

Biological Problems With Fortymile Wolf Control

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Background

In 1992, the Alaska Department of Fish and Game (ADF&G 1992) proposed wolf control for the 15,000-square-mile Fortymile region of eastcentral Alaska, to generate a Fortymile caribou increase from 21,884+ to 35,000-60,000 (3 options) within 7-12 years. However, this plan, which was to be carried out by aerial shotgunning, was canceled in late 1992 before it began, primarily because of a strong national, international, and statewide public outcry and threatened tourism boycott. It was also strongly criticized on biological/scientific grounds (Haber 1992).

ADF&G nonetheless proceeded with a related wolf control effort in nearby Game Management Unit 20A in 1993-1994 (ADF&G 1992), again despite scientific opposition (e.g., Haber 1992, 1993, 1997). There was less initial public opposition to this effort because ADF&G convinced Alaskans and others that the primary killing method, saturation snaring, would be an acceptable alternative to aerial shotgunning. In late 1994, I videotaped the details at one of ADF&G's GMU 20A wolf snaring sites, and within days this tape was aired worldwide. The ensuing worldwide outcry, including by the ADF&G commissioner and Alaska governor-elect, resulted in a shutdown of the 20A control program over the next few days. The Alaska State Troopers investigated the way ADF&G had been doing the killing. In February 1995, the governor released the resulting, highly critical Department of Public Safety report and announced he was permanently canceling the 20A wolf control program.

Meanwhile, ADF&G was not giving up on its Fortymile vision. It assembled a Fortymile Planning Team consisting primarily of nonscientists and heavily of individuals and organizations with a history of advocating/favoring Fortymile wolf control. It provided the Team with selected background information and guided it through a process culminating in the public release of two reports in September-October 1995 (ADF&G 1995a, 1995b), collectively referred to as the Fortymile Caribou Management/Recovery Plan or (here) "the Plan." The Alaska Board of Game later provided regulatory authority for implementing the Plan, in 5AAC.92.125(4).

The Plan concluded there was a Fortymile caribou problem justifying wolf control. It, and 5ACC.92.125(4)(A), established a revised (vis-a-vis 1992) growth objective for the Fortymile herd - a 5-10% annual increase and 28,000-36,000 caribou by spring 2001. The Plan and

subsequent ADF&G comments, reports, updates, etc. emphasized that Fortymile wolf control would be carried out by nonlethal methods, specifically sterilization of alpha pairs and relocation of all other wolves. The Plan and ADF&G (repeatedly since 1995) have referred to this as a new era of “kinder, gentler nonlethal” wolf control with more respect for wolves and more sensitivity to other values. However the Plan also indicated that private wolf trappers would be encouraged to redirect their efforts into this area, to kill Fortymile wolves prior to the sterilizations and relocations. In winters 1995-96 and 1996-97, ADF&G helped to facilitate a private, \$400 per wolf bounty program organized by the Alaska Trappers Association and related groups specifically as a Fortymile wolf control measure. Approximately half the wolves present in the control area had been killed by the end of winter 1996-97; fewer than 50-80 remained. In November 1997, ADF&G began sterilizing and relocating the survivors and their new (May) pups; these activities continue at present (Feb. 1999).

In the following sections, I consider three basic scientific/biological questions that remain despite the Plan, 5AAC.92.125(4), and other ADF&G reports and comments: Is there really a Fortymile caribou problem? If there is a problem for which a Fortymile caribou increase is the solution, is the current wolf control effort needed to ensure the increase? Can the current wolf control effort be measured accurately, i.e., to evaluate its specific role, if any, in generating a caribou increase? Not addressed here are other lingering questions about the long-term behavioral, genetic, and related impacts of control on Fortymile wolves, per details in Haber (1996).

Is there a Fortymile caribou problem?

I addressed this question in Haber (1997), from which the following summary is largely derived:

ADF&G's basic premise is that Fortymile caribou declines from more than 500,000 in the 1920s and again from 40,000-60,000 in the 1950s, ultimately to a low of about 6,500 in 1973, resulted from an unnatural series of events, namely, excessive hunting and predation. ADF&G claims that the higher numbers and use of a larger (Alaska-Yukon) range area in the 1920s had lasted for “thousands of years,” and that by restoring a semblance of this earlier status wolf control would merely convert an unnatural biological situation back to a natural one, rectifying what ADF&G considers to be a fundamental problem. ADF&G has emphasized that the lower caribou numbers and more limited range of recent decades have also resulted in

something unnatural about the region's predator/scavenger complements and overall biodiversity, and that wolf control would similarly right this wrong.

Thus, according to ADF&G, the Fortymile herd's size at the time the Plan was released in 1995, i.e., 22,558+, and since then – the estimate was 31,000 as of July 1998, warrants five years of remedial wolf control. This is despite the fact that these numbers represent a 4-5-fold increase since the 1973 low.

There is no such Fortymile problem at present, and no evidence of the unnatural biological consequences ADF&G has alleged in the Plan and other promotions for Fortymile wolf control. This is a myth, which ADF&G has been able to sell by misrepresenting what is known about the history of Fortymile caribou.

There is no dispute that the aforementioned Fortymile caribou declines occurred. However they were most likely triggered by *natural dispersals* to other regions, especially to the north, not excessive hunting, unusually heavy predation, a combination of the two, or anything else unnatural. Hunting and predation certainly contributed, especially in the latter stages of both declines, but there is no evidence the declines would have occurred solely because of these two factors. The lower numbers and more limited distribution of Fortymile caribou over recent decades represent just as natural of a biological state as the higher numbers and larger ranges observed earlier in the century. The overall, highly dynamic pattern of change spanning periods of many more decades – the prolonged lows and shifting ranges as well as the highs – is what is natural, not just one or another of these phases, however favored of a snapshot it might be to certain user groups. It is crucial to remember that ADF&G sold the Plan to Alaskans and others in 1995 *as a way to restore natural patterns* of the Fortymile region, not simply to produce more caribou.

Contrary to ADF&G's claims, there is nothing in the historical record to indicate that Fortymile caribou maintained high numbers and the same large Alaska-Yukon range for "thousands of years" prior to the post-1920s declines. In the late 1800s, for example, when the Fortymile herd was larger than it is now, its movements and distribution were primarily within the eastern half of its "historic" range, i.e., *east* of the Alaska-Yukon border. At the turn of the century it still had relatively little presence in Alaska, whether because of its movements, size, or both. It was only after it increased toward the huge 1920s peak of at least 500,000 (probably closer to a million) that its movements and distribution once again shifted well westward into Alaska. During this period of increase, which was most likely triggered by large influxes of caribou from the northeast, there were far-reaching seasonal migrations in many directions. For at least 6-7 years between 1918 and 1931, these migrations even extended southward across the

Alaska Range all the way to Copper Center and wintering grounds in the Nelchina Basin, completely outside the area ADF&G describes as the historic Fortymile range.

In other words, there is no single “traditional” or “historic” Fortymile herd size or range to “restore.” These terms are inapplicable here. Like all major caribou herds, the Fortymile herd has fluctuated widely in its size and movements/distribution over the decades with and without heavy hunting or wolf control, in large measure because of shifts and dispersals to/from other regions. Short of building fences to suppress the inherent vagility of this species, ADF&G’s visions of relatively high, stable Fortymile caribou numbers, harvests, and predictable migrations across all or even most of the Alaska-Yukon areas used in the past are almost certain to be unattainable except for short periods, regardless of wolf abundance, and would be anything but natural.

In continuing to advance these objectives, ADF&G ignores a large body of scientific thinking and advice to the contrary, including from the National Academy of Sciences (National Research Council 1997) and at least seven other prominent wolf-ungulate scientists (see Haber 1997), including two former ADF&G commissioners (Skoog and Brooks) and two outside biologists ADF&G itself commissioned for a review. ADF&G’s own Fortymile Plan (ADF&G 1995a: 1; ADF&G 1995b: 16) challenges the notion that it should be possible to maintain anything close to the high Fortymile numbers of the 1920s, while ADF&G nonetheless continues to refer to the 1920s high in portraying the current numbers as unnatural and deficient.

Perhaps most important, the known history of Fortymile caribou illustrates how unrealistic it is for ADF&G and the Plan to claim that numbers and movements can be controlled on a local (“herd”) basis. Fortymile caribou do not even behave as a single group. For much of the year they are often split into at least two major segments, separated by at least 50-100 miles. The history of other heavily managed caribou, notably the Nelchina herd, which currently winters primarily within the Fortymile range, and the Western Arctic and Mulchatna herds, which now range across most of the western half of Alaska, illustrates the same.

ADF&G continues to rely on the discrete (single) caribou herd management thinking despite its many problems, with increasingly bizarre results: ADF&G is going to extremes to try to sharply *reduce* the numbers and movements of Nelchina caribou, while doing likewise to increase the numbers and movements of Fortymile caribou. But now major segments of the two “herds” are mixing, indistinguishably, for about half of each year in the Fortymile region (just as they mixed in the Nelchina range in the late teens and 1920s), with heavy hunting allowed temporarily for Nelchina but not Fortymile caribou. Fortymile wolves that are likely associating more

with the Nelchina caribou ADF&G wants to reduce are nonetheless targeted for reduction to save Fortymile caribou.

ADF&G insists that one of the underpinnings of its Fortymile wolf control plan and statewide caribou management in general - the idea that caribou can be maintained more-or-less indefinitely in relatively large, discrete herds - is supported by observations of traditional fidelity to calving grounds, if not to winter ranges. This is another longstanding myth of wildlife biology. Caribou calving grounds can and do change dramatically, as has happened several times over recent years in the Fortymile region. In any case it is on the highly varying, inter-mixing *winter* ranges where ADF&G must implement most of the caribou hunting-wolf control nuts and bolts measures arising from its single (discrete)-herd thinking.

The components of ecological and other nonlinear systems typically fluctuate widely under natural conditions, at differing scales of space and time, whether the systems are in chaotic, oscillatory, or “stable” states. Whatever related changes these fluctuations imply, including in species diversity and population sizes, are “natural.” Suppressing such changes – as ADF&G would like to do in the Fortymile by eliminating the entire lower range of variation in caribou and predator/scavenger numbers and containing the caribou within a stable, predictable range – would amount to wildlife farming, not the conservation or restoration of something natural that ADF&G touts as its objective.

This also provides a textbook example of what Holling and Meffe (1996) refer to as “command and control” resource management, which, they warn, is likely to result in diminished resilience of resource systems to natural and human-caused disruptions and more management surprises and crises, ultimately with collapsing resources, increased social and economic strife, and unnatural losses of biodiversity. I would add another consequence – the unwitting degradation of some cherished features of our surroundings with subtle but essential connections to the swings in abundance and other variations that we eliminate. Current fisheries problems, especially involving salmon stocks, may well hint at what ultimately can be expected for caribou as ADF&G proceeds along much the same (parochial, “active management”) pathway, seemingly oblivious to the broader, underlying patterns of system behavior.

More than a million caribou inhabit Alaska at present, probably rivaling or exceeding any statewide population high for at least 150 years, and the numbers are increasing. There is a reasonable, adaptive way to exploit these caribou, representing the true sustained yield management approach that the state constitution mandates. Holling and Meffe (1996: 334) stressed the importance of exploiting systems, including wildlife systems, in ways that:

“... retain critical types and ranges of natural variation ... [to] facilitate existing processes and variabilities rather than changing or controlling them.”

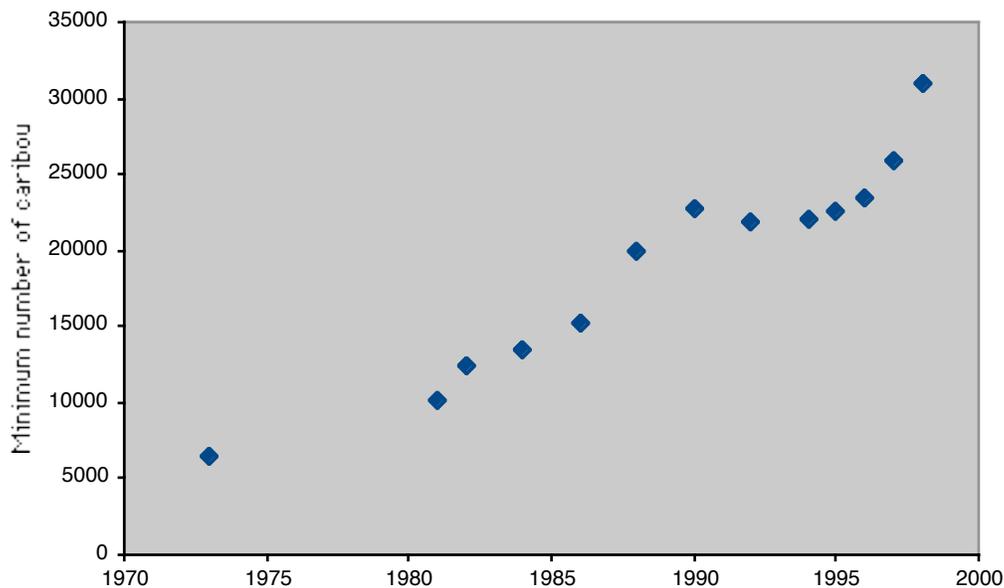
Contemporary hunters need only shift their emphasis – at a scale of decades rather than years – with the shifting centers of caribou abundance, which currently are located primarily in westward and northward areas of the state. This would merely be doing what their nomadic predecessors did successfully for at least 8,000 years before the mid 1900s with far less technology and no wolf control. It is the *statewide* caribou population that lends itself to sustainable hunting, not whatever groupings that happen to be present on each of the local range areas – like the Fortymile – from time to time.

Is wolf control needed to ensure a Fortymile caribou increase?

Even if there was a problem for which the current Fortymile caribou increase is the solution, this increase almost certainly would have occurred without wolf control.

Figure 1 shows the trend in Fortymile caribou numbers since the 1973 low of 6,500, based on all available ADF&G census estimates (ADF&G 1995b: 5, 17; Boertje et al. 1995:22;

Figure 1. Fortymile caribou trend, 1973-1998



Boertje and Gardner 1996; D. Reed, pers. commun.). It is important to understand that these are only *minimum* estimates that are subject to an unknown, varying degree of error from year-to-year. Hence there is no way to reliably interpret relatively small, short term differences. Where such estimates appear to indicate stability, an increase or decrease is just as likely, and what appears to be an increase could actually be a decrease and vice versa. This is because of the nature of the censusing procedure, which is based on June-July aerial searches using photography and radio tracking. Radio collars are used to locate the major aggregations, and these

are photographed for later counting. Other areas are searched visually, in attempting to locate caribou not associated with any radio collars.

The problem is that caribou, including resting groups of several hundred individuals each, can be extremely difficult to find and see from the air at this time of the year, given their non-random distribution, the large, mountainous areas and abundance of camouflaging terrain and vegetation (e.g., talus slopes and willows). It would be difficult enough to find all significant groups of caribou if the radio collars were distributed proportionately between the sexes. However, few if any collars are placed on adult bulls, which typically constitute 15-20% of all caribou present at that time of the year and are more widely dispersed in different areas, well away from the cow-calf bands. Year-to-year variability in migrations, flying weather, search patterns, search effort, pilot experience, and other factors further affect the census results and biologists' ability to determine *how much* the error varies from year-to-year. Only a few hundred caribou might be missed one year but a thousand or more could be missed the next.

Thus about all that can be reliably concluded from the census data (Fig. 1) is that there has been a *general*, approximately 5-fold increase over the past 20-25 years. ADF&G instead implies the Fortymile estimates are good enough to confirm that numbers "stagnated" in the early 1990s. It justifies the present wolf control effort with this argument, emphasizing to the media that control is needed specifically so that "reproduction can begin outpacing predation" (e.g., D. Reed, KTUU-TV, 12/1/97). Then it applies a different standard to the 1995 estimate, which when interpreted the same literal way as the 1990-1994 estimates indicates an increase of 2% (from 22,104 in 1994) and argues against any immediate need for control. I emphasize that none of these estimates is good enough for reliable interpretation of such small annual differences, but ADF&G should at least be consistent.

The earlier estimates (Fig. 1) similarly call into question ADF&G's arguments about needing wolf control for a caribou increase. Fortymile wolves were at or near natural levels of abundance during all but four years (1982-1985) of the previous 10-15-year period of caribou increase (Gasaway et al. 1992; Haber 1992).

Wolf control, i.e., via the ADF&G-facilitated private bounty program, began in winter 1995-96. The next caribou census – June-July 1996 – resulted in an estimate about 4% higher than in 1995. However, it is doubtful that this first winter of wolf control had much to do with whatever 1995 to 1996 increase may have occurred, primarily because the killing did not begin in a major way until March 1996 (I monitored these activities closely throughout the winter, via aerial observation). In other words it did not have much chance to influence the survival of 1995

caribou calves to yearling age (to become 1996 recruits in May). Nor did I see any indication of a change in the rate at which older caribou were killed.

My October caribou composition surveys indicated virtually the same overall calf percentages in 1995 and 1996 (8.9-10.7% [n=1558] and 10.1% [n=1054]), and both of these were lower than I observed in 1994 (12.0-14.3%). ADF&G's radio collar samples of annual mortality for older caribou remained essentially stable throughout this period (Boertje et al. 1995; Boertje and Gardner 1996: 25). Although these data likewise suggest only slight changes, they would not necessarily reflect a shift of caribou from another region (e.g., Nelchina), and in any case even a slight upward trend would have justified at least a delay in the wolf control effort.

The census estimates for 1997 and 1998 indicate increases that the recruitment and mortality variables – through which wolf predation must operate – cannot explain. The estimate for 1997 was 26,000, 11% higher than for 1996. According to ADF&G's data and what generally prevails for ungulates, about 12% of the Fortymile adult and yearling caribou combined died due to natural causes from May 1996 through April 1997. Somewhat less than 1% more were killed by hunters. Just after birth in May-early June 1996, about 65% of the total caribou present were yearlings (1996 recruits) and adults combined and 35% were new calves (ADF&G observed a 97% pregnancy rate in 1996; Boertje and Gardner 1998). With 13% of the yearlings+adults dying between May 1996 and April 1997, about 24% of the 1996 calves had to survive to yearling age (becoming 1997 recruits) for total numbers to remain stable over that 12-month period ($.24 \times .35 = .13 \times .65$).

My October 1996 composition survey indicated that about 21% had survived up to that point (10.1% of all caribou surveyed were calves). There were no later surveys by myself or ADF&G with which to determine how much further the calf percentage decreased during the second half of the biological year (November-April). For caribou and most other ungulates this percentage usually decreases sharply over the first few months of the biological year and then gradually. So it is safe to conclude that 21% in October at best would have meant somewhat fewer recruits in April than were necessary simply to keep numbers stable. And the entire 11% increase in total numbers estimated from the 1997 census would require other explanations, not related to anything wolf predation is likely to influence.

ADF&G's fall (1996) composition survey (Boertje and Gardner 1998; D Reed, pers. commun.), which is typically carried out in September, a month earlier than mine, suggested that about twice as many calves had survived until then (20.3% of all caribou surveyed were calves). Some of the discrepancy between ADF&G's fall 1996 calf ratio and mine (20.3 vs. 10.1%) can be explained by the earlier timing of ADF&G's survey, when mixing between the

sexes (for rutting) is sometimes less complete and there has been less chance for calves to die. However more of it is probably due to sampling error from two sources. ADF&G tries to distinguish among multiple classes (adult cows vs. adult bulls vs. long yearlings vs. calves) and uses a helicopter. In contrast to the response from a small fixed wing aircraft, a helicopter typically causes caribou (and other wildlife) to flee in panic and makes counting and classifying much more difficult, particularly since most of the caribou are in groups. In my surveys I attempt to distinguish only between calves and all other classes combined and use a small fixed wing aircraft (Super Cub). Nonetheless, given the certainty of additional (October-April) mortality, not even ADF&G's September calf ratio would translate into enough of an overall increase to explain more than about half the difference between the 1996 and 1997 census estimates.

The census estimate for 1998 was 31,000, 19% higher than for 1997. With about the same (13%) adult+yearling mortality from May 1997 through April 1998 as during the previous biological year and May-early June 1997 at-birth adult+yearling and calf percentages of 69% and 31% (85% pregnancy rate; Boertje and Gardner 1998), about 29% of the 1997 calves had to survive through April 1998 for total numbers to remain stable over that 12-month period ($.29 \times .31 = .0919$). I was not able to do a fall composition survey in 1997. However ADF&G's September 1997 composition survey (41 calves/44 yrings/40 adult bulls/100 adult cows [n=6196]; D. Reed, pers. commun.) - the last one available for the biological year - indicated about 48% of the 1997 calves had survived up to that point (18.2% of all caribou surveyed were calves). Given that seven months still remained in the biological year and the possibility of an overestimate of the calf ratio (as in 1996, per above), at best this ratio would have produced less than a 9% increase in the 1997 to 1998 census estimates, rather than the 19% ADF&G reported. Again, this requires explanations not related to anything wolf predation is likely to influence.

The pregnancy rate *decreased* in 1997, to 85%, so this doesn't help in explaining the 11% 1996 vs. 1997 census difference; it *adds* to the difficulty of explaining it with any wolf-related recruitment and mortality results. On the other hand the pregnancy rate increased to 98% in 1998 (D. Reed, pers. commun.) and therefore does help to explain the 1997 vs. 1998 difference, though only by about 2% more. The extra calf births in combination with the 1998 recruits (surviving 1997 calves) raise the explainable increase from *at-birth* 1997 to 1998 to about 11%, still well short of the 19% difference between the 1997 and 1998 estimates. Census error remains a likelihood, as always. But the most important candidate (in combination with census error) for explaining up to two-thirds of the overall 32% increase in the census estimates from 1996 to 1998 is a shift of caribou from the Nelchina herd.

As in other recent years (Haber 1997), most of the 35,000-40,000+ Nelchina caribou, from ~200 miles southwestward, wintered within the heart of the Fortymile range during winters 1996-97 and 1997-98, as far northward as the Chicken-Eagle area. ADF&G observed this same obvious use of the core Fortymile range by Nelchina caribou as I did but conspicuously has said next to nothing about it in public. Significant numbers of Nelchina caribou may not have returned southwestward to the “traditional” calving area in the spring. I heard at least several Nelchina radio collars in the Fortymile range during May-September. ADF&G has observed groups of up to 192 caribou without any functioning radio collars in the heavily collared Delta herd and I have observed up to 350 in the heavily collared Fortymile herd (Haber 1997). ADF&G’s June 1997 Nelchina census indicated “a decline of over 5,000 adult caribou [from 1996] that cannot be explained” (ADF&G 1997). Even with an adjustment for a 5% census error (ADF&G 1998), this still means several thousand “missing” Nelchina caribou. These and other results leave a Nelchina shift in 1996-97 and/or 1997-98 as a good possibility despite ADF&G’s frequent claims that there is no radio collar evidence of any shifts.

ADF&G relies heavily (e.g., Boertje and Gardner 1996: 78-79, 1998: 35-37) on annual samples of 50-60 radio collared newborn caribou calves, i.e., “neonates,” in reaching conclusions about the 12-month survival rate of calves (recruitment), the causes of calf deaths, the way these causes change relative to each other as control progresses from year-to-year, and the accuracy of its censuses. Its calculations (“models”) about annual changes in total numbers feature these neonate samples (no. of collared calves dying/no. that were collared) despite the availability of calf ratios from the much larger fall composition samples (n=thousands vs. 50-60) and even though the latter represent what happened over the first 5-6 months of the biological year instead of just the first four (most of the neonate samples are monitored only through August). ADF&G itself recognizes that the fall composition surveys are better than the neonate samples for estimating how many calves survive to become recruits. It pointed this out as recently as November 1998 in its widely distributed news bulletin about the status of Fortymile caribou (“The Comeback Trail,” Nov. 1998, p. 1):

“By radiocollaring at least 70 [sic] calves per year we can determine the types of mortality occurring. The [fall] *composition survey* gives us the best estimate of how many calves are being *recruited* into the herd each year.” (emphasis added).

There are several problems with using the neonate samples even to determine the types of mortality. They can potentially identify only the relative *proximate* causes of mortality. They provide little insight as to how many of the dead neonates would have died soon afterward anyway, from other causes, including due to other predators (e.g., heavier eagle predation may

follow lower wolf and bear predation - Haber 1995:18; heavier bear predation may follow lower wolf predation - Haber 1992:36-37).

“Potentially” should be emphasized, because of the subjectivity involved in assigning specific (proximate) causes of death. Few if any deaths are observed directly. It is usually not possible to examine calf mortalities until at least hours, days, or even weeks after death (Boertje et al 1995; Boertje and Gardner 1996), by which time often little, if anything, of the calf carcass remains, especially for the youngest calves. For example, on June 19, 1996, my pilot and I circled overhead as a single wolf that had just killed a one-month-old caribou calf consumed more than half the carcass in less than 10 minutes. The wolf was rapidly consuming the rest as we left. A bear typically eats a calf carcass at least as fast as this.

Thus ADF&G relies heavily on tracks, scats, hair, hide remnants, condition of the radio collar, and the like (Boertje et al 1995; Boertje and Gardner 1996; Adams et al 1989). All of these indicators can be easily misinterpreted, particularly since the calving areas are magnets for at least several kinds of predators and scavengers (notably wolves, bears, golden eagles, ravens, and wolverines) that sometimes compete at the same carcass, which may not have been a predator kill in the first place. For a sample size of only 50-60 calves and with a half dozen or so potential causes of death (non-predator as well as 3-4+ kinds of predators), it would take only a relative few misinterpretations to significantly distort the results and conclusions as to proximate causes of death and how these change with control, even without any bias toward showing that certain causes are more important than others.

In summary, there is no clear evidence that wolf control was needed for a major Fortymile caribou increase in the past or that it is needed for a continuing major increase now. Most of the available census, recruitment, mortality, and related information is to the contrary, indicating that wolf control has contributed only in minor ways if at all. There has been a general, ~5-fold caribou increase since the 1970s, most of which occurred during periods of natural or near natural wolf abundance. If numbers “stagnated” in the early 1990s, it is just as likely that an increase had already resumed before wolf control began in 1996. A major increase almost certainly would have continued through at least 1998 without control.

The specific growth objectives of the Plan – a 5-10% annual increase and 28,000-36,000 caribou by spring 2001 (ADF&G 1995b; Boertje and Gardner 1996: 59, 67; 5AAC 92.125(4)(A) – have been met, at least based on the results ADF&G continues to emphasize for other purposes in the Fortymile debate. Apart from the questions about why control began, by

virtue of ADF&G's own latest numbers, the objectives specified in the Plan, and the Plan's regulatory authority it is inconsistent for ADF&G to argue that control should continue.

Can the results of Fortymile wolf control be measured accurately?

The most basic recommendation of the National Research Council (1997) review of wolf and bear management policies in Alaska was that wolf control should be undertaken in a way that allows specific evaluation of its results, so as to unambiguously connect any prey responses to the control actions. NRC (1997) found that most previous control programs had not met this important scientific test.

The current Fortymile wolf control effort does not do any better. As pointed out in the previous section, there is a likelihood of major, variable year-to-year error in the caribou census estimates and sampling error in the recruitment estimates. There are problems in interpreting the neonate mortality samples. There is a likelihood of a significant shift of Nelchina caribou. There are questions about the timing, extent, and location of the early phases of control relative to caribou changes that began about the same time or were already underway.

At least two other complications remain to be addressed. First, regardless of the extent to which Nelchina caribou have augmented Fortymile numbers on a year-round basis, the presence of tens of thousands of Nelchina caribou in the core Fortymile range for even half the year is bound to divert substantial wolf predation from Fortymile caribou. Given this mixing of Nelchina with Fortymile caribou, how does ADF&G propose to distinguish between changes in Fortymile recruitment and mortality that were due to *reductions* of Fortymile wolf numbers via control versus changes that were caused by *diversions* of predation from Fortymile to Nelchina caribou versus changes that were caused by some combination of the two (ignoring for the moment the absurdity of even thinking about this "herd" distinction)?

Second, my 1993-1999 aerial radio-tracking observations indicate that the Fortymile wolf population is highly mobile, with seasonal migrations by certain individuals and family groups extending up to 50-100 miles to caribou calving, post-calving, and wintering areas. These migrations and related hunting forays involve wolves from surrounding areas as well as wolves living primarily inside the control area. For example, in June 1998 two of five non-control area wolves that I radio tracked regularly migrated 40-50 miles from their primary territories to the Fortymile caribou post-calving concentration, inside the northeast corner of the control area (Wallcutt Mtn-Glacier Mtn), within the territory of the "treated" Butte wolves (i.e., ADF&G had reduced this resident family group to a sterilized pair). In addition, I saw another, uncollared, adult wolf amidst these caribou; this wolf was not one of the Butte pair. To what extent does

control provide new hunting opportunities for migrating outsiders, even if they visit only temporarily? How would ADF&G measure this additional predation effect and the way it interacts with predation by control-area residents relative to any caribou recruitment and mortality changes? How does it alter recruitment-mortality results of the control-area reductions?

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