HISTORICAL GRIZZLY BEAR TRENDS IN GLACIER NATIONAL PARK, MONTANA: A RESPONSE

KIMBERLY A. KEATING

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Hayward (1989) argued that my analysis (Keating 1986) of historical grizzly bear (*Ursus arctos*) population trends in Glacier National Park (GNP) was flawed by interdependence between sighting rates and population estimates, and that trends were due to biases in data rather than an increase in population. Reanalyzing population estimates, Hayward found "no evidence" to support conclusions regarding grizzly population trends. Recommending "a sound sampling procedure" to "produce more reliable results," he implied that sound management must await more conclusive data.

I respond below, emphasizing 3 points: (1) a high degree of statistical interdependence was not self-evident, nor would it have affected the study's conclusions; (2) hypothesized biases could not account for observed trends in sighting data; and (3) the critique of population trends evinced misinterpretation and incomplete reading of the study, and inappropriately inferred quantitative relationships not supported by the data. Finally, I suggest that awaiting conclusive data is neither realistic nor prudent, that management strategies should be experimentally oriented, and that the hypothesis offered in Keating (1986) was a legitimate step in that direction.

STATISTICAL ANALYSES

Asserting that population estimates and sighting rates were ". . . based on essentially the same data . . . ," Hayward concluded that interdependence of variables invalidated the study's partial regression analysis. A high degree of interdependence cannot be assumed, however. Population estimates were based on grizzly bear sightings from all sources, rangers' intuitive judgments, and estimates from the previous year (Martinka 1971; R. Wasem, North Cascades Natl. Park, pers. commun.). Sighting rates reflected only sightings by rangers; they were objectively corrected for varying observer effort; and they were not influenced by previous sampling periods.

More importantly, rejecting the partial regression analysis would not affect the study's conclusions. Partial regression suggested that sighting rates did not increase according to the pattern that would be predicted if increases were due to habituation. The same conclusion also was derived (Keating 1986) from 3 lines of qualitative analyses unaffected by possible interdependence between sighting rates and population estimates: (1) visitation increased homogeneously over time, while sighting rates were characterized by long periods of relative stability punctuated by periods of rapid increase; (2) habituation could not explain increased sighting rates circa 1930; and (3) habituation could not explain similar increases in sighting rates in both remote and high-use areas.

SIGHTING DATA

Using sighting rates to index population trends entailed 2 assumptions (Keating 1986, Hayward 1989): (1) the proportion of unreported sightings was constant over time; and (2) if a bear was present in a given area, the probability of seeing that bear was constant over time. Hayward hypothesized that changes in vegetation, and rangers' travel routes and behavior violated these assumptions, biasing sighting rates upward. To accept Hayward's thesis, hypothesized biases must account for sighting rate trends, specifically, parkwide increases in sighting rates about 1930 and 1965 and an increase in mean observed group size about 1960. I examine the hypothesized biases below.

Vegetation Changes

Hayward's hypothesis that vegetation succession may have biased sighting rates by altering food distribution or vegetation structure was inconsistent with sighting rate trends and probable patterns of plant succession. The "food distribution" hypothesis was improbable. Consider that virtually all sightings used in calculating sighting rates were made from trails, that an increase in total food availability would likely have led to an increased bear population, and that Hayward argued against an increased bear population. To explain increased sighting rates under these constraints, Hayward's hypothesis requires a net increase in bear foods in areas near trails or visible from trails, and a compensatory decrease in foods in areas not visible from trails, so that total food availability either declined or remained constant. To explain punctuated, park-wide increases in sighting rates further requires that these nonrandom, compensatory shifts in food distribution were relatively sudden and ubiquitous. Such changes were highly improbable given differences in historic burn patterns throughout GNP (GNP, unpubl. data), relatively slow response rates of bear food species to natural fire disturbance (Martin 1983, Zager et al. 1983), and differences in response rates among sites (Martin 1983).

Furthermore, changes in vegetation structure were not a plausible explanation for sighting rate trends. Major fires in 1929 preceded an increase in sighting rates circa 1930, but could not account for increases on the east side of the park (GNP, unpubl. data). No major fires occurred in GNP between 1936 and 1967; thus, vegetation height and density would have been increasing (and visibility would have been declining) when sighting rates increased about 1965. Historic photographs (GNP, unpubl. data) supported this interpretation.

Changes in Rangers' Travel Routes and Behavior

Hayward suggested that, in 1967, 2 fatal maulings and Martinka's (1974) initiation of a grizzly bear study ". . . raised awareness of park rangers toward bears." He postulated that "raised awareness" biased sighting rates upward, specifically suggesting that rangers changed their travel routes over time ". . . such that areas with greater visibility, more bear problems, or higher bear numbers . . ." were traversed more frequently.

The historic record argued against this hypothesis. If violent confrontations between bears and humans biased sighting rates upward, then rates should have increased during the 1956–1960 or 1961–1965 sampling periods. Between 1956 and 1965, 10 people were mauled by grizzly bears in GNP; 2 maulings were life-threatening, and 3 were listed as serious (GNP, unpubl. data). Maulings in 1960 generated national publicity and a special act of Congress, while a 1961 settlement awarded $100,000 to the victim of an earlier mauling (Ruffin 1965). These events certainly would have raised awareness of park rangers toward bears, but the increased sighting rates predicted by Hayward were not observed. Furthermore, it is unlikely that the events of 1967 could have raised awareness by an additional increment sufficient to account for the approximately 5-fold increase in sighting rates between the 1950's and 1970's.

There was no evidence to support the suggestion that Martinka's (1974) study biased sighting rates. The study was insufficient in scope to influence park-wide sighting rates, because it covered only about 20% of the park and was confined to the park's northern half. Also, Martinka used only Wildlife Observation Cards to record sightings (C. Martinka, GNP, pers. commun.). Thus, his study created no incentive to increase reporting effort in ranger logs, which were the sole source of sighting rate data.

If sighting rates were biased by scientific studies, rates should have increased during the period of intensive census efforts between 1924 and 1939, and should have declined thereafter. Although sighting rates increased about 1930, rates remained generally low during the 1924–1939 period and did not decline as scientific interest faded.

If observer awareness increased, mean observed group size should have declined, assuming that a keen observer is more likely to detect lone bears. Contrary to this prediction, mean group size increased between the late 1950's and early 1960's and remained higher thereafter.

Finally, Hayward offered a poor explanation for the fact that sighting rates increased similarly in all areas of the park, despite intra-park differences in visibility, numbers of visitors, and numbers of bear problems.

Furthermore, at least 4 factors probably caused underestimated sighting rates, thereby mitigating biases that might have existed. First, reporting of grizzly bear sightings in ranger logs probably declined in the 1960's and 1970's as alternate reporting forms proliferated. In addition to Wildlife Observation Cards, the use of R-10 Wildlife

Observation Forms was initiated in the 1960's, followed by Case Incident Reports for backcountry patrols, and Bear Information Management System reports in the 1970's.

Second, sightings from the Many Glacier and St. Mary subdistricts were not included after 1959 because changes in the character of the ranger logs made them incomparable to earlier records. These areas had some of the highest visibilities and greatest numbers of grizzly-human confrontations—52% of the injuries and 33% of the fatalities through 1976 (Jope 1982). Thus, sighting rates for 1960–1976 were based on areas with lower than average visibility and numbers of bear problems.

Third, rangers of the 1920's and 1930's spent more time on horseback, which should have increased the likelihood of seeing grizzly bears (Jope 1982). Also, they were more likely to be on the trail during crepuscular hours when grizzly bears are more likely to be sighted.

Fourth, rangers were required to log 300 miles of backcountry travel per month before 1936. Consequently, they spent more time in the backcountry and, on average, were probably more practiced and astute observers than rangers of the post-war period.

Mean Group Size

I stated previously (Keating 1986:85) that an increase in mean observed group size intimated an increased grizzly population, because it "... suggested that family groups, as a proportion of the population, increased about 1960. This may have reflected increased survival to breeding age as a result of reduced mortality along the park's periphery." Group size data were independent of sighting rates and unaffected by the biases postulated by Hayward. Therefore, they provided independent evidence of an increased grizzly bear population about 1960, although an alternate hypothesis merits discussion.

Theoretically, if the population were declining from a relatively high initial density, density-dependent responses in recruitment (McCullough 1981) may have caused increased group sizes. However, this hypothesis was difficult to reconcile with predator control and sheep ranching, which were extensive in the first half of the century and declined during the 1950's and 1960's. Because these activities were historically associated with high bear mortality, the pattern of their decline argued against both the high initial density and the declining population trend required by this hypothesis. Population growth remained the most plausible explanation for the increase in mean group size. Hayward failed to consider group size data before concluding that there was "no evidence" to suggest a population increase.

POPULATION TRENDS

Hayward stated that the magnitude of the hypothesized population increase was "... implied by the tie between GNP's [population] estimates and sighting rates which increased [exponentially] at 5.2%/year." He noted that population estimates actually increased in a biologically unrealistic manner from 100 in 1966 to 175 in 1967. According to Hayward, I argued "... that the trend represented a real change in bear numbers and offered no explanation for the timing or magnitude ..." of the increase.

I did not conclude that the population increased at 5.2%/year or that estimates accurately described a real population increase. Population growth rates cannot be inferred from sighting data because the relationship between population estimates and sighting rates is unknown. Estimates for 1966–1967 clearly suggested unrealistic population growth, which can only be attributed to a change in methodology. I did argue that sighting data indicated real population growth had occurred concurrent with, or somewhat before, methodological changes. This interpretation addressed only the direction, timing, and probable causes of population change, not the magnitude of that change.

An explanation for the timing of population increases was offered (Keating 1986). Increases in sighting rates and population estimates, circa 1930, correlated closely with the discontinuance of predator control in national parks in 1931. Increases in sighting rates, mean group size, and population estimates, circa 1960 and 1965, correlated with the decline of sheep ranching and predator control adjacent to GNP. Hayward supported this interpretation when he stated that he had "... little doubt that the GNP grizzly bear population has increased since the early 1900's when control of bears was thorough ...." He failed to note, however, that control of bears adjacent to GNP apparently remained "thorough" until after 1950.

MANAGEMENT IMPLICATIONS

Hayward concluded that statistically sound sampling procedures were needed to provide reliable data, and implied that sound management must be predicated upon such data. In part, I concur. Improved sampling procedures are desirable and deserve greater attention.

However, predicating management on conclusive data equates to the extreme of linear comprehensiveness, which requires clear definition of goals, and "comprehensive, quantitative, and continuous knowledge of the system" (Bailey 1982). Bailey (1982) noted that biotic complexity, difficulties in measuring and interpreting data, meager funding, and conflicting goals make the application of linear comprehensiveness to wildlife management especially difficult. Given the complex natural and political environment surrounding grizzly bear management in GNP, Bailey's comments suggest that linear-comprehensive management is particularly untenable in this case. Trends also suggest that a linear-comprehensive approach would be imprudent. Since 1939, 45 people have been mauled—8 fatally—by grizzly bears in GNP (GNP, unpubl. data). Martinka (1982) demonstrated that confrontation rates and numbers of bear removals were both increasing linearly by 0.3 incidents/year, indicating that risks to both visitors and bears are becoming more acute. Effective solutions will not await conclusive data.

An experimental approach is more realistic. Experimental management equates to the extreme of cyclic incrementalism, which requires only limited knowledge of the system and incomplete definition of goals; management is then ". . . designed to enhance accumulation of meaningful, local information on responses of populations, habitats, and the public to treatments" (Bailey 1982). Findings detailed in Keating (1986) were consistent with this approach.

Although Hayward labeled them "conclusions," findings of my study (Keating 1986) were, in fact, phrased as a hypothesis. The distinction is an implicit argument for management backed by experimental design, an argument that acknowledges uncertainties in the data, yet suggests that they constitute a reasonable and legitimate basis for exploring management alternatives. Toward that end, a more powerful hypothesis would be welcomed, but has yet to be offered.

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KIMBERLY A. KEATING, Research Division, Glacier National Park, West Glacier, MT 59936.

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