risk of Type I error. In each panel, the dashed lines reflect a 50% change and the solid lines a 30% change in track density between the baseline and follow-up surveys. The label on each line indicates α (0.05, 0.10, or 0.20).

yield statistically valid inferences about population change, unlike indices based on hunter harvests or depredation data. Compared to capturing, marking, or other more direct measures, indices based on animal signs are inexpensive, easily replicated, and easily standardized among observers. Also, observing sign does not influence the target population.

We feel that 80% power is a reasonable goal for such surveys. A lower target would result in real changes in population going undetected. If a manager is willing to accept lower power, we question whether the survey has a purpose other than making the public think that the manager is "doing something." On the other hand, 80% approximates the inflection point on most of the power curves (Fig. 2). Increases in power beyond this point are achieved at high marginal cost.

The number of transects needed to detect a change with 80% power depends on the magnitude and direction of the change that a manager wishes to detect, and on the manager's willingness to accept Type I error. The specific management situation will determine how these parameters are chosen, but we offer some general conclusions. First, track surveys that are intended to promptly detect a population decline do not require as much effort as surveys intended to detect a population increase. Second, we believe that an $\alpha > 0.05$ is a reasonable choice in most realistic management situations. For threatened populations, an α of 0.10-0.20 is a better alternative (presuming that emergency steps would be taken if a decline is detected in time) because the potential consequence of Type II error is extinction. For hunted populations (where monitoring is designed to ensure that a population is not being overexploited), we favor an α of 0.05-0.1 because Type I error could lead to negative (and incorrect) public perception of a hunting program.

Unlike Kendall et al. (1992), we did not model the process of track deposition on each segment of a trail. Managers can thus readily test if their data meet our single assumption that track density per transect follows a Poisson distribution. If observed data depart significantly from a Poisson distribution, our estimates of power and required sampling effort will not be valid. Our results are independent of our starting density of 1.14 segments with tracks/transect and thus apply to any track counts that follow a Poisson distribution.

We did not expect the low correlation we found between consecutive observations of track density on a given trail. In cases where serial correlation is evident, a Wilcoxon matched-pair signed-rank test would yield greater statistical power. The Wilcoxon test should have no negative statistical consequences even with data like ours. We only made a simple comparison between baseline and follow-up periods. Time series analyses (e.g., Harris 1986, Gerrodette 1987) may have greater statistical power to detect trends, but only for long-term ($\geq$10-12 yrs) data sets and only when statistical assumptions are met (Kendall et al. 1992).

Managers wishing to use to track surveys should locate transects throughout an area used by a target population and should avoid the temptation to place transects only in areas expected to have abundant cougars. Without transects in marginal habitats (where increases and decreases in population often occur earliest and are most pronounced), an effort will have lower than expected power and will not detect change promptly. On the other hand, if survey transects are located in both superior and marginal habitat, our results show that power will not be compromised by the expected patchiness in population change. Because we modeled patchiness of an unrealistically extreme type (all change confined to half the transects), this conclusion should apply broadly. Although we advocate placing transects throughout an area occupied by a target population, we endorse the recommendation of Smallwood and Fitzhugh (1992, 1995) that transects be located where local cougars (of any size population) are most likely to travel and where their tracks are most likely to be visible.

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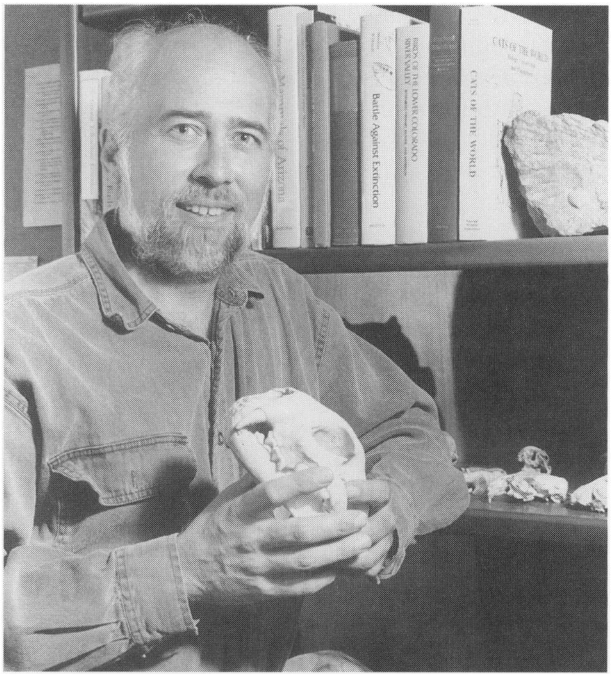

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