David J. Mattson, Ph.D.

Wyoming Wildlife Advocates

Comments on the

US Fish & Wildlife Service proposal to remove grizzly bears in the Yellowstone ecosystem from the list of endangered and threatened wildlife protected under the US Endangered Species Act (ESA); Federal Register 81(48): 13174-13227

And Related Materials

May 5, 2016

Introduction

On behalf of Wyoming Wildlife Advocates, this package of materials constitutes my review of the Rule and related materials (hereafter the Rule) issued by the US Fish & Wildlife Service (the Service) covering a proposal to remove grizzly bears in the Yellowstone ecosystem from the list of endangered and threatened wildlife protected under the US Endangered Species Act (ESA). The Rule itself was published in the Federal Register 81(48): 13174-13227; I obtained additional related materials from the Service's web site under Docket no. FWs-R6-ES-2016-0042: FXES111309000000C6-156-FF09E42000. These additional materials include the draft 2016 Conservation Strategy plus appendices (hereafter the CS) and the draft Grizzly Bear Recovery Plan Supplement: Revised Demographic Criteria (hereafter the RP).

Together, these materials comprise an astounding 675 plus pages (not counting cover pages), of which 54 pages are triple column, 9-point font, with a provision in the Rule for a mere 60 days within which to submit any comments. Up front, this allocation of such limited time to come to terms with such a large amount of highly complicated materials defies any explanation that invokes a genuine interest on the part of the Service in revision and improvement. That being said, my intent here is provide comments and critiques in hopes that the Service will genuinely engage with the substance of what I offer.

The package of materials assembled by the Service in support of its proposed Rule clearly represents a huge effort on the part of what I am sure are well-intentioned people. However, I see room for major improvement ranging from revision of framing concepts, to inclusion of major bodies of currently neglected research, to improved interpretation of the science that is referenced, to remedy of the many points of internal illogic. My comments cover this entire spectrum.

My review adheres to two sets of standards invoked by the Service on pages 13175 and 13176 of the Rule; first, the conventional standard of the ESA requiring use of "the best scientific and commercial information available," and, second, a purported desire on the part of the Service to insure that the Rule is "based on scientifically sound data, assumptions, and analysis." Tacit to this second set of standards is a laudable recognition on the part of the Service that the scientific information most recently published in a scientific journal can, in fact, be junk; hence a need to consider the very merits of this corpus of science, including the nature of the data used, the veracity of analyses employed, and the validity of interpretations, not only in the science products themselves, but also by the Service in application of the science to this Rule.

I also assume in my review that the best available science includes all publicly available data or information of direct relevance to the soundness of this Rule, subject to the same kinds of summaries and interpretations employed by the Service, and regardless of whether it is included in a peer-reviewed journal. For example, I draw heavily on graphic representation and simple statistical analyses of summary data published by the Interagency Grizzly Bear Study Team (IGBST). I also draw at times on other publicly available databases. This approach is based, in turn, on the assumption that there is good faith on the part of the Service—along with a genuine interest in improvement—rather than a focus on dismissing inconvenient input on the basis of some legalistic or less-than-logical technicalities.

My review that follows is organized by numbered main topics with subtopics within, each highlighted in green. The references cited in my comments are in **Attachment 1**.

My Credentials

I am currently Lecturer and Visiting Senior Scientist at the Yale School of Forestry and Environmental Studies, Adjunct Faculty at Northern Arizona University, Science Advisor for Wyoming Wildlife Advocates, and Research Associate with the Northern Rockies Conservation Cooperative. My former positions, prior to retirement from the U.S. Geological Survey, included Research Wildlife Biologist, Leader of the Colorado Plateau

Research Station, and Western Field Director of the MIT-USGS Science Impact Collaborative, all with the USGS. I hold degrees in Forest Resource Management and Forest Ecology. My doctorate is in Wildlife Resource Management from the University of Idaho.

I have studied large carnivores for over 30 years. This research has included the incorporation of ecological information from mountain lions and grizzly bears into demographic, habitat, and risk management models as well as investigations of foraging, predation, and relations with humans. My human-dimensions research spans nearly 23 years and has focused on social, political, and organizational dynamics that shape policies and practices of carnivore and other conservation programs. As part of this focus I routinely teach classes on relations between science and policy. My research has been reported in over 120 peer-reviewed publications, including work featured in *Science* and *Ecology* and as part of invited talks at the Smithsonian, American Museum of Natural History, the American Institute of Biological Sciences, and International Conferences on Bear Research and Management.

Of more direct relevance to these comments, I have studied grizzly bears in the Yellowstone ecosystem since 1979. I was part of the Interagency Grizzly Bear Study Team 1979-1993 and held primary responsibility for field investigations of the IGBST between 1984 and 1993. During this 16- year period I spent most of March-October documenting grizzly bear foraging behavior and involved in other related field projects. My grizzly bear-focused research resulted in over 60 publications, including several that are considered classics in the field of bear research. I continue to closely follow grizzly bear research in the Yellowstone ecosystem with an interest in improving not only the science, but also the use of scientific results in authoritative decision-making.

1. Given current monopolistic scientific arrangements, the Service should use a much more cautious tone when invoking IGBST science in the Rule and remove what is currently a pattern of systematically inflated claims based on this body of science. They also need to give serious consideration to patterns of evidence that call into question IGBST science, many of which are described in this review.

1.1. Virtually none of the most recent science published in peer-reviewed journals by the IGBST and used by the Service in this Rule is reliable.

This problem follows from the simple fact that the IGBST, in concert with the Service, has maintained a monopoly on virtually all of the data of direct relevance to the Rule. There is only one Yellowstone grizzly bear population and only one set of data that has been collected from this population; and none of these data, but a pittance, have been available to any researchers or research teams other than the IGBST and those scientists whom the Team has invited to collaborate. A monopoly such as this debars replication, disallows alternative models and hypotheses, provides ample scope for bias, and cannot be corrected by peer review (see my following point).

Transparency and open access to data and/or to relevant systems is at the core of scientific advancement (e.g., Popper 1959, Merton 1973, Pickering 1992, Maxwell 1998, Losee 2004, and many more). There is no substitute or sufficient remedy when such conditions are lacking. In the absence of ample opportunity for others to independently check, test, replicate, contest, and advance a body of research, those who enjoy a monopoly predictably fall prey to well-documented phenomena, including confirmation bias and groupthink, especially when they are part of a closed or mostly closed research team. They can also more easily fall prey to political influence and the sway of money, especially when there are those involved in crafting or internally reviewing the research who have an outside vested interest, and doubly so when such people provide substantial funds. All of this holds for the IGSBT and the science it has produced during the last decade, which is the purported foundation for this Rule.

These intrinsic problems are further amplified by the fact that most of the science produced by the IGBST and invoked by the Service entails complex models and complex assumption-ridden statistical methods. Such is the case for all of the results germane to judging the demographic and genetic plight of Yellowstone's grizzly bears. Yet it is well known to even half-way thoughtful scientists that there is no one correct model, and that model-building is perhaps the most vagarious and bias prone of scientific undertakings (see Point 4.2 below). The best we can collectively hope for is that a range of relevant models are produced by as many truly independent researchers as possible for collective deliberations. Unfortunately, what we have is the opposite of this: a closed enterprise under IGBST and Service auspices, privileged with fielding a few chosen models produced under predictably biasing circumstances. Again, as I describe in my next key point, no amount of peer review can correct for this.

I anticipate that the Service's response to this critique will be to list all of the scientists who have been <u>invited</u> by the Service and the IGBST to <u>collaborate</u> on scientific undertakings. Such a list would be largely irrelevant to the basic point I make here, which is that scientific progress and reliability require free and unhindered access to systems and/or data for use by independent investigators as part of independent inquiry.

There are potentially several ways to interpret what appear to be patterns of systematic bias in public statements made by IGBST scientist—and patterns there are. But there is little doubt about the highly problematic structural configurations of the environment within which IGBST scientists have produced the science relied upon disproportionately by the Service in this rule, and which virtually guarantee the production of unreliable science. Put another way, the burden should be on the Service to articulate a compelling argument for how and why a veritable monopoly on the relevant scientific enterprise guaranteed or even increased the odds of reliable scientific outcomes.

1.2. Peer review is unlikely to detect much less correct for bias in IGBST science introduced by monopolistic arrangements.

The IGBST operates under the auspices and authority of the US Geological Survey (USGS). In its various policy documents, the USGS makes many claims regarding the efficacies of peer review, including the assertion that “peer review…insures the scientific quality of USGS information (USGS Manual, 502.4, Fundamental Science Practices) and “peer review, as cornerstone of scientific practice, validates and ensures the quality of published USGS science” (USGS Manual, 502.3, Fundamental Science Practices). It is these claims that apparently undergird the Service’s and the IGBST’s defense of a monopolistic scientific enterprise. In keeping with these policy statements by USGS, the Service and IGBST commonly claim in public that peer review will detect error and ensure, if not guarantee, the quality and reliability of the IGBST’s science. In fact, these assumptions appear to be behind the Service’s elicitations of peer review by three select reviewers as a presumed corrective for error during further development of the Rule.

Put bluntly, the claims made on behalf of peer review by the Service, USGS, and IGBST are not empirically substantiated. The corpus of scientific inquiry into the efficacies and problems of peer review (e.g., Burnham 1990, Armstrong 1997, Campanario 1998, Bacchetti 2002, Biagioli 2002, Benos et al. 2007, and many more) has shown that error is detected at about a rate one would expect by chance, and that, if anything, peer review is about adhering to dominant paradigms and drawing support from the predictably commonplace reviewers who adhere to them as well. (Not coincidentally, the IGBST has published over 75% of its research since 2004 in *The Journal of Wildlife Management* and *Ursus*, both of which could be considered bastions of dominant paradigms.)

Because of empirically-based concerns about peer review, there has been a corresponding groundswell of criticism of peer review in the scientific community. About the best that thoughtful defenders of peer review can say for this practice is something akin to what Winston Churchill said about democracy: “Democracy is the worst form of government, except for all the others.” Although defenses—and critiques—such as this still allow a constructive place for peer review, they are hardly a basis for putting faith in this process as some sort of guarantor of scientific quality.

Peer review clearly functions as a mechanism for censorship, improvement, maintenance of identify amongst a community of self-styled elites, and a device to be used for political advantage. Unfortunately, claims on behalf of peer review by the Service, the USGS, and the IGBST bespeak a political enterprise more than they reflect any objective and empirically-based assessment of what peer review has to offer (see Appendix 2 for an elaboration of this argument). As above, the burden should be on the Service to provide an empirically-based and compelling argument for how, in fact, peer review corrects for the potential bias-inducing effects at play in this situation. This burden weighs all the heavier because of the monopolistic arrangements I describe immediately above.

2. The frames currently used by the Service and the IGBST to organize information about the interrelations of food quality and quantity, hazards, and birth and death rates are deficient and do not reflect the best available science.

2.1. The Service fails to coherently address the joint dimensions of diet that include food quality and the characteristic hazards associated with consumption of a given food. Because of this failure, the Service's representation and analysis of the interaction of hazards with quality/quantity is haphazard and prone to selective manipulation in service of the immediate argument. This translates into patently deficient logic and analysis throughout the Rule and CS.

The demographic implications of consuming a given food will be defined at a population level by effects on both birth and death rates. Effects on fecundity will be predictably mediated by effects on the condition of reproductive females and the extent to which those effects translate into reproductive rate. Effects on death rate will be dictated by the characteristic hazards associated with using a particular food. A high quality food with positive effects on birth rates can either be associated with foraging opportunities concentrated in highly secure habitat or the opposite. This latter set of circumstances, where a high-quality food lures an animal into a highly lethal environment, has become the focus of scientific inquiry organized under the rubric of "ecological traps" (e.g., Delibes et al, 2001, Schlaepfer et al. 2002, Hale & Swearer 2016), which is interrelated with but distinct from source-sink dynamics (Kristan 2003).

The concept of ecological traps is not new to bear researchers. Most notably, Joe Northrup and Scott Nielson deployed this concept (with Nielson using slightly different terminology) in geospatial conservation assessments for grizzly bears in Alberta (Nielson et al 2006, 2009; Northrup et al. 2012). Moreover, there have been several highly congruent landscape-level assessments of hazards for Yellowstone grizzly bears (Carroll et al 2001, 2003; Merrill & Mattson 2003; Johnson et al 2004; Schwartz et al. 2010) which have been directly relevant to judging the hazards of using different foods with different geographic distributions (e.g., livestock, whitebark pine, and cutthroat trout), albeit without explicit integration of hazards and productivity under rubrics such as ecological trap.

Yet the Service does not deploy the concept of ecological traps; nor do they, in fact, deploy any explicit framework by which the nutritional benefits and entailed hazards of any given diet or dietary shift are simultaneously considered. Throughout the Rule and CS, nutritional aspects are examined in isolation from effects on survival. This is a major failing given that the Rule and CS devote much text to presumably assessing the consequences of the dietary changes afoot with Yellowstone's grizzly bears, related to the loss of most whitebark pine and cutthroat trout, and the apparent shift of surviving bears to eating more army cutworm moths and meat from terrestrial mammals—among other things. The integration of hazards with nutrition needs to occur if for no other reason than the overwhelming evidence for variation of the hazards incurred by adults when using foods of otherwise similar nutritional quality: e.g., much lower for bears eating whitebark pine seeds and trout compared to bears consuming a diet of meat from ungulates, or any other food that brings bears near and into conflict with people.

At a minimum, the Service needs to differentiate major grizzly bear foods according to some sort of matrix that distinguishes high-quality foods entailing characteristically high hazards from high-quality foods entailing characteristically low hazards, and furthermore differentiate whether the hazards are born primarily by dependent young, independent bears (adults and adolescents), or both. This latter distinction is important to any assessment because 80-90% of all independent-aged grizzlies die from human-related causes (e.g., Schwartz et al. 2006) whereas many dependent young bears die for "natural" reasons, including predation by other bears. Hazards associated with humans are thus thrown into sharper relief in any assessment of dietary shifts among adult bears, whereas hazards associated with potential "natural" predators are a more prominent consideration in assessing changes for cubs and yearlings. Parenthetically, the Service also needs to assess the hazard-related consequences of shifts in consumption of lower-quality foods (e.g., any roots or foliage).

2.2. The Service fails to adequately frame and address the many environmental changes that have occurred in Yellowstone's grizzly bear range during the last 15 years. As a result, the Service has no basis for judging the effects of any single change, including loss of whitebark pine. This problem partly derives from the fact that the Service relies almost exclusively on science produced by the IGBST, which is similarly deficient in its conceptualizations.

All of Yellowstone's grizzly bear data have been collected as part of an observational study, which translates into serious demands on analysts if they are to arrive at even half-way defensible conclusions regarding cause and effect. Observational studies are almost invariably beset by a host of causal dynamics, many of which interact or are correlated in time and space. As a result, isolating the effect of one factor on an outcome of interest—say, the effect of whitebark pine seed consumption on grizzly bear birth rates—requires somehow accounting for and controlling the effects of all other casual factors of any probable significance.

In practical terms, this requires building replete conceptual models of the system of interest that incorporate as much knowledge about prospective ecological drivers as possible; identifying valid measurable surrogates for each of these drivers; and then incorporating them into some kind of model as covariates or causal variables. Sir Ronald Fisher, the father of modern statistics, was amongst the first to recognize this imperative when he stated "make your theories elaborate" as a precondition for taking the step from association to causation in observational studies. William Cochran, who elaborated on the use of covariates in analyzing data collected from observational studies of complex systems, laid the foundation for following seminal work (e.g., Dawid 1979, Rosenbaum 1984, Hilborn & Mangel 1997) on the importance of having considered enough factors to ensure that the resulting models were sufficient; that is, not likely to substantially change with the introduction of additional covariates. And so on.

Given the extent to which the Service depends upon science produced by the IGBST, this issue of sufficiency raises the question of whether the conceptual and statistical models produced by the IGBST were defensible—or not; whether they adequately attended to all of the factors that prior knowledge would suggest were drivers of variation in, say, birth and death rates of Yellowstone's grizzly bears. If the conceptual underpinnings of the analyses undertaken by the IGBST were *prima facie* inadequate, there is all the more reason for skepticism of their results, especially in the face of contradictory evidence.

In short, the conceptual models and resulting statistical results produced by the IGBST have been patently inadequate. The Study Team has failed to address a number of potential drivers of birth and death rates, and has not adequately dealt with the high degree of spatial and temporal correlation that besets many of these factors. To date, the IGBST has only considered routine effects (bear sex and age class), plus whether a bear was trapped for research or management purposes (as a surrogate for whether the bear was food-conditioned, habituated, or not); management jurisdiction and nearness to roads (as a surrogate for human lethality and exposure to humans); bear density (through suspect indices; see my later comment on this); and, in earlier analyses, size of whitebark pine cone crops (Schwartz et al. 2006). Spatial representations of whitebark pine forests and density were added recently by Van Manen et al. (2015).

At a minimum, by slavishly relying on the IGBST's science, the Service has failed to address in any sort of integrated way the effects of on-going increases in use of army cutworms and the sites where these insects aggregate; the effects of losing virtually all of the cutthroat trout spawning in streams tributary to Yellowstone Lake since the mid-1990s; the effects of major declines in most elk and some bison herds, also since the mid-1990s; the effects of a severe drought between 1998 and 2008; the effects of a short-term masking trend towards increased per tree cone crops on whitebark pine since roughly 1997; increasing exploitation of livestock and the remains of hunter-killed elk since roughly 2006; and the extent to which trout losses, ungulate herd declines, losses of whitebark pine, increases in cone crops, increases in availability of moths, and putative increases in bear density were highly correlated in time, sometimes in space, and sometimes as an interaction of both. This multitude of tangled effects probably constitutes as severe an analytic challenge as any analysts have faced trying to divine cause and effect in an ecological system. This complexity also increases the odds that bias can and will be introduced in the necessary simplifications made by the IGBST, its collaborators, and the Service—by the choices of what effects to consider, and how.

In fact, bias is amply evident in the extent to which the IGBST structured its post-2009 inquiry into grizzly bear vital rates as an artificially simplified contest between the extent of whitebark pine forests, on the one hand, and bear density (e.g., density dependence), on the other (see also my comment below on use of the density-dependence concept by the Service and the IGBST). It hardly seems a coincidence that the 2009 and 2011 Court rulings over-turning the Service's 2007 delisting rule were based on the extent to which the Service was perceived to neglect and misrepresent the body of science showing a major effect of whitebark pine seed availability on grizzly bear vital rates. And even during the period of litigation, the Service was contending that bear density alone was driving changes in vital rates, and that losses of whitebark pine were minor. It is hard to avoid concluding that the post-2009 science produced by the IGBST was (and is) merely the fulfillment of a politically-driven agenda.

At this point I need to clarify my critique with reference to the standard practice reported by the IGBST in virtually all of its publications, which is to generate *pro forma* lists of rote models containing permutations of variables that they considered, along with AIC values for each. Such lists do not address the IGBST's and Service's failure to consider certain effects altogether and the Study Team's consistent under-use of useful measures of model goodness-of-fit and predictive value. As I noted above, these are all critically important considerations in the production of models designed to address the complexities of open ecological systems, which, if neglected, can result in nothing more than misleading if not useless results (see Pilkey & Pilkey-Jarvis 2009).

As a bottom line, this problem of deficient conceptualization of the ecological system debars any confidence in how the Service represents or otherwise uses science produced by the IGBST. One option would be to put the Rule on hold and allow for additional investigations of alternative models by independent researchers. Another option would be, at a minimum, for the Service to acknowledge the limitations of the science they rely on, clarify the resulting uncertainties and ambiguities, and adopt a suitably precautionary approach.

But regardless of deficiencies in the research produced by the IGBST, this does not excuse the Service from deploying a conceptual framework sufficient to the complex and demanding task of assessing the effects on Yellowstone grizzly bears of all the environmental changes that have occurred since the time of Listing. In fact, nowhere is there any evidence of the Service deploying such a frame.

2.3. The Service fails to use the best available science in its conceptualization of factors driving levels of human-caused grizzly bear mortality. As a result, the Service conflates static spatial delineations of so-called secure habitat with what is, in fact, a highly dynamic set of factors driving current increases in human-caused mortality.

The Service repeatedly states in both the Rule and the CS that maintenance of a select number of GIS-delineated spatial habitat attributes at 1998 levels within the PCA will insure that rates of human-caused mortality will be no higher in the future than those that occurred during roughly 1988-1998. These spatial attributes include road densities, the extent of areas >500m away from human infrastructure (so-called "secure" habitat), and numbers of livestock allotments.

These arguments are substantively deficient in several regards. First, the Service fails to recognize that human-caused mortality is driven by two distinct phenomenon, one with a major geospatial dimension, the other largely without: the simple distinction between how often bears encounter people (frequency of contact) and the likelihood that the encounter will end up lethal for the involved bear (lethality of encounter).Second, and related to the first, the equivalence of static landscape features with main drivers of human-caused mortality is simple-minded and thereby misses several other key factors that render a static geospatial analysis patently deficient.

Taking each of these failings in turn, first, Mattson et al. (1996a, 1996b) provide a clear articulation of the logic behind and implications of distinguishing between frequency and lethality of human-bear encounters as determinants of total human-caused bear mortality. Numbers of deaths can be high under conditions of low-frequency contact, as typified by encounters between grizzly bears and big game hunters or grizzly bears and wildlife managers operating at the behest of livestock producers; and numbers of deaths can be low under conditions of high-frequency contact, as typified by encounters between habituated bears and masses of tourists along National Park roads. It is human lethality, not frequency of encounter as such, that differentiates these kinds of scenarios.

Interestingly, the Service awkwardly and tacitly implies these distinctions in its references to research that has purported to explain variation in grizzly bear mortality rates on the basis of management jurisdictions or delineations (NPS lands versus the PCA versus outside the PCA; Schwartz et al. 2006, 2010). Without explicitly saying so, these effects derive almost wholly from variation in human lethality, which varies, in turn, as a function of whether people tend to be well-armed (e.g., big game hunters), associated with features that generate conflict (e.g., livestock allotments), and tolerant (or not) of grizzly bears (e.g., most amenity migrants versus most livestock producers). And, importantly, human lethality can vary by orders of magnitude over time, as between the 1800s-early 1900s and the period after which grizzly bears were protected under the ESA (Mattson & Merrill 2002). Likewise, the Service tacitly argues in its many claims for the efficacies of its ESA-mandated management that declines in human lethality were a result of better garbage management, food storage, and education.

The main point here is that human lethality can vary widely in time and space as a function of human attitudes and behaviors. It is a highly dynamic phenomenon that contributes every bit as much as frequency of human-bear contact to total grizzly bear mortality. The Service does not recognize this, which results in other deficiencies in its argument that I refer to elsewhere.

Second, and related, a static spatial representation of absolute lethality of the Yellowstone landscape (which the Service tacitly does through its invocation of a static spatial baseline) fails to recognize that grizzly bears may spend more or less time near people—both inside and outside of so-called secure areas—as a function of social interactions amongst bears and changes in the abundance and distribution of key foods. As important, these redistributions of bears can expose them to people of varying lethality. Grizzly bears can encounter highly lethal people in so-called secure areas at varying rates depending on factors that motivate the bears. The main example of this is grizzly bears encountering elk hunters in the backcountry at frequencies that vary depending upon how strongly the bears orient to the offal left behind from elk kills by hunters (Haroldson et al. 2004). All of this plays out in so-called "secure" areas. And, as I point out elsewhere, bear mortalities resulting from conflicts with elk hunters have skyrocketed since roughly 2007, again, mostly located in what the Service is calling "secure" habitat.

The main point here is that grizzly bears are free agents motivated by a search for preferred food (e.g., Mattson et al. 1992, Haroldson et al. 2004) as well as avoidance of other bears (Mattson et al. 1987, McLellan & Shackleton 1988). These movements and resulting redistributions with respect to humans can systematically change over time (as claimed by Costello et al. 2014), at various scales, with resulting changes in exposure of even the same numbers of bears to varying hazards and levels of risk associated with humans—resulting in varying levels of human-caused mortality even with the same amount of so-called "secure" habitat. The Service essentially ignores all of this in its unqualified claims that maintaining a static spatial baseline dating back to 1998 will somehow insure the security of grizzly bears (e.g., pages 13182 & 13184).

For this and other reasons articulated in my critique, the Service needs to account in its habitat management prescriptions for all of the social and dietary changes since 1998 that have resulted in increased exposure of grizzly bears to human hazards synchronous with no net change (or increase) in livestock allotments and human infrastructure.

---

3. The Service employs a simple-minded and unsubstantiated conception of carrying capacity as a basis for in turn making unsubstantiated claims regarding the number of bears able to be sustained in the PCA and DMA, and past and likely future effects of food quality and quantity on Yellowstone's grizzly bear population.

The Service's representation of carrying capacity (*K*) throughout the Rule and CS is almost wantonly simple-minded and seemly constructed to serve the sole purpose of supporting assertions that (1) there is no more room for grizzly bears in the PCA (and DMA), (2) that no deleterious habitat trends are afoot that might compromise current conditions, and, because of that, (3) the population is secure, recovered, and ready to be delisted.

The gratuitous figure on page   of the Rule is emblematic of the Service's problematic use of the carrying-capacity concept. Here, they show *K* as being static and unchangeable, with population size fluctuating around it—presumably as a representation of what has happened and will continue to happen with Yellowstone's grizzly bear habitat and population. In fact, this representation is utterly at variance with both reality and the several references that the Service cites as if for substantiation.

In fact, carrying capacity not only changes from year to year, but also exhibits long-term trends as a function of long-term changes in availability of critical resources—most importantly, food. As I have pointed out throughout my critique (and as the Service tacitly admits in many places), carrying capacity has changed for grizzly bears in the Yellowstone ecosystem as a function of changes in food availability. Since the mid-1990s positive changes can readily be attributed to increasing consumption of moths in the Absaroka Mountains and increases in the Northern Range bison herd. The more numerous negative changes can be attributed to functional extirpation of cutthroat trout in Yellowstone Lake; 50-70%+ loss of mature cone-producing whitebark pine throughout the ecosystem; major declines in virtually all of the ecosystem's elk herds; and major recent declines in the Central Yellowstone Park bison herd. A major decade-long decline is also plausibly attributable to the epic drought that lasted 1998-2008 (see the graphs I present under point 10). In the net, *K* almost certainly declined during the last 20 years. Put another way, it defies reason and all available evidence to credibly argue otherwise—which is what the Service tries to do in the Rule.

Another key point is that, when reckoned against total size of a population that is expanding in distribution, *K* must be assessed not only for a given area, but also for the cumulative area occupied by the expanding population. This is relevant for Yellowstone's grizzly bear population given that its distribution has apparently expanded by 30-40% (Bjornlie et al. 2013) at the same time that the population was "stable"; i.e., not increasing in numbers. In other words, approximately the same number of grizzly bears was sustaining themselves by occupying an ever-larger area, which presumably cumulatively encompassed an equal or greater amount of food; increasingly including livestock on the periphery of the ecosystem (see my point 9).

Figure 3.1 encapsulates all of these points in graphic form and is presented as a corrective for the egregiously misleading figure offered by the Service in the Rule. Here carrying capacity for the core of the ecosystem is shown by a dark gray line as an annually highly variable value, but with a long-term decline over the long run. The effect of bears expanding into new areas is shown by the light-gray-shaded area above, constituting a mitigating offset for losses in the core. Population size is shown by the red arrow, and actually exhibits less variation than occurs in *K* itself. This arises from the fact that grizzly bears can buffer themselves somatically and behaviorally from inter-annual variation in habitat productivity (e.g., Herrero 1972), and because grizzly bear populations are subject to potentially long lags between when habitat change occurs and when this change is manifest in population size (Doak [1995], McLellan [2015]; see my point 5 about lag effects).

As a bottom line, *K* for Yellowstone's grizzly bear habitat has certainly varied widely and has probably declined substantially. To the extent that the bear population has reached "carrying capacity," this probably has more to do with declines in *K* than to increases in population size, as such. Understood this way, being at carrying capacity is not an argument for current or long-term security, but rather an argument for current and long-term vulnerability. By itself, a population at *K* is not an argument one way or another for judging recovery.

The Service needs to correct its misleading and simple-minded representations of carrying capacity along with relations of *K* to population size in its risk assessment. In service of this end, the Service has my permission to use the graphic above in the Rule and related materials.

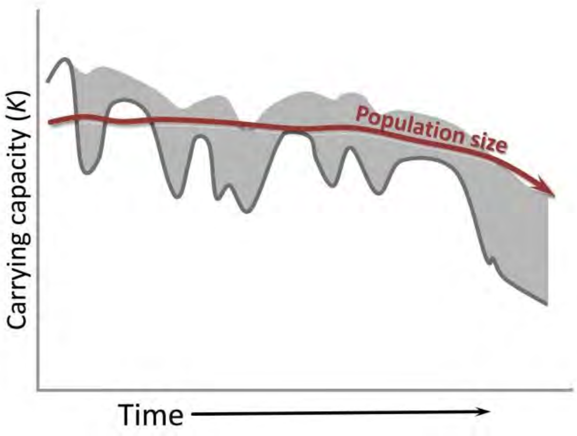


Figure 3.1. A more realistic conceptual portrayal of how size of the Yellowstone grizzly bear population (in red) has reflected—or related to—carrying capacity, in the core (dark gray), and accounting for the mitigating effects of population expansion into new areas (light gray).

---

4. The Service employs "density-dependent effects" in this Rule with little apparent understanding of the concept and with no reference to, and in contravention of, the most reliable and straight-forward scientific reckoning of trends in grizzly bear density in the Yellowstone ecosystem.

The Service claims throughout the Rule that changes in the demography of Yellowstone's grizzly bear population since the mid-1980s have been driven largely (if not solely) by changes in population density, entraining "density-dependent effects." On this basis the Service then goes on to dismiss effects attributable to changes in abundance of whitebark pine—or any other food. The primary basis for this oft-repeated claim is Van Manen et al. (2015), although with parenthetical reference to Schwartz et al. (2006), who also purport to have detected "density-dependent effects." The Service, citing Van Manen et al. (2015), attributes declines in survival rates of cubs and yearlings to increasing bear densities, but otherwise flags few other effects of density on either birth or death rates.

The Service's claims suffer from an invocation of naïve if not downright misleading conceptions of density-dependence and from critical—even fatal—failings of the cited research. Its claims that density has increased since around (or even before) 2000 are also contradicted by straight-forward and unambiguous scientific data, and otherwise have little scientific credibility. In short, the Service's invocation of density is one of many slender broken reeds upon which its arguments are based.

Starting with the most straight-forward point first: grizzly bear densities almost certainly did not increase in the Yellowstone ecosystem during the 2000s and after. As the Service frequently asserts, the population "stabilized" during this period, meaning few if any increases in numbers of bears. A trend line fit to Mark-

Resight-based estimates of population size (a less bias-prone method compared to the Chao2-based approach) substantiate this assertion, with the added possibility of population declines since around 2006-2007 (see the bottom graph in figure 5.3.1). At the same time, the distribution of the population increased by roughly 40% (Bjornlie et al. 2013). Simple math shows that if you have the same number of individuals spread out over a substantially larger area (n/area), density axiomatically decreases. To the extent that demographic rates of the Yellowstone grizzly bear population have changed at all during the last 15 years, it is almost certainly not because of increased bear densities—at least by our most straight-forward and unambiguous reckonings of density.

Turning more specifically to the methods employed by Schwartz et al. (2006) and Van Manen et al. (2015): In the case of Schwartz et al. (2006), they used a time-specific population-wide index of "density" based explicitly on annual counts of females with COY. As I point out elsewhere (my point 19), this index of population size almost perfectly mirrors changes in efforts by researchers and managers to find bears, along with changes in the intrinsic sightability of the bears they were seeking. By contrast, the correlation between annual counts of females with COY and actual population size is unknown and almost certainly meaningless if not deceptive. Moreover, the index used by Schwartz et al. does not account for steady increases in the distributional extent of the population (e.g., Schwartz et al. 2006b, Bjornlie et al. 2013), which introduces further unaccounted-for bias. Compounding these intrinsic problems is the fact that Schwartz et al. (2006), like Van Manen et al. (2015), did not control for a number of major temporal changes in abundance of key foods that were correlated with their purported index of bear density (e.g., cutthroat trout, elk, bison, and army cutworm moths), which debars any isolation of a density effect—even assuming a valid index—and introduces major statistical problems related to covariance of explanatory variables.

In contrast to Schwartz et al. (2006a), Van Manen et al. (2015; and Bjornlie et al. 2014) used a purported index of population density that varies not only by year, but also with respect to a grid of cells sized to approximate the extent of female home ranges. Because of this spatial and temporal resolution, the Bjornlie/Van Manen index gives the reassuring impression of high-resolution precision to those looking at their results with an uncritical eye—which seems to be the case for the Service.

In fact, this index is yet another instance of a metric with no known relationship to the parameter it purports to indicate (i.e., bear density), and almost certainly substantially contaminated by, in this case, efforts of researchers and managers to capture and collar grizzly bears. Briefly, the density index is directly based on the number of bears trapped and radio-marked in a given area during a given year, weighted by rote survival rates to "extrude" [sic] these bears forward and backward through time, after which the "extruded" bears are stacked and added to come up with a purported index of density. Van Manen and Bjornlie attempt to "validate" this index, *prima facie* biased by trapping effort, by correlating it with two other metrics that have no known relationship to density—other than what Van Manen and Bjornlie assert through argumentation. More tellingly, the population-level results of the arcane and unsubstantiated Van Manen/Bjornlie density index contradict the straight-forward calculation I describe immediately based on estimates of total population size and distribution.

And, again, because both Bjornlie et al. (2014) and Van Manen et al. (2015) fail to account for numerous other major changes in Yellowstone's grizzly bear habitat—also correlated in both time and space with their purported density index (see my point 10)—they have no basis for isolating any density effect, even assuming the index they use is valid. Given all of these problems with the Van Manen/Bjornlie density index, the burden is clearly on the Service to reconcile the contradictions between, on the one hand, the best available science regarding population size and distribution and, on the other, grossly deficient science that uses a *prima facie* problematic unproven index of density in context of patently inadequate analyses that fail to account for most of what is likely to affect grizzly bear birth and death rates (as per most of my review here). More to the point, the Service does not, in fact, use the best available science regarding density effects and, instead, relies almost solely on science of comparatively little merit.

Finally, the Service and the IGBST research that it invokes employ a simple-minded and often misleading conception of “density-dependence” that offers little insight of relevance to the deliberations of this Rule. As one insightful and well-respected population ecologist put it: "Density is not a mechanism" (this from Charles Krebs [1995, 2002]). What he meant by this is that birth and death rates of animals are driven by levels of predation, disease, and intra- and interspecific competition, along with the quality and abundance of foods, especially those eaten by females. If population density has any effect at all, it is through somehow modifying these critical factors. There is no direct effect of density, even in New York on a subway. Invoking density as an explanation for anything is equivalent to assuming that animals are ping-pong balls moving at random, with each ball equal in all of its effects.

To the extent that density has any effect, it necessarily interacts with carrying capacity and changes in the microscale and mesoscale distributions of individual bears in reflection of dietary changes driven by changes in availability of foods. In other words, at the same exact density, the frequency and lethality of interactions among bears can vary substantially depending on total abundance of food and the extent to which these foods are concentrated in time and space—as with differences in British Columbia between coastal areas with salmon spawning runs and interior areas with abundant by widely-distributed berries. The upshot of this is that a simple shift in diet among Yellowstone’s female grizzly bears from pine seeds to terrestrial meat (see my comments under point 9) could substantially modify levels of interactions with other bears, especially males, and without any change in bear densities.

Parenthetically, as I point out elsewhere (point 17), changes in cub and yearling survival rates can be plausibly explained simply by changes in foraging behaviors of female bears, and without needing to resort to poorly conceptualized and unsubstantiated claims regarding density. Survival rates of cubs and yearlings have probably declined simply because adult female grizzlies are eating more meat and incurring substantially more hazards for their dependent offspring; hazards primarily, but not solely, related to increasing odds of predation by other adult bears and wolves.

The Service needs to upgrade it conceptualization of density effects; recognize deficiencies in the science it currently invokes to justify its density-related arguments; adopt, instead, the best available science; and, finally, realize that changes in demography and trend of Yellowstone’s grizzly bear population are almost certainly a result of changes in food availability, diet, carrying capacity, and distributions—not increasing densities and presumed “density-dependent” effects.

5. The Service fails to adequately address the issue of lag effects in the Rule. There are no habitat-based standards proffered by the Service that would trigger reviews by the IGBST or Service; nor is there any history of the IGBST seriously engaging in its research with the existing suite of monitored habitat indicators; nor is there any history of managers or researchers in the Yellowstone ecosystem seriously considering any habitat trends except in response to litigation or threat posed by research published by other researchers.

On page 13224 of the Rule the Service addresses the issue of lags between changes in habitat and manifestation of these changes in population size and trend by asserting: “…the IGBST will monitor a suite of indices simultaneously to provide a highly sensitive system to monitor the health of the population and its habitat and to provide a sound scientific basis to respond to any changes or needs with adaptive mgmt. actions. This “lag effect” is only a concern if the sole method to detect a change in habitat is monitoring changes in population size. … [W]e feel confident that we will be able to detect the consequences of significant changes in habitat.”

This treatment of an important even critical issue by the Service is deficient in several regards. First, the only standards proffered by the Service that would trigger an authoritative review by the IGBST or Service are linked solely to population size—or mortality rates as a partial function of population size. Second, the history of how habitat trends have historically been treated by the IGBST and the Service provides no basis for trust. Third, there is no indication that the IGBST or the Service are responding to current evidence of unfolding lag effects with anything other than dismissal. And, fourth, the current suite of habitat indicators is lacking. Parenthetically, "feeling" confident seems a poor substitute for concrete authoritative measures and a history of competence and attention. Taking each issue in turn:

5.1. The Service does not describe any habitat-based standards in the Rule that would trigger authoritative reviews by the IGBST or Service.

According to the Rule, the only standards that would trigger an authoritative review by the IGBST or Service are linked to population size—or mortality rate as a partial function of population size. There are no triggers that link to trends in foods or other important features of habitat; all such effects are presumably filtered through population responses before warranting some sort of significant response by either state or federal managers.

In other words—and contrary to the Service's current innuendo—the provisions of the Rule that fundamentally matter ignore lags effects and solely invoke lagged population responses as a basis for triggering any meaningful management response.

5.2. The IGBST and Service have responded historically to lag-inducing trends in food and habitat with inattention or dismissal, and almost solely in reaction to litigation or threats posed by independently-published research. This is not a basis for trust.

The Service deploys what is essentially a "trust us" argument in its description of how the IGBST and managers will respond to trends in the "suite of indicators"—and this in the absence of any authoritative provisions for triggering a response (as per 5.1). Put bluntly, history provides no basis for trust.

In fact, history shows that the IGBST and Service and have responded to changes in foods and habitat (e.g., whitebark pine) either with inattention, dismissal, and, if attentive, only in response to litigation or research that threatens current hegemony. For example, nowhere in any of the IGBST research published during the last 10 years is there evidence that this research group has explicitly considered trends in the existing suite of habitat indicators that it monitors—with the exception of cone counts on whitebark pine transects (more on this later). Trends related to cutthroat trout, moths, spring carrion, and numbers of hunters and park visitors are reported each year in IGBST Annual Reports, and then essentially ignored in any analyses of grizzly bear movements or demography.

On a more positive note, the IGBST and Service have seriously engaged with the effects of changes in whitebark pine abundance on grizzly bear movements and demography. However, as I describe elsewhere in these comments, these efforts are fatally flawed and seemingly politically motivated. As evidence, the science on whitebark pine effects undertaken by the IGBST—at the behest of the Service—has almost solely been in response to either litigation or a challenge by independent researchers. The Service describes on pages 13175-1376 how rulings by the Montana federal District Court and 9th Circuit Court of Appeals motivated the current crop of whitebark pine-focused science. Prior to that, the treatise by Schwartz et al. (2006) covering whitebark pine effects did so primarily in response to a publication (Pease & Mattson 1999) that authoritatively flagged the issue as something to be considered.

Likewise, improvements in methods for monitoring population and mortality occurred in response to outside research that flagged short-comings in current approaches: Keating et al. (2002) and Cherry et al. (2007) in response to Mattson (1997c); and Cherry et al. (2002) in response to Mattson (1998). Spatially-explicit appraisals of habitat suitability and hazards by Schwartz et al. (2010, 2012) occurred in response to Merrill & Mattson (2003) and Johnson et al. (2004). The point here is that, during the last 15 years, the IGBST and Service have not demonstrated an interest in proactively addressing potentially lag-inducing habitat trends—or other science issues of import for that matter.

5.3. The IGBST and Service have shown no response to current evidence of lagged environmentally-driven changes in population size and trend; if anything, the response has been dismissal and denial.

Over 20 years ago Doak (1995) explicitly related the concept of lag effects to dynamics of the Yellowstone grizzly bear population. More recently, McLellan (2015) demonstrated lag effects of this nature for a grizzly bear population in the North Fork of the Flathead River drainage of British Columbia and Montana. A sustained crash in huckleberry production began in 1996 followed by the beginning of a correspondingly sustained decline in population growth rate the year after. However, tellingly, a definitive and dramatic decline in population size did not manifest until 11 years later—beginning in 2007—after which the population declined substantially.

Lag effects are highly relevant to judging the current status of Yellowstone's grizzly bear population given the major declines in major food sources that have occurred during the last 15 years, most notably, the terminal decline in whitebark pine seed availability beginning in 2007. Yet, if anything, the Service portrays a static world for Yellowstone's grizzly bears in its portrayal of carrying capacity (see my comment 3), while at the same time claiming that all changes will be subsumed by the bears' remarkable—even magical—absorptive omnivory and resilience.

Figure 5.3.1 is illustrative of unfolding lag effects for Yellowstone's grizzly bear population, referenced to data from the Flathead study taken from McLellan (2015). At top I've presented trend data for population size and growth rate for the Flathead grizzly bear population; my representation of growth rate is based on a 6-year moving average of proportional change in population size from one year to the next (i.e., ʎ = lambda). Green dots represent population density and gray squares represent growth rate. The take-away from the Flathead data is that population growth rate began to decline almost as soon as berry production tanked around 1996, but didn't turn negative until roughly 5-years later. More importantly, population size, as such, didn't begin a substantial long-term decline until 10-11 years after the berry drought started.

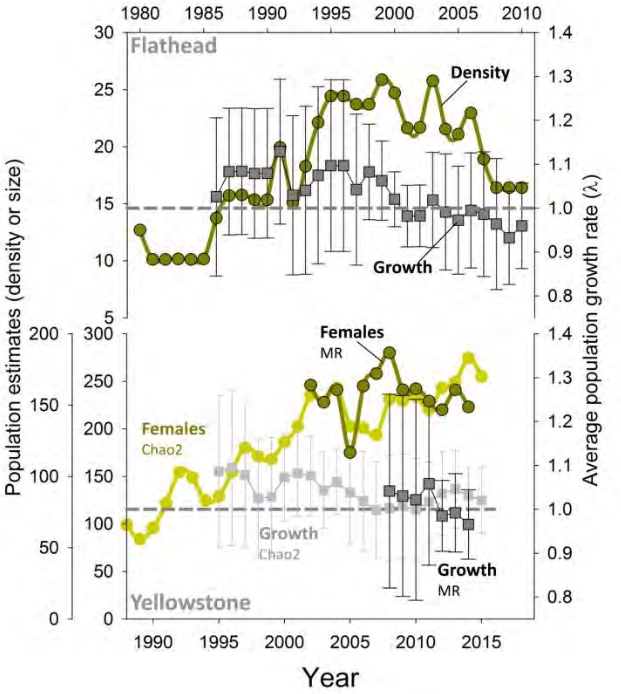


Figure 5.3.1. Trend data for the grizzly bear population in the North Fork of the Flathead, top (McLellan 2015), and for the Yellowstone grizzly bear population, bottom. Dark green dots represent best estimates of density or total population size, and the dark gray squares, derivative estimates of population growth rate (ʎ; as a 6-yr running average). Uncertainty bars around estimates of growth rate represent standard deviations. Yellowstone estimates based on the highly-biased Chao2 estimator are shown as yellowish-green dots and as light gray squares.

I show Yellowstone data in the bottom graph of figure 5.1. The darker green dots represent estimates of population size using the Mark-Resight (MR) method, which is less biased than the flawed Chao2 method adopted by the Service (see my point 19). The dark gray squares represent a running average of population growth rate based on MR population estimates. I also show population estimates using the Chao2 method (in light yellowish-green) and derivative estimates of growth rate (in light gray). The take-away point here is that average population growth began to decline around 2008, shortly after the onset of terminal declines in whitebark pine seed availability, and became negative (i.e, showing decline) only in 2012. Meanwhile, total population size has appeared more-or-less-static, with the slight suggestion of a down-turn during recent years.

Trends unfolding for Yellowstone's grizzly bear population bear an uncanny resemblance to trends that unfolded in the North Fork of the Flathead between 1996 and 2006, which substantiates concerns about on-going lag effects in Yellowstone, with worse yet to come. More to the point, the IGBST and Service show no evidence of, first, acknowledging these dynamics or, second, responding to them.

5.4. The foods and habitat features currently monitored by the IGBST, and presumably comprising the suite referenced by the Rule, are inadequate.

The IGBST currently monitors and reports on availability and grizzly bear use of moth sites, cutthroat trout streams, and spring carrion on ungulate winter ranges, along with numbers of cones on whitebark pine at fixed transects.

These monitoring efforts are inadequate because they fail to include foods or aspects of foods that are of unambiguous importance to detecting early signals of change in Yellowstone's grizzly bear habitat. More specifically, the IGBST needs to add numbers of elk and bison in various ecosystem herds as well as aerial extent of whitebark pine to its suite of indicators. The IGBST itself shows that meat from ungulates is becoming increasingly important to Yellowstone's grizzly bears (Schwartz et al. 2014, Ebinger et al. 2016) and that aerial extent of whitebark pine forests is a critical dimension of grizzly bear habitat (Bjornlie et al. 2014, Van Manen et al. 2015).

5.5. The Service needs to make substantial revisions to the Rule to insure that lag effects are adequately dealt with.

The Service needs to make several substantive changes to the Rule designed to remedy the short-comings that I describe here. For one, the Service needs to set meaningful standards linked to monitored foods and habitat features that will authoritatively trigger reviews by the IGBST and Service. These standards and associated triggers should be designed to anticipate and help mitigate for unfolding changes that will likely drive lagged responses in population size and trend. For another, the Service needs to add numbers of elk and bison in the ecosystem as well as aerial extent of mature whitebark pine to the suite of habitat features to be monitored by the IGBST. Finally, The Service needs to demonstrate that it takes the issue of lag effects seriously by acknowledging and accounting for unfolding trends—in contrast to its current pattern of denial and dismissal.

---

6. The Service fails to use the best available science when describing the taxonomy and evolutionary biogeography of Yellowstone's grizzly bears. Instead, the Service uses outdated science to categorize Yellowstone's grizzly bears as part of a purported continent-spanning subspecies when the best available science clearly shows that the Yellowstone population is part of a clade (Clade 4) with an ancient and unique history, a restricted distribution, and warranting consideration as an evolutionarily unique and threatened genetic lineage.

The Service makes reference in the Rule to an antiquated research paper (Rausch 1963) as a basis for lumping Yellowstone's grizzly bears in with a presumed subspecies that spans the entirety of western North America. The best available science, comprised of a large body of post-1995 research, clearly and emphatically shows that Yellowstone's grizzlies are, instead, part of the Clade 4 lineage. This matters because Clade 4 brown bears have a unique and ancient history and a current distribution that is restricted to central-western North America and an isolate on the island of Hokkaido. In North America, Clade 4 bears probably extend no farther north than central Alberta and British Columbia, which makes them the most fragmented Clade in North America, and collectively as threatened as the Clade 1 bears of Europe and Clade 5 and 6 bears of southern and southeastern Asia. The details of all this are elaborated in Attachment 2, which I have included both as substantiation for my comments and to assist the authors of the Rule, who are apparently unacquainted with the referenced body of literature on brown bear taxonomy and biogeography.

As brief background, Clade 4 brown bears were probably the first representative of *Ursus arctos* to migrate across Beringia into North America, certainly before 30,000 years before present, and perhaps as early as 70,000+ years ago. By all indications, bears of this clade were then isolated south of the continental ice-sheets for 10s of thousands of years. As the ice sheets melted during the early Holocene, Clade 4 bear migrated north where they encountered more recently arrived Clade 3 brown bears moving south from Beringia. With arrival of Europeans, Clade 4 grizzlies bore the brunt of post-1800 extirpations, and are unique among all North American Clades in being the most diminished of all. Yellowstone's grizzlies currently represent the southernmost relic of Clade 4 brown bears.

The Service does not make reference to this unique evolutionary and biogeographic circumstance of Yellowstone's grizzly bears anywhere in the current delisting package. This is a major omission. Nor does the Service anywhere make mention of the unique and much diminished status of Clade 4 grizzly bears. If duly considered, all of this information would logically support a larger vision for recovery of this Clade, including explicit provision for connectivity of Yellowstone's grizzlies with other Clade 4 populations farther north, which the Rule does not currently include. Moreover, such a perspective would lead to recognition of the special taxonomic and historical status of Yellowstone's grizzly bears, all of which would argue for a more cautious approach than is currently being taken by the Service.

In short, the Service needs to update its taxonomic reference for Yellowstone's grizzly bears; describe the unique evolutionary and biogeographic status of this population; develop an explicit plan for recovery of at least the US portion of Clade 4 grizzly bears, including an explicit provision for connectivity amongst all representative populations; and put the move to delist Yellowstone's grizzly bear population on hold until such plans and provisions have been developed.

In making this recommendation I realize that I am calling into question the very standards by which the Service has chosen to judge recovery of Yellowstone's grizzly bears, which is a logical consequence of the Service's failure to update the Recovery Plan for this population so as to reflect the massive amount of "best available science" that has been produced since the early 1990s. In other words, this critique is a commentary on the Service's failure to exercise due diligence, including following through with its own 2011 recommendation to update and revise the Yellowstone grizzly bear Recovery Plan.

---

7. The Service fails to account for the nutritional ecology of grizzly bears in its assessment of recent dietary shifts because it inaccurately, incompletely, and erroneously represents the best available science. This arises from the extent to which the Service engages in tortuous logic and selective even grossly incomplete representations of relevant research. Because of this, the Service reaches patently erroneous conclusions.

The Service's representation of nutritional ecology is a critical aspect of its overall argument that historical and prospective food losses, and resulting dietary shifts, "do not and will never pose a threat" to Yellowstone's grizzly bears. The Service claims that grizzly bears are not just omnivorous, but "*extremely* omnivorous," and, on top of that, "display *great* diet plasticity." As evidence of this claim, the Service then cites Gunther et al. (2014), that Yellowstone's grizzlies are documented to eat over "260 species of foods…representing 4 of the 5 kingdoms of life." Thereafter the Service asserts outright or by innuendo that one food is basically as good as another—this despite giving lip service on page 13178 to the fact that energy concentration and nutrient content vary among foods—and that Yellowstone's grizzly bears are thereby well able to substitute one food for another. The Service then goes on to presumably substantiate this assertion by offering presumed evidence for how losses of whitebark pine and cutthroat trout have not affected birth and death rates of the population.

Put bluntly, the Service's treatment of nutritional ecology reads more like propaganda than it does like a deliberative consideration of the best available science to arrive at a prudent conclusion. In other words, the Service misconstrues, misrepresents, and altogether misses a considerable body of relevant science in apparent service of reaching a preordained/pre-decisional outcome. The short-comings of the Services representations are so egregiously deficient that I feel compelled to offer a corrective primer on nutritional ecology (hereafter, the Primer; Attachment 3) as an attachment to these comments. But more specifically:

The nutritional quality of foods available to and eaten by grizzly bears in the Yellowstone ecosystem varies by orders of magnitude. Mattson et al. (2004) as well as the Primer provide a summary of digestibilities and protein content for Yellowstone's bear foods. In other words, all foods are not equal insofar as digestible protein and energy are concerned. Moreover, and perhaps more importantly, bear foods are especially disparate in concentrations of dietary fat. Fat is perhaps the most important of all nutrients to bears (Erlenbach et al. 2014), and is uniquely abundant in army cutworm moths, whitebark pine seeds, and late-season ungulates such as elk and bison (Mattson et al. 2004, Erlenbach et al. 2014).

On top of this, the density, architecture, and comparative nutrient contents of foods have a major effect on foraging efficiencies, efficiencies of weight gain, and accretion of fat versus lean body mass among bears, with subsequent effects on mass dynamics during and after hibernation (The Primer; e.g., Farley & Robbins 1995; Atkinson et al. 1996; Welch et al. 1997; Hilderbrand et al. 1999a, 1999b; Rode & Robbins 2000; Rode et al. 2001; Felicetti et al. 2003; Robbins et al. 2007; McLellan 2011; Robbins et al. 2012; Erlenbach et al. 2014). Generally speaking, smaller bears fare better than larger bears on fleshy fruits and grazed foliage, whereas larger bears disproportionately benefit from eating meat (The Primer). Even so, all bears are beset by a need to balance the energy and protein concentrations of their diet to maintain lean body mass and accrue fat (The Primer). On top of this, the energetic costs of extraction vary among foods substantially, to the extent that extraction costs largely negate the greater digestible energy available in most root foods (Holcroft & Herrero 1984, Mattson 1997a, Mattson et al. 2004).In other words, foods are of varied relative benefit to bears based on body mass, sex, and overall diet composition. Not all foods are equal, nor are all foods equal to different bears.

Parenthetically, and at risk of stating the obvious, bears are not Latin taxonomists. In other words, bears are not wandering around in the woods with a copy of the *Flora of the Pacific Northwest* keying out different foods before they eat them—say, differentiating one Poa from another, or Poa from wheatgrass. In other words, taxonomic distinctions have little or no relevance for grizzly bears. As per what I outline immediately above, differences among foods arise from characteristic densities and architectures at foraging sites, the energetic expenses of extraction, and per gram densities of energy and nutrients—and how all of this varies seasonally and from one year to the next. In other words, the fact that Yellowstone's grizzly bears eat "260 species of foods…representing 4 of the 5 kingdoms of life" is largely irrelevant, disingenuous, and prey to artificial inflation based on the resolution of taxonomic distinctions employed (e.g., should we be talking about genera, or species, or even subspecies?).

With this as background, it is worth looking critically at where Yellowstone's grizzly bears obtained most of their energy and nutrients—say, between 1977 and 2003—and the comparative importance of vegetal foods that the Service in places features so prominently as potential generic dietary alternatives to whitebark pine seeds and cutthroat trout; and elk, for that matter (see   below).

Figure 7.1 immediately below offers a seasonal picture of relative contributions of different foods to digested energy and ingested protein and fat of Yellowstone's grizzly bears for the period 1977-1993, differentiating contributions from large herbivores (elk and bison; Mattson 1997b), cutthroat trout, and whitebark pine seeds. This representation is based on a sample of >6000 bear feces collected over a 16 year period throughout the Yellowstone ecosystem, and corrected for differential detectabililties (Hewitt & Robbins 1996) and digestibilities (Mattson et al. 2004) of each food, and then further adjusted to reflect the proportional numbers of bears out of dens (Haroldson et al. 2002) as well as varied levels of monthly feeding activity (Mattson et al. 1991a). But this comes with a proviso. Consumption of army cutworm moths is underrepresented in this sample of feces because of the remoteness of the sites where this food is eaten by grizzlies.

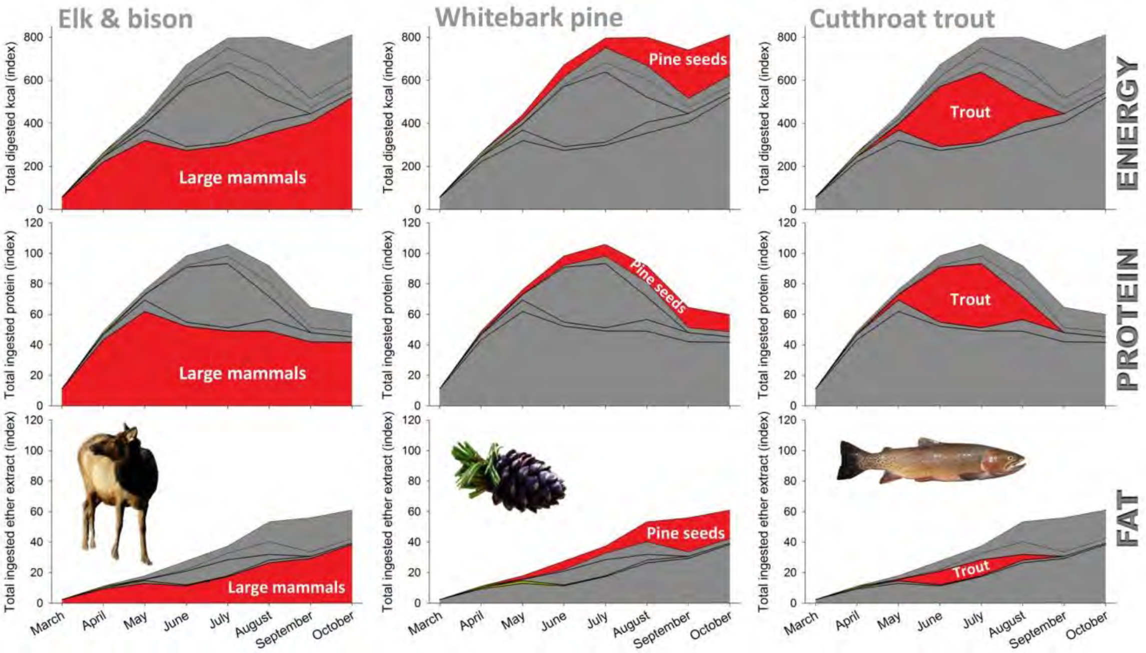


Figure 7.1. Seasonal fractional contributions of ungulates (elk & bison, left), whitebark pine seeds (center), and cutthroat trout (right) to the 1977-1992 diet of Yellowstone's grizzly bears, differentiating digested energy (top row) from ingested protein and fat (middle and bottom rows, respectively). This representation accounts for detectabilities, digestibilities, and monthly population-level differences in overall consumption and is based on a corrected sample of >6000 grizzly bear fecal remains. Army cutworm moths are under-represented because of sampling issues.

The key point of this graph is that ungulate meat, cutthroat trout, and whitebark pine seeds (plus army cutworm moths, more on that later) historically contributed the overwhelming majority of energy and nutrients to Yellowstone's grizzly bear diet. This representation is broadly consistent with similar estimates corrected for detection, digestibility, and prevalence made by Mattson et al. (2004:26-28) and Lopez-Alfaro et al. (2013). Parenthetically, the major contribution of ungulate meat to Yellowstone's grizzly bear diet correlates well with other independent estimates made on the basis of feeding site examinations (Mattson 1997b) and analysis of isotopes in tissues collected from captured or killed bears (Jacoby et al. 1999; Schwartz et al. 2014).

Insofar as army cutworm moths are concerned, the evidence for their dietary importance is more circumstantial, but nonetheless compelling. First, when the composition of feces collected on or near moth sites is corrected for differential passage through the digestive tract, moths comprise 80-90% of the total (Mattson et al. 1991b). Second, the numbers of bears seen on moth sites has climbed steadily since the mid-1980s and currently accounts for hundreds of sightings (300-400 recently) and, since 1996, roughly 15-45% of all initial sightings of unduplicated females with cubs-of-the-year (IGBST 2015), the range depending on the specific year. Third, high levels of bear activity on moth sites are sustained over a 2-month period, from roughly mid-July through mid-September (see my point 11.5). Taken together, this evidence is consistent with moths being a major food for grizzly bears within range of moth sites.

Put another way, given the overwhelming reliance by Yellowstone's grizzly bears on essentially four foods (lumping elk and bison together as ungulates), major losses of any one are almost certainly to have major impacts. The Service confesses to major losses of cutthroat trout and whitebark pine. Moreover, as I point out below, elk populations have declined substantially since the mid-1990s, with prospects of army cutworm moths being hit hard by climate warming (see my point). This begs the question of whether plausible alternative foods are (and would be) of sufficient quality, simply from a nutritional perspective, to compensate for the losses of the euphemistic "big 4" that we've seen.

Figure 7.2 summarizes information on the net digested energy estimated to be obtained by Yellowstone grizzly bears from five different groups of foods on a per gram basis and from a given feeding bout (or feeding site). These data come from (Mattson et al. 2004) and differentiate the 'big 4" (ungulates, cutthroat trout, whitebark pine seeds, and army cutworm moths) from other categories including foods that might be included in dietary shifts in response to losses of current dietary mainstays.

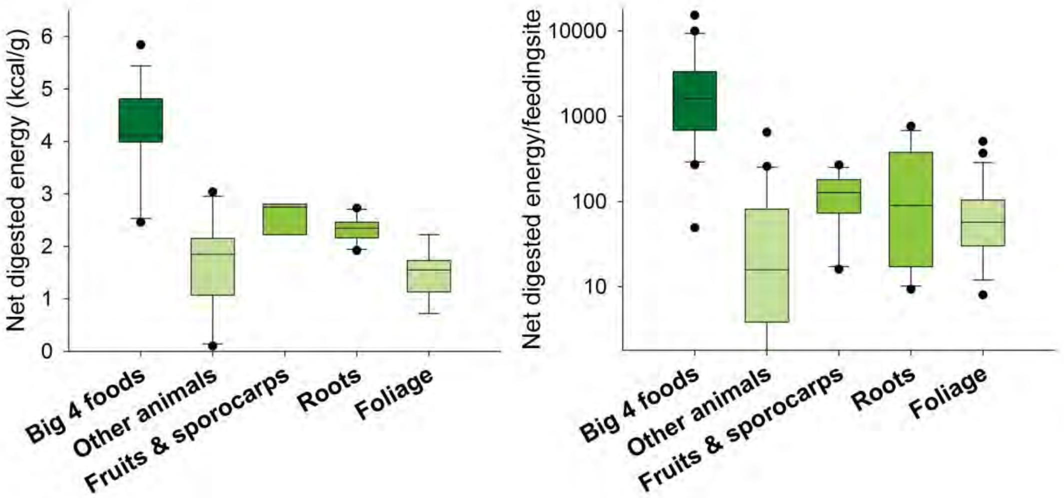


Figure 7.2. Box and whisker diagrams showing the energetic benefits of different categories of foods, including the "big 4" (ungulates, army cutworm moths, whitebark pine seeds, and cutthroat trout) and other categories of foods that include items plausibly turned to in compensation for losses among the "big 4."

The pattern is pretty obvious. Even accounting for outlier items, the "big 4" are energetically superior to any other foods that might be used by bears in compensation for losses, including other animal foods (e.g., rodents and ants) and fruits and fungi (i.e., sporocarps); and this without taking into account the landscape-level abundance of foods or the extent to which they occur at sites in such a way as to favor efficient use (which relates to the density, architecture, and ease of extraction issues).

There is another important point that further belies any tacit or outright claims by the Service that changes in food abundance can be blithely accommodated by "extremely omnivorous" and "resilient" grizzly bears. This point relates to continent-wide differences in grizzly bear densities and the extent to which these densities reflect differences in habitat productivity. Perhaps the seminal paper putting all of this together is Mowat et al. (2013). These authors show that grizzly bear densities systematically vary by orders of magnitude as a direct function of habitat productivity. The most obvious difference is between coastal areas with spawning salmon and interior areas without. But, even in interior areas, densities can, again, vary by orders of magnitude in reflection of various factors that are surrogates for overall productivity.

The final point of relevance here is that the Service's argument of potential last resort has no merit. In other words, these comments extensively cover the Service's numerous fatal errors and failings that debar any dismissal of changes in foods and diet as drivers of historical and prospective future changes in demography of Yellowstone's grizzly bears. In fact, there is ample evidence for detrimental effects arising from losses of whitebark pine, cutthroat trout, and elk, including major increases in mortality arising from the greater reliance of bears on meat—which has included consumption of livestock.

8. The Service fails to account for the effect of recent widespread losses of whitebark pine by its reliance on fatally flawed science produced by the IGBST. This IGBST research fails to account for temporal and spatial variation in availability of whitebark pine seeds as well as temporal and spatial variation in other critically important bear foods. As a result, no confidence can be placed in conclusions reached by the Service regarding the effects any driver of grizzly bear birth and death rates, including losses of whitebark pine.

8.1. The Service fails to accurately account for the spatial distribution of cone-producing whitebark pine trees when making its many claims regarding the lack of historical importance of pine seeds. This failure follows, in part, from the Service's near exclusive reliance on spatial analyses produced by the IGBST that were based on a map of whitebark pine distribution containing substantial errors of omission.

The IGBST used a map of whitebark pine distribution for its spatial representation of whitebark pine forests that was derived from remote sensing. This map was used for analyses of grizzly bear demography, home ranges, and habitat selection (Costello et al. 2014, Bjornlie et al. 2014, Van Manen et al. 2015), and was the basis for the Service's claims that roughly 23-33% of all recent bear ranges were comprised of <1% whitebark pine forest.

Put succinctly, the map of whitebark pine distribution used by the IGBST is contaminated by large errors of omission. Even more egregiously, this map shows large areas as being without mature whitebark pine which, in fact, contained significant enclaves of mature cone-producing trees. These omissions are the entire basis for claims made by Bjornlie et al. (2014), Costello et al. (2014) and later by the Service in its Rule and CS that a significant percentage of grizzly bear ranges lacked access to cones produced by mature whitebark pine trees. Quite simply, these claims are unfounded and false.

I base this conclusion on comparing the distribution map used by the IGBST with the distribution of sites where grizzly bears were documented by the IGBST to feed on whitebark pine seeds between 1977 and 1996 (Fig. 8.1.1). Feeding on whitebark pine seeds was documented by field crews during the course of investigations that involved humans beings standing on the ground, looking at the remains of whitebark pine cones savaged by grizzly bears, and then looking upwards at the canopy, almost invariably to observe mature whitebark pine trees. Or, put another way, compared to remotely-sensed maps, these ground observations are a more definitive reckoning of, not only the presence of cone-producing whitebark pine, but also the presence of these trees in sufficient numbers to support grizzly bear consumption of pine seeds.

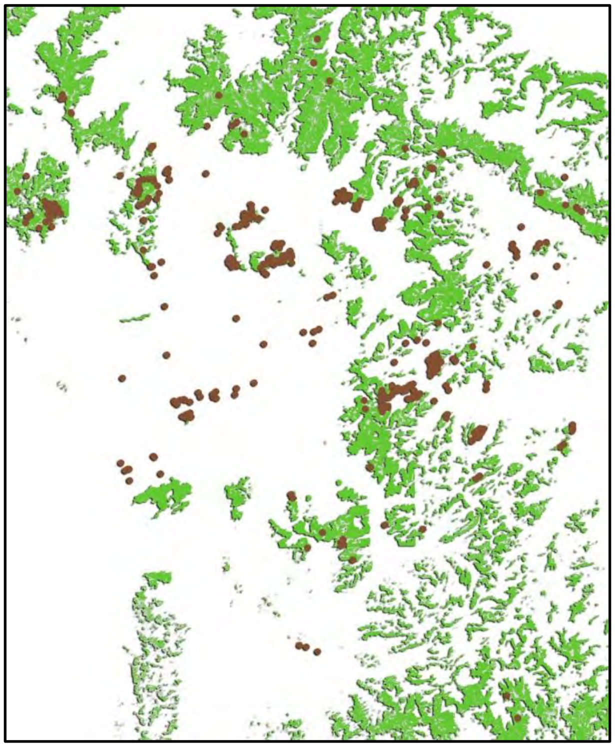


Fig. 8.1.1. This map shows the distribution of known instances where grizzly bears fed on whitebark pine seeds (dark brown dots) relative to the mapped distribution of whitebark pine used by the IGBST in recent analyses relied upon by the Service (in green). The numerous feeding sites far outside the mapped distribution correspond with the central plateaus of Yellowstone National Park.

The map in Figure 8.1.1 shows the distribution of known grizzly bear feeding on pine seeds as dark brown dots superimposed on the map of whitebark pine distribution that the IGBST used in analyses undertaken by Bjornlie et al. (2014), Costello et al. (2014), and Van Manen et al. (2015) shown in green. The mapped distribution contains only 42% of all known instances of grizzly bear feeding on pine seeds. Being generous and assuming, for inexplicable reasons, that IGBST field crews had a systematic tendency to erroneously locate feeding sites as much as 200 m outside the mapped distribution of whitebark pine, the level of inclusion increases to only 63%. Fully 25% of feeding sites are located >650 m away from the nearest mapped whitebark pine. Importantly, almost all of the pine seed feeding sites missed by the mapped distribution are located at lower elevations, and of those missed by the farthest distance, almost all are on the central plateaus of Yellowstone National Park, which is precisely where Bjornlie et al. (2014), Costello et al. (2014), and the Service (repeatedly) claim that grizzly bear home ranges contained little or no mature whitebark pine.

In short, Bjornlie et al. (2014), Costello et al. (2014), and the Service, in turn, are simply wrong in claiming that 23-33% of historic grizzly bear ranges contained little or no whitebark pine and, from that, further claiming that whitebark pine was unimportant to a corresponding percentage of bears. In fact, very few grizzly bear home ranges probably contained little or no whitebark pine. The exact percentage is still unknown given that I do not have access to the home range delineations used by IGBST scientists, but this percentage is almost certainly trivial. In addition to the error made by Bjornlie et al. (2014) in their temporal representations of pine seed availability (see below), this additional error makes the Bjornlie research and related Service claims based on it baseless. Furthermore, the major errors of omission in the map of whitebark pine distribution used by Costello et al. (2014) and Van Manen et al. (2015) introduce yet more error and bias into these researchers' analyses, on top of the fatal errors introduced by their treatment of annual variation in availability of whitebark pine seeds (see below).

8.2. The Service fails to account for the impacts of recent widespread losses of whitebark pine on Yellowstone's grizzly bears because it relies on science that is fatally flawed. This IGBST science conflates the extent of whitebark pine forests with levels of cone and seed availability, thereby misrepresenting periods of pine seed abundance as periods of pine seed shortage, and periods of pine seed shortage as periods of pine seed abundance.

At the risk of stating the obvious, Yellowstone's grizzly bears do not eat whitebark pine trees, as such. Rather, they eat the seeds contained in whitebark pine cones, most of which they obtain by raiding larders made by red squirrels (e.g., Mattson & Reinhart 1997). As a result, any annual or even multi-annual representation of whitebark pine seed availability to Yellowstone's grizzly bears needs to be in terms of cone or seed production, not the number of cone-producing trees, as such, or even the aerial extent of forests containing mature whitebark pine. Yet the IGBST conflates temporal trends in abundance of mature trees with seed production in its recent analyses of the effects of whitebark pine on grizzly bear demography (e.g., Van Manen et al 2015).

This matters because during and after massive numbers of mature trees had died from an outbreak of mountain pine beetles, median annual cone production apparently increased by as much as 2.5-fold on surviving trees. Figure 8.2.1 shows counts of cones on whitebark pine trees monitored on fixed transects by the IGBST, averaged per annum over all monitored trees. The running three-year average of annual values is shown as a yellow-green line, which is relevant because of the somatic and behavioral averaging that goes with the three-year reproductive cycle of Yellowstone's female grizzly bears. In addition, I show medians of annual averages for three time periods corresponding with breaks in long-term trends. Of relevance to my point here, the median for the period 2006-2014 is roughly 2.5-times greater than the median for either 1982-1995 or 1997-2004. In other words, at the same time that we were losing—or had lost—perhaps the majority of cone-producing trees in the ecosystem (Macfarlane et al. 2013), cone production on the remaining trees increased by over 2-fold.

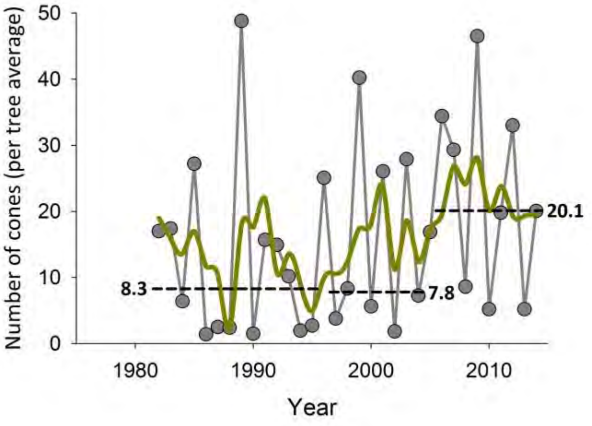


Figure. 8.2.1. Average number of cones counted on whitebark pine trees at fixed transects by the IGBST between 1982 and 2014 (gray dots) along with a 3-yr moving average of these values (yellow-green line) and medians calculated for annual values for three different time periods (dashed lines).

As noted before, the IGBST purports to represent availability of pine seeds with remotely-sensed estimates of the aerial extent of mature whitebark pine trees. These estimates are shown in Figure 1 of Van Manen et al. (2015). One way to integrate information about the size of cone crops on surviving trees with an estimate of the numbers of such survivors is to simply multiply the index generated by Van Manen et al. (2015) by the average number of cones on live trees at fixed transects, as per Figure 8.2.1. This logically produces an index of the total landscape-level abundance of seed-containing cones available to Yellowstone's grizzly bears during any given year. This is, in fact, the relevant metric for use in any analysis of bear demography or movements given that, as I noted before, bears eat seeds not trees. The results of this metric, along with an annually averaged representation of the data in figure 1 of Van Manen et al. (2015), are shown in Figure 8.2.2: the index of cone availability as the dark yellow-green line and the index of tree abundance as the gray line.

There is one important error to note even in the way that Van Manen et al. (2015) represent the aerial extent of whitebark pine forests: they neglected to include the considerable losses that occurred during 1988 as a result of massive wildfires. The constant abundance that they show between 1983 and roughly 2000 should actually drop by around 17% (3-50%, depending on the precise area; Mattson 2000) between 1988 and 1989. In addition to blatantly ignoring cone production, this is itself a non-trivial error even in the metric they do use—an error that I correct in generating the estimate of total seed availability.

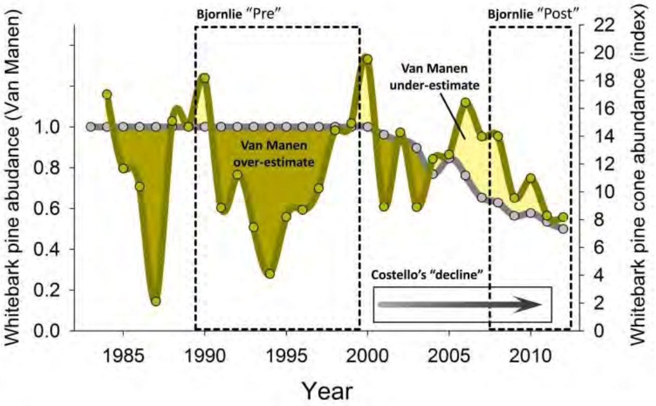


Figure. 8.2.2. The IGBST index of mature whitebark pine is shown by the dotted gray line; the index of landscape-level cone availability, accounting for losses of trees to bark beetles and the 1988 wildfires, is shown by the dotted green line; and time periods relevant to errors in three key IGBST research publications are denoted by the first author's name and, in the case of Bjornlie et al. (2014), by dashed boxes, in the case of Costello et al. (2014), by an arrow, and, in the case of Van Manen et al. (2015), by the extent of erroneous over- and under-estimations.

But more importantly, the index of cone availability shows a dramatically different inter-annual pattern compared to the index of rote tree availability. More specifically, pine seeds were least available during two early periods that the IGBST claimed were typified by the greatest abundance of whitebark pine. By contrast, the large average increase in cone crops on surviving trees largely mitigated losses to bark beetles that occurred during the early 2000s. It was only after 2006 that losses of trees to beetles began to swamp the effects of larger cone crops, resulting in the onset of a terminal decline. More to the point here, the IGBST under-represented availability of pine seeds during the early 2000s by its reliance on an index of tree abundance.

These errors are fatal and, more specifically, render the results of Bjornlie et al. (2014) and Van Manen et al. (2015) meaningless if not downright wrong. Both papers are central to the Service's arguments in the Rule and CS. The IGBST misrepresented early periods of cone shortage as periods of cone abundance and later periods of cone abundance as periods of cone shortage. They and the

Service got things pretty much entirely backward, barring the terminal decline beginning in 2006.

Figure 8.2.2 shows in more detail the implications of this problem for the three referenced papers. The analysis of home ranges by Bjornlie et al. (2014) was based on the premise that the period 1988-1989 was a period of pine seed abundance, and the period 2007-2012 a period of pine seed dearth. In reality there was greater pine seed availability during the later period compared to the earlier periods. They got it entirely backwards. Likewise, the analysis of demography by Van Manen et al. (2015) grossly over-estimated pine seed abundance during 1983-1989 and substantially underestimated pine seed abundance during 2005-2010, which turned their assumptions upside down.

Having made this point in reference to the veracity of recent IGBST analyses and the Service's arguments that derive from them, I need to at the same time make clear that losses of mature whitebark pine trees are real and on-going (e.g., Logan et al. 2014). Looking to the future, even though losses have been largely masked by increases in per tree cone production—especially during the early 2000s—such natural mitigations will eventually run the course. When that happens, consequences will almost certainly unfold that are even more dramatic than any we have so far seen. In fact, the record number of grizzly bears dying during 2015 is a likely preview of more of the same to come.

---

9. The Service fails to account for—or even acknowledge—convincing evidence of major deleterious consequences arising from losses of whitebark pine in the Yellowstone ecosystem. Most prominently, these consequences include compensatory increases in consumption of meat from ungulates by Yellowstone's bears and, with that, dramatic increases in meat-related bear-human conflicts and resulting human-caused bear deaths.

Throughout the Rule and the CS the Service freely asserts that losses of whitebark pine have had little impact on the Yellowstone grizzly bear population. These assertions are based wholly on fatally-flawed IGBST science and on the Service's own misrepresentations of the nutritional ecology of bears. I critique the Service's deficient representation of what we know about nutritional ecology elsewhere (point 7). Points 8 and 10, immediately before and following, describe fatal flaws in recent IGBST research that renders this science useless at best, but which the Service liberally invokes as a primary basis for its many correspondingly flawed arguments. In short, the Service has no basis for dismissing the potential impacts of losing whitebark pine in the Yellowstone ecosystem.

Perhaps more important, the Service does not acknowledge nor present the compelling body of evidence showing major deleterious changes in grizzly bear behavior and demography coincident with terminal losses of whitebark pine—and cutthroat trout—in the Yellowstone ecosystem. These changes include increasing consumption of meat from terrestrial sources and, along with this, a dramatic increase in meat-related conflicts with people, leading to increases in numbers of human-caused deaths. Moreover, the decline in cub and yearling survival rates that the Service attributes to "density-dependent effects" (citing IGBST [2012] and Van Manen et al. [2015]) is more plausibly attributable to the consequences of reproductive females eating more meat in the wake of trout and whitebark pine losses. All of these changes have ultimately manifested in a dramatic rise in known and probable grizzly bear deaths that correlates perfectly with terminal losses of whitebark pine, and which amounts to a substantial increase in grizzly bear death rates at a time when the population has reached stasis and even begun to decline (see point 19.4).

Taking each of these points in turn, there is ample evidence of grizzly bears eating more terrestrial meat coincident with losses of whitebark pine and cutthroat trout. Mattson (1997) first noted that grizzlies in Yellowstone tended to eat more meat during years of poor whitebark pine seed crops—this at a time when Jacoby et al. (1999), and Mattson (2000) showed that, compared to females, male grizzly bears ate up to twice as much terrestrial meat, and all of this prior to widespread losses of whitebark pine that began during the early 2000s.

Figure 9.1. This map shows the current distribution of Yellowstone's grizzly bear population in relation to livestock-related conflicts that occurred during 2012 (each represented by a cattle skull) and the distribution of moth sites (encompassed by yellow-green blobs). Grizzly bear distribution is differentiated by the area occupied by bears prior to circa 2000 in green and areas occupied since then in orange.

In the wake of losses of cutthroat trout during the late 1990s and early 2000s and whitebark pine during the mid- to late-2000s, overall consumption of terrestrial meat has trended upward (Schwartz et al. 2014, Ebinger et al. 2016), with much of that increase fueled by increased predation on elk calves (Fortin et al. 2013, Middleton et al. 2013) and scavenging of offal from elk killed by big game hunters (Orozco & Miles 2013). Even more consequentially, grizzly bear exploitation of livestock has increased dramatically, almost all of it concentrated on the periphery of the ecosystem in areas first occupied by bears after the mid-1990s and early 2000s—and much of this figuratively downslope from moth sites also first occupied during this same period (see maps of conflicts presented each year in IGBST Annual Reports; also, see the map in figure 9.1 at left which shows data for an emblematic year together with locations of moth sites). As important for the population, differences between the sexes in consumption of meat have diminished (Fortin et al. 2013, Schwartz et al. 2014) presumably as females increasingly resort to eating meat in the wake of losing other key foods.

In short, grizzly bears in the Yellowstone ecosystem seem to be eating more meat from elk and livestock, with most livestock consumption concentrated on the ecosystem periphery, and with little current difference in dietary meat between males and females. And meat now potentially accounts for the majority of energy and nutrients for both sexes.

Nutritionally, this dietary shift has probably not been detrimental. Mattson et al. (2004) and the Primer on Nutritional Ecology attached to these comments clearly show that meat is a high-quality bear food, and that during fall ungulates can offer bears a substantial quantity of fat. As a result, there is no reason to expect major changes in female fecundity, which is consistent with recent data on reproduction (e.g., litter sizes as reported in IGBST Annual Reports).

The problem with meat consumption arises from the entailed hazards for any grizzly bears engaging in this activity (see my point   related to a frame that jointly accounts for both hazards and nutrition). Figure 9.2 shows, at top, trends in numbers of grizzly bears dying because of conflicts with big game (primarily elk) hunters together with trends in number of hunters afield and, at bottom, trends in numbers of livestock-related human-bear conflicts and numbers of bear dying because of these conflicts. All of these data come from IGBST databases or Annual Reports. Each figure also shows terminal declines in whitebark pine cone/seed availability that I describe more fully under point 8.2.

The temporal correlation between declines in cone availability and stark increases in meat-related conflicts and deaths is quite good. Given that bears seem to be compensating for losses of whitebark pine by eating more meat, these astounding increases in meat-related grizzly bear deaths can hardly be considered a coincidence.

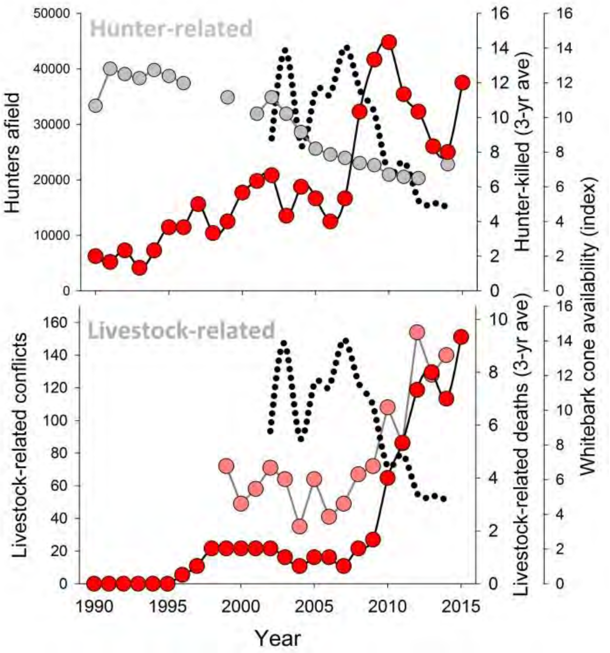


Figure 9.2. Trends in numbers of grizzly bears killed (top) because of conflicts with big game hunters (red dots) and (bottom) conflicts over livestock (red dots as well). Gray dots in the top graph show numbers of hunters afield and pink dots in the bottom graph, total numbers of livestock-related conflicts. The black dotted lines show trends in whitebark pine cone availability since onset of losses to bark beetles.

On top of this, rather than being ascribed to "density-dependent effects," the increased death rates of cubs and yearling seen recently in Yellowstone are more plausibly ascribed to the increased hazards for these vulnerable young bears arising from their mothers eating more meat. (Parenthetically, see my critique of how the Service uses the concept of density-dependence under point 4). These hazards for young bears derive partly from the human-associated hazards incurred by their mothers, but also from hazards associated with increased predation by wolves and other bears (for more on this, see my point 17).

As a bottom line, dramatic increases in total known and probable grizzly bear deaths in the Yellowstone ecosystem—both natural and human-caused—are clearly correlated with terminal declines in availability of whitebark pine seeds (Figure 9.3). Much of this increase is equally clearly driven by the consequences of dietary shifts entrained by loss of whitebark pine, as per the shift to eating more meat.

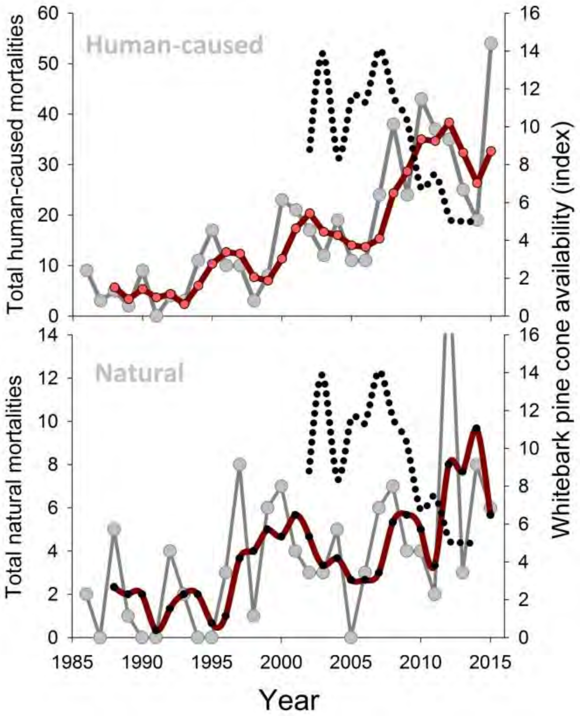


Figure 9.3. Trends in total numbers of known-probable human-caused grizzly bear deaths, top, and probable natural deaths, bottom. The gray lines are annual counts and the red trend lines running 3-year averages. The black dotted lines show trends in whitebark pine cone availability since onset of losses to bark beetles.

Taken together, this constitutes compelling evidence for substantial deleterious population-level effects arising from loss of whitebark pine (and cutthroat trout) in the Yellowstone ecosystem, with more foreseeable negative effects yet to come (see my critique of the Service's treatment of trout, ungulates, whitebark pine, and moths, especially in relation to climate change). Quite simply, the Service has no credible basis for dismissing losses of whitebark pine as being inconsequential. In fact, the Service's representation of this issue appears arbitrary and capricious. The Service needs to correct what amounts to an egregious error in its analysis supporting this Rule.

10. The Service fails to account for major changes in abundance of other key bear foods besides whitebark pine because of its reliance on unsubstantiated rhetorical arguments and fatally deficient IGBST research in the Rule. This IGBST research does not include any explicit consideration of an epic decade-long drought or variation in abundance of cutthroat trout, elk, bison, and army cutworm moths in its analyses of movements and demography. The Service consequently has no scientific basis for any claims regarding drivers of change in the demography and movements of Yellowstone's grizzly bears.

The Service relies heavily—almost exclusively—on recent research published by the IGBST. The Service invokes this research as its primary basis for claiming that recent losses of whitebark pine have not resulted in any changes in the demography of Yellowstone's grizzly bears. The three seminal papers cited repeatly on pages   of the Rule are Bjornlie et al. (2014), Costello et al. (2014), and Van Manen et al. (2015), the first dealing with changes in home ranges, the second, changes in movements and habitat selection relative to whitebark pine and secure habitats, and, the third, changes primarily in death rates. The first and last conclude that losses of whitebark pine to bark beeltes, 2001-2012, had no effect, and that all changes could be attributed to changes in bear densities. Notably, these papers claim to have looked at both spatial and temporal variation—but limited only to considerations of density and extent of whitebark pine forests.

Elsewhere I address fatal errors in how the IGBST—and Service—addressed availability of whitebark pine seeds to grizzly bears both in space and time (point 8). I also cover problems with both the IGBST's density argument and density index under a different comment (point 4). Here I point out another fatal flaw in the IGBST's and Service's analysis that is rooted in lack of attention to a number of major changes in Yellowstone's grizzly bear habitat—in addition to changes in whitebark pine abundance. All of these ignored dynamics are of *prima facie* importance given that they pertain to availability of patently important grizzly bear foods, including elk, bison, cutthroat trout, army cutworm moths, and foods affected by levels of drought (also see my point 11.1).

Before delving into particulars of the neglected factors, it is worth reiterating my first comment (1.1): most of the science produced by the IGBST and invoked by the Service entails complex models and complex assumption-ridden statistical methods. Such is the case for all of the results germane to judging the demographic plight of Yellowstone's grizzly bears. Yet it is well known to even half-way thoughtful scientists that there is no one correct model, and that model-building is perhaps the most vagarious and bias prone of scientific undertakings. Moreover, any credible inference based on complex models of open ecological systems depends almost entirely on insuring that all factors likely to have had a significant effect on outcomes of interest (i.e., changes in demography)were accounted for. Isolating the effect of any single factor such as bear density or whitebark pine abundance depends upon controlling for the effects of every other factor of plausible importance.

In other words, by failing to account for a number of plausibly important changes in Yellowstone's grizzly bear habitat, the IGBST—and Service—have no basis for making any claims about affects of density or whitebark pine on grizzly bear birth and death rates, and this aside from the fatal flaws in how they putatively addressed the factors they did consider.

Figures 10.1 and 10.2, below, provides some specifics. I show trends in known important foods over time, encompassing the span of relevance to the various analysis undertaken by the IGBST. These foods, top to bottom, include size of whitebark pine cone crops (not extent of whitebark pine forests), bear use of army cutworm moth sites, numbers of spawning cutthroat trout, numbers of elk in the ecosystem's two largest herds, and, finally, at bottom, numbers of bison in Yellowstone's two herds.

These figures also show how time period was treated in the three seminal IGBST papers, denoted by the vertical bars shaded different colors of orange. Bjornlie et al. (2014) assumed two time periods, one before (pre) and one after (post) major losses of whitebark pine to beetles, assuming that the only changes in the Yellowstone environment between these two periods were extent of whitebark pine forests and bear density.

The vertical shading in the graph farther right shows how Van Manen et al. (2015) approximated the presumed decrease in whitebark pine forests with ever-darker hues of orange—and again, without considering any other environmental trends. Finally, the center graph shows the time period addressed by Costello et al. (2014) as a shaded vertial box, and, again, this reseearcher assumed that the only change afoot pertained to whitebark pine.

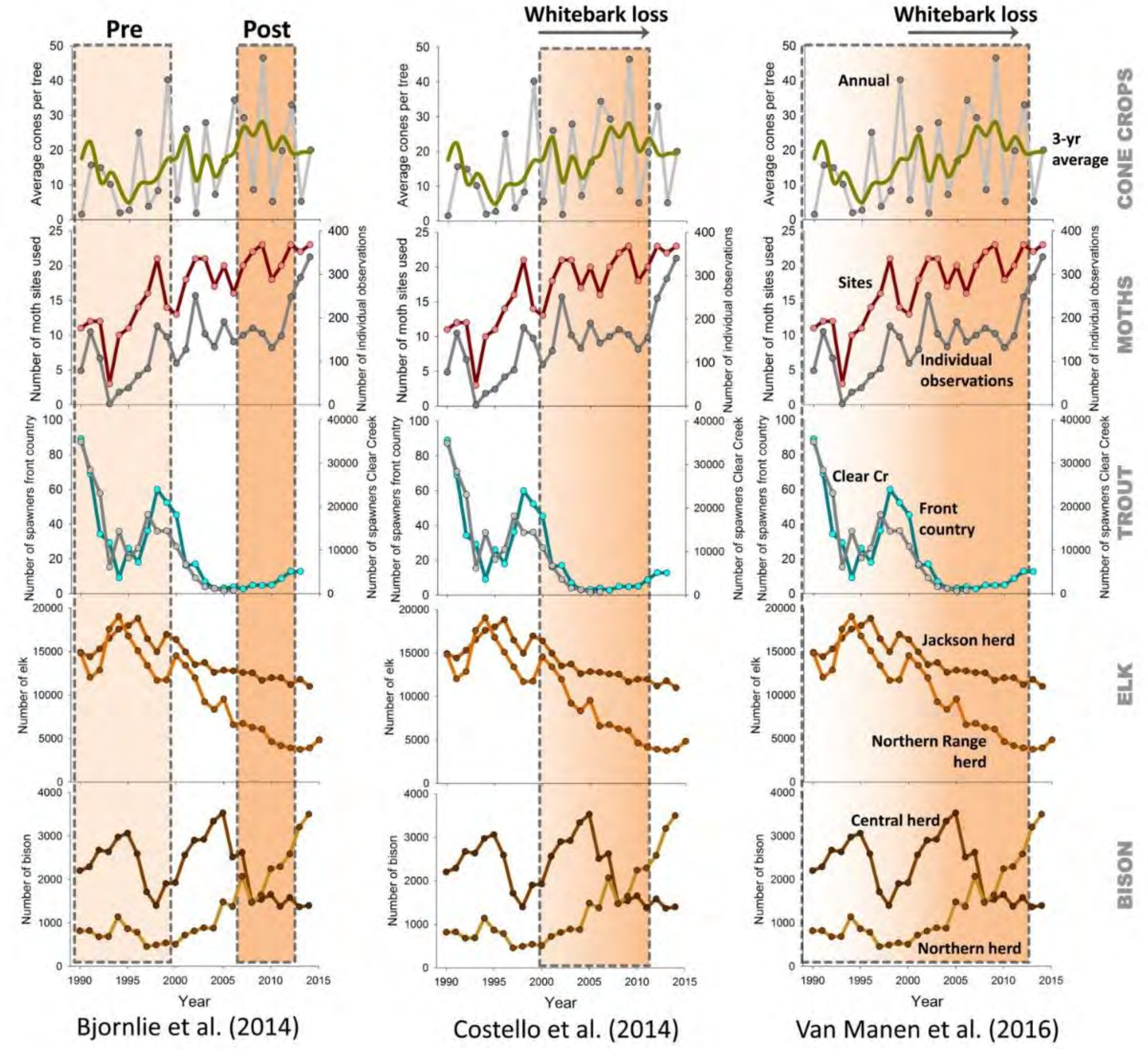


Figure 10.1. Trends in indicators of abundance for key Yellowstone grizzly bear foods are shown by rows, from top to bottom, intersected with rows denoting how three different analyses reported by the IGBST treated time periods, show as vertical boxes shaded various hues of orange. Progressively darker hues of orange denote less versus more whitebark pine, at least as treated in the IGBST analyses.

The takeaway from figure 10.1 is pretty straight-forward. The IGBST failed to account for major unfolding trends in food abundance. Bjornlie et al. (2014) failed to account for the fact that elk and cutthroat trout had declined and moth site use and whitebark pine cone crop sizes had increased between the ‘pre’ and ‘post’ periods. Likewise, Van Manen et al. (2015) neglected to account for these very same trends as continuous temporal (and spatial) phenomena. Costello et al. (2014) failed to account for trends towards increasing sizes of whitebark pine seed crops, increasing levels of moth site use, and decreasing numbers of elk. All of these failures fatally compromise these analyses.

Figure 10.2, below, makes the added point that these three IGBST papers not only failed to account for trends in abundance of important foods, but also neglected to account for some longer-term tends in weather, with near certain effects, in turn, on other vegetal and animal foods. More specifically, Bjornlie et al. (2014) failed to account for a higher average summer temperatures during their ‘post’ period; Van Manen et al. (2015) for an epic drought and for a trend towards progressively higher summer temperatures; and Costello et al. (2014) for the gradual recovery from deep drought. All of these patterns plausibly affected grizzly bear movements and diet.

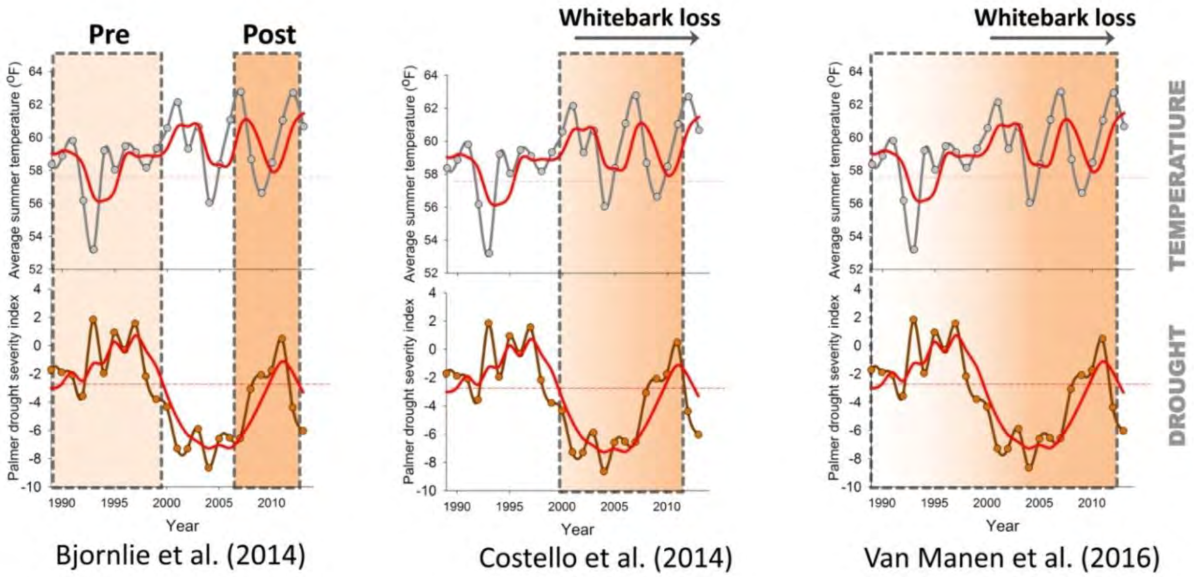


Figure 10.2. The basic configuraiton here is the same as in figure x.1, but featuring trends in summer temperature (top row) and drought severity (bottom). The red lines are 3-year moving averages and the gray dots, annual values. Drought is indexed by the Palmer Drought Severity Index. All data are publicly available from NOAA.

Finally, in addition to fatal neglect of temporal trends, the IGBST’s science—and Service’s derivative claims—fail to account for potential interactions between spatial distributions of and temporal trends in key food resources. This specific critique is relevant both because the IGBST pretends to deal with spatial aspects of food availability (albeit limited to whitebark pine) and because interactions among environmental factors in space and time are a very real consideration in any analysis of complex ecological systems. And, in fact, there are many potential interactions that were altogether neglected in analyses by the IGBST and Service.

Figure 10.3 illustrates a subset of potential interactions (the legend for this figure explains the various map features). For one, moth sites (the grayish-green blobs) correspond almost exactly with where we have seen the greatest losses of whitebark pine (in gray). In other words, availability of moths has very likely partially compensated for the severe losses of whitebark pine that occurred in this area. For another, as per trends for Yellowstone’s two bison herds shown in figure 10.3, bison have declined in precisely the same areas (the Central herd) where near extirpation of cutthroat trout also occurred, resulting in an amplification of the effects of trout losses. By contrast, the Northern Range bison herd (in green) has increased, with some likely compensation for losses of whitebark pine and declines in elk numbers in the northern part of the ecosystem.

As a bottom line: by failing to account for both the temporal and spatial aspects of major changes in the environment of Yellowstone’s grizzly bears the Service fatally compromises its assessment of past and prospective future changes in demography and behavior of Yellowstone’s grizzly bears. A prudent course of action would be for the Service to withdraw this draft Rule and remedy the profound deficiencies in its analysis and the science it has so far relied upon.

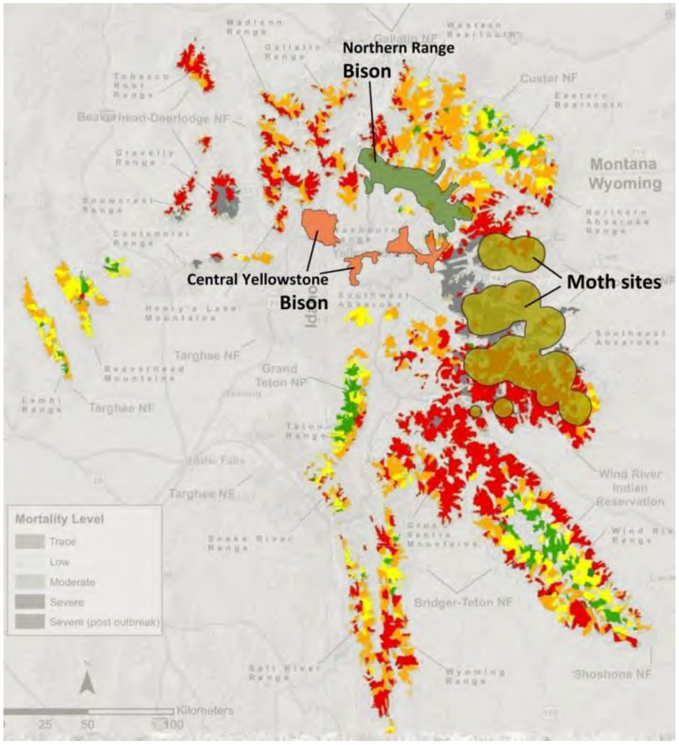


Figure 10.3. Distributions of whitebark pine forests, moth sites, and core bison ranges in the Yellowstone ecosystem. Whitebark pine forests are shown in various colors corresponding to the magnitude of mortality from bark beetles as of 2009; gray indicates near total mortality, red, high levels, green, healthy forests, and yellow and orange somewhere in between (Macfarlane et al. 2013). All known moth sites are encompassed by buffers denoted by yellowish-green shading. The Northern Range bison herd range is shown in green and the Central bison herd in off-orange.

10. The Service's assumption that maintenance of a simplistic version of the 1998 human footprint will insure sustainable future human impacts is flawed for a number of reasons, including failure to account for changes in bear behavior in response to habitat conditions, changes in human lethality, and changes in edge effects attributable to development on lands adjacent to the PCA.

In multiple places the Service asserts that maintenance of certain GIS-delineated human features within the PCA will insure conservation of the Yellowstone grizzly bears into the indefinite future by maintaining demographic characteristics that ostensibly prevailed during a period of sustained population growth. The spatial attributes that presumably govern grizzly bear demography include road densities, the extent of areas >500m away from human infrastructure (so-called "secure" habitat), and numbers of livestock allotments.

This all-critical assumption that a static few GIS-delineated landscape features will insure anything into the indefinite future fails for numerous reasons, including (1) over-stated population growth during the invoked benchmark period of 1988-1998; (2) failure to account for a critical sources of human-caused mortality that can vary independently of the chosen few landscape features; (3) failure to account for changes in bear behavior driven by changes in diet and food availability that will affect how often bears encounter the mapped lethal human features—independent of any change in these features; (4) changes in the lethality and behavior of people both in relation to and independent of mapped and monitored features; (5) changes in numbers of people on, off, and nearby to the mapped features, with resulting effects on encounter rates between bears and people; and (6) related edge effects associated with human population growth and residential and recreational developments on private lands near and inside the PCA. Taking each one of these in turn:

1. As I describe in my critique of the currently-adopted Chao2 method for monitoring trend of Yellowstone's grizzly bear population (point 19.1), this method has almost certainly overstated growth, including for the

period 1988-1998. Which means that, although some growth was very likely happening, is was of a lesser magnitude than being asserted—and relied upon—by the Service in the Rule. As a practical matter, this translates into less of a buffer on this basis alone than the Service would have us believe (but see all my following points).

Perhaps more importantly, the 1998 footprint has, in fact, been associated with a range of demographic conditions, including (according to the Service) a decline in population growth rate to the point where the population has not changed in size for roughly 15 years. In fact, as I argue elsewhere (point 5.3), the population has likely declined during the last decade and has perhaps passed a tipping point. The entire premise of the Service's argument is rendered implausible by the fact that a wide range of human-caused grizzly bear mortality rates has been encompassed by a set of landscape conditions presumably designed to guard against such amplitude (see 19.4).

2. Currently, the primary human-related causes of grizzly bear mortality in the Yellowstone ecosystem are: availability of attractants at residences and recreational developments; conflicts over livestock; and conflicts with big game hunters. Additional minor causes include collisions with vehicles and removals because of more direct human-safety concerns. *As a spatial phenomenon*, most of these causes are covered by the Service's monitored features, but with the important exception of big game hunters. Hunters are particularly lethal to grizzly bears and, more importantly, often distributed in a way that is poorly correlated with roads, "secure" habitat, and livestock allotments. The upshot is that the monitoring spatial features provide a poor basis for monitoring what's happening with numbers and distributions of a certain class of human that is a major cause of grizzly bear mortality.

3. As I describe at length elsewhere in this critique (and as the Service also avers), the diet of Yellowstone's grizzly bears has changed substantially, with prospects of even more change to come. These changes organize around loss of cutthroat trout and whitebark pine seeds in some of the most secure habitats in the ecosystem, along with prospective losses of army cutworm moths, also in remote secure areas. There is little doubt that, among other things, grizzly bears are turning to eating more meat, including livestock and scavenged offal left by hunters. As a consequence, grizzly bears are spending more time in highly lethal environments typified by grazing allotments and areas used by big game hunters (e.g., my points below; Haroldson et al. 2004). Use of alternate foods also seems to draw bears more often into less secure habitat nearer roads and other human facilities (Haroldson & Gunther 2013, Costello et al. 2014), as has been the case since the 1980s (Mattson et al. 1992). Even farther back in time, closure of garbage dumps in and around Yellowstone National Park during 1959-1962 had profound impacts on how grizzly bears distributed themselves relative to a comparatively static human infrastructure, but with catastrophic consequences for the bear population (Craighead et al. 1994). The main point here is that grizzly bears can end up being exposed much more often to lethal human-associated features as a result of habitat and diet changes and without any spatial change in the extent of human facilities or livestock grazing allotments—as per the 1998 human footprint.

4. Augmenting issue 3, a given number of people can change how they orient to mapped and monitored spatial features and, perhaps more important, can become more or less per capita lethal to bears. This point is historically emphasized by the fact that grizzly bears would have probably come closer to near-total extirpation in the Yellowstone ecosystem during the early to mid-1900s but for the fact that aggregate human lethality changed, and in spite of increasing human numbers (Mattson & Merrill 2002). More recently, as the Rule describes, human lethality has been reduced by instituting measures that reduce availability of human-associated foods around human facilities, thus reducing human lethality—all without any explicit correlation with roads and grazing allotments. We can only hope that humans continue to become ever less lethal, ever more assiduous in managing attractants, and continue to concentrate as much on roads, but simply monitoring roads and livestock grazing allotments will not provide any information on this critical dimension of the human footprint.

5. As the IGBST recognizes through its monitoring of big game hunters afield on national forest lands, and human recreational activity in Yellowstone and Grand Teton

National Parks, numbers of people matter to grizzly bear conservation, even given a fixed human infrastructure. It is thus probably relevant that numbers of visitors to Yellowstone Park increased by roughly 400,000 between the 1990s and 2010s, although remaining nearer the same in Grand Teton (IGBST 2015). But of perhaps even greater relevance is that visitation to these parks has trended ever upward from 2005 to the present (2,868,317 to 4,097,710 in Yellowstone; 2,463,442 to 3,149,921 in Grand Teton), meaning ever more people on roads and at recreational facilities, which no doubt creates mounting challenges for managers attempting to control attractants and risky human behaviors—but with no change in the physical infrastructure. Here, again, numbers alone matter. More directly, the management challenges that sheer numbers pose matter, with prospects of affecting grizzly bears that are exposed to areas near the human infrastructure.

6. It perhaps goes without saying (and as the Rule describes), population growth and associated development on private lands in the Yellowstone ecosystem continues at a rapid but, more recently, accelerating pace, with development disproportionately concentrated near protected areas and in productive riparian habitats (e.g., Gude et al. 2006). Strangely, the Service fails to feature research by Schwartz et al (2012) that explicitly estimates the impacts of projected exurban development on Yellowstone's grizzly bears, differentiating impacts by whether they will occur in the PCA or outside in occupied grizzly bear habitat. "Boom" or even status quo growth could bring major loss of source habitats, contributing to even more habitat fragmentation than exists now (see my comments under point 14). These results are summarized in figure 10.1 immediately below.

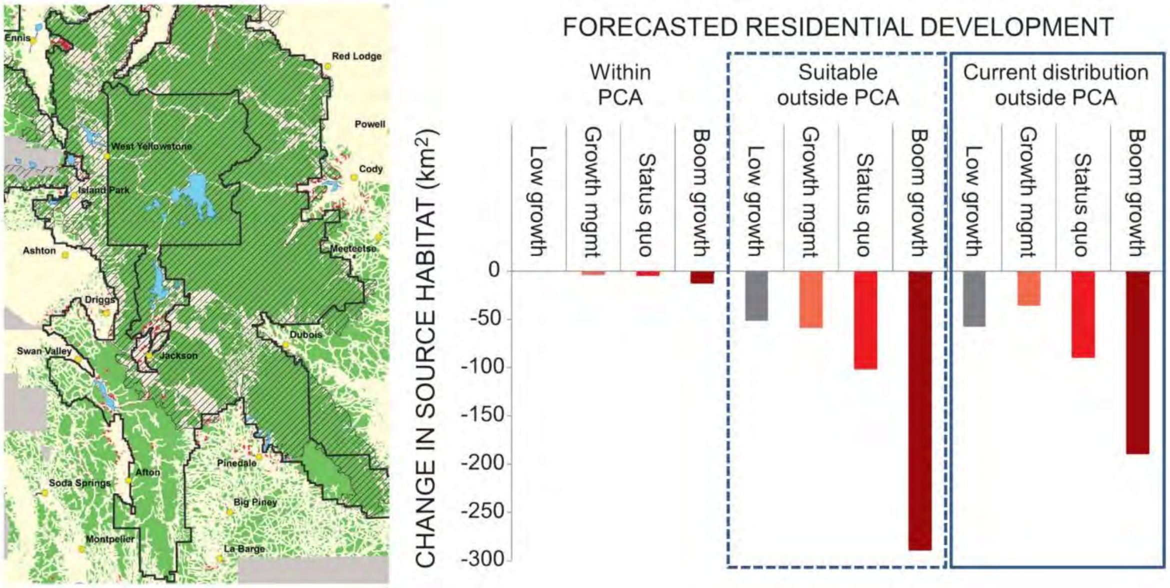


Figure 10.1. Figures extracted from Schwartz et al. (2012) pertaining to projected effects of exurban development on Yellowstone's grizzly bear habitat. The beige in the map at left corresponds to projected human-impaired habitat. The bar graph to the right shows loss of source grizzly bear habitat in and out of the PCA under four different scenarios for growth of the human footprint.

The Service liberally employs the notion of 'edge effects' as a basis for excluding habitat near sheep allotments in the Wind River Range from its classification as "suitable" habitat. The Service's usage in that circumstance is problematic. However, edge effects are a very real consideration in relation to on-going and foreseeable exurban development adjacent to the PCA, with the possibility of transboundary effects plausibly attributable to the boundary-spanning movements of both bears and people. The map at left in figure 10.1 shows this potential in graphic form, with projections of lethal habitat shown in beige. The point of this being that effects of mounting human activity on private lands in and near the PCA will engender largely negative effects that will not be accounted for in the 1998 benchmark pegged to federally-managed lands. The Service engages in what largely amounts to a bunch of hand-waving on pages 13199-13200 of the Rule about how extrajurisdictional activities on the part of state and federal managers will somehow mitigate for this. Such claims are unsubstantiated. Moreover, there is precedent for federal managers undertaking mitigation on public lands for harm arising from activities on nearby private holdings (e.g., the 1997 Swan Valley Grizzly Bear Conservation Agreement). In other words, the Rule's current arm-waving is not good enough in light of prospective trends in private land development in the Yellowstone ecosystem.

Parenthetically, the Service will probably assert in response to my critique of their 1998 benchmark that, even if valid, there are other safeguards in place in the form of methods for monitoring and managing mortality. As I hope is clear from the remainder of my critique, there are, in fact, critical failings and short-comings affecting all of these safeguards that debar them from providing a reliable backstop.

11. The Service fails to meaningfully account for past changes in major Yellowstone grizzly bears foods; this on top of the problems with its analysis regarding whitebark pine. Among key foods that the Service essentially dismisses out of hand are elk, bison, cutthroat trout, army cutworm moths and all of the vegetal foods that are affected by growing season drought. In short, not only does the Service fail to provide any useful basis for assessing past and prospective future changes in natural foods, but, even more problematic, fails to acknowledge and address clear scientific evidence that the Yellowstone grizzly bear population is in trouble and threatened by further deterioration of habitat conditions.

11.1. The Service fails to use or even acknowledge scientific evidence showing that there is *prima facie* reason to expect that availability of ungulates, army cutworm moths, cutthroat trout, and drought have affected grizzly bear death rates, with prospects of leading to elevated death rates over the next decade to century.

On page 13212 of the Rule the Service references a single research paper of only peripheral relevance (Schwartz et al. 2010) as a basis for asserting that "…only whitebark pine seeds are known to have an influence on grizzly bear mortality risk and reproduction. There is no known relationship between grizzly bear mortality risk or reproduction and any other individual food." The Service then uses this assertion to dismiss out-of-hand any serious consideration of demographic consequences arising from past and likely future trends in any food source other than whitebark pine. In rushing through these bemusing leaps of logic the Service altogether ignores a trove of scientific information relevant to judging whether other foods might—or even do—have an effect on birth and death rates of Yellowstone's grizzly bears. When taken seriously, this body of science paints a fundamentally different picture—which is the picture I paint here in my following points as well as throughout the rest of my comments.

It is first worth noting that the Service fails to explain how "…some of the highest calorie food sources available to grizzly bears in the GYE"—and of enough importance to be monitored by the IGBST (page 13212)—can be dismissed essentially out of hand. Or, related, why the ample trend data from foods important enough to be monitored year-after-year by multiple state and federal agencies is not worth the Service's consideration (as with army cutworm moths, cutthroat trout, and ungulate carrion). Even without pursuing this issue any further, these twists of logic suggest an up-front disinterest by the Service in seriously engaging with the effects of foods such as elk, bison, army cutworm moths, and cutthroat trout—or all of the other foods that might be affected by drought.

Moreover, it takes convoluted logic to conclude that "there is no known relationship" simply because the Service chooses to rely solely on fatally flawed science that failed to consider the possibility of such relationships in the first place (see my points 2.2 & 10 above). Failure on the part of a few scientists to exercise adequate scientific rigor does not constitute a thorough and sufficient examination of the science relevant to judging why birth and death rates of Yellowstone's grizzly bears might have changed in the past, and how these rates might be affected in the future.

Beyond these logical problems, one important point to reiterate (see my point 7) is one that the Service itself makes in several places, including on page 13212. There are, in fact, only a handful of foods from which Yellowstone's grizzly bears historically obtained perhaps as much as 80% of their energy and nutrients: meat from elk and bison; meat from cutthroat trout; seeds from whitebark pine; and army cutworm moths. As the Service notes in its (far from adequate) review of nutritional ecology, other foods the bears ate aplenty, but none came even close to providing the same amount of energy, protein, or fat as the four dominant foods. On this basis alone there is ample reason to expect that all of the four key foods—not just whitebark pine seeds--affected birth and death rates of Yellowstone's grizzly bears in some measure. Certainly, on the basis of energetics and nutritional considerations alone, such a conclusion is more defensible than any other, including the conclusion hastily reached by the Service.

And, as I point out in numerous other places, it is fundamentally problematic that ALL but one of the analyses relating food abundance to grizzly bear demography in the Yellowstone ecosystem failed to even consider the effects of moths, trout, elk, or bison—or any other food for that matter (Mattson et al. 1992, Pease & Mattson 1999, Schwartz et al. 2006, Van Manen et al. 2015). The only food effect that was entertained by these researchers was that of whitebark pine seeds. It is no wonder that if you don't look for or otherwise even consider additional effects, you probably won't document any. And, importantly, such a failure of scientific rationality does not equate to the absence of such effects—such as potentially those of trout or moths or elk or bison on grizzly bear birth and death rates. In fact, the single more replete analysis (Mattson 2000), detected effects of meat-eating on cub survival and root consumption on litter size.

More positively, though, there is evidence that death rates of Yellowstone's grizzly bears have varied in reflection of moth, elk, and trout abundance—also perhaps as a function of drought—especially between 1990 and 2005. Elsewhere I present definitive evidence that an increase in consumption of meat by Yellowstone's grizzlies as compensation for losses of whitebark pine seeds led to the sharp increases in natural and human-caused mortality since 2006 (see my points 9, 17, & 18). But here I focus on 1990-2005 using publicly available data from the IGBST, the National Park Service, the states of Wyoming and Montana, and NOAA, realizing that a rigorous analysis of all data currently sequestered by the Service would also entail spatial and temporal interactions.

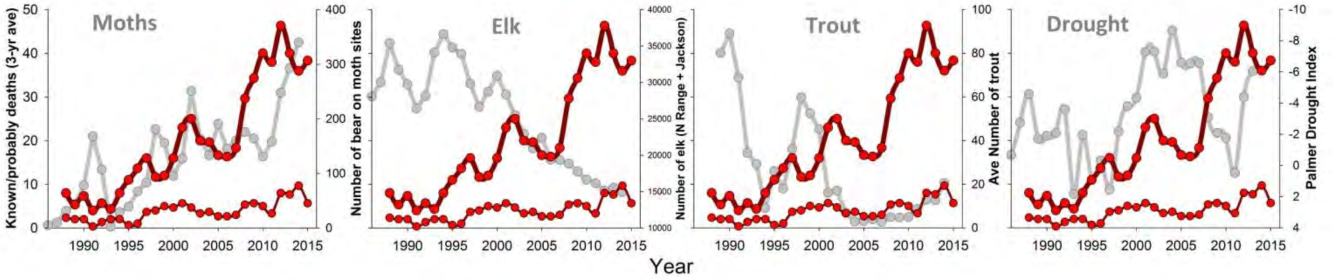


Figure 11.1.1. These four panels superimpose trends in total known-probable grizzly bear mortality on trends in abundance of four foods plus drought for the period 1986-2015. Mortality is shown in red as a 3-year running average, with natural mortality (lower red line) differentiated from total mortality (top red line). The gray lines show trends, left to right, in number of bears seen on moth sites; numbers of elk in the Northern Range and Greater Jackson herds; average per stream numbers of trout counted in spawning streams around Yellowstone Lake; and an inverted representation of the Palmer Drought Index for the highest-elevation drainages of the Yellowstone ecosystem (i.e., peaks in the trend line indicate greater drought severity).

Figure 11.1.1 shows trends in total known and probable grizzly bear deaths in red as a 3-year running average superimposed on trends in foods, or food proxies, all of which are currently neglected by the Service.

Put simply, trends in human-caused and total grizzly bear deaths can be largely explained in terms of trends in availability of key foods. For example, a simple regression model containing trends in elk populations and availability of whitebark pine seeds (see my point 8.2) explains 74% of total and 76% of human-caused mortality (with P-values <0.0001)—with both food effects negative, as one would expect. A similar model including elk, trout (also negative), and drought (positive) explains 70% of human-caused mortality (also with P<0.0001). Another model yet, in which I used latent variables to deal with the high degree of temporal correlation among covariates (i.e., principal components), explains 70% of total deaths and 72% of human-caused mortality, with elk, trout, and whitebark pine all having negative effects, and moths and drought, positive effects.

Parenthetically, I suspect that the anomalous *positive* relationship between grizzly bear death rates and use of moth sites is related to geography. As shown in Figure 11.1.2., all of the moth sites are on the east side and towards the periphery of the ecosystem, in areas where we have seen expansion of the grizzly bear population, and a burgeoning of livestock-related conflicts and resulting grizzly bears deaths (see my point 9). Emblematic of this, the cow skulls in figure 11.1.2 each represent a grizzly bear-livestock conflict for one representative year, of which a disproportionate number are figuratively just downslope from a moth site. The point of this being that moth sites are spatially correlated with an emerging major cause of grizzly bear deaths, and thus probably positively correlated with temporal trends in deaths largely as an artifact of this geography.

As a bottom line, there is strong evidence that all of the key grizzly bear foods have had a strong if not dominant effect on death rates, enough so that essentially all of the trends in total and human-caused grizzly bear deaths since roughly 1990 can be explained by availability of elk, cutthroat trout, whitebark pine seeds, army cutworm moths, and drought (as a proxy for other vegetal foods). On the basis of straight-forward nutritional considerations, there is also good cause to expect that these same foods have had, and will continue to have, strong effects on grizzly bear birth rates.

Quite simply, there is no excuse other than perhaps prejudice, laziness, and limited imagination for the Service's failure to consider the effects of all nutritionally important foods on birth and death rates of Yellowstone's grizzly bears. The Service needs to remedy its currently deficient analysis and seriously address past effects and future prospects for army cutworm moths, cutthroat trout, elk, and bison—as I outline in more detail in my following points.

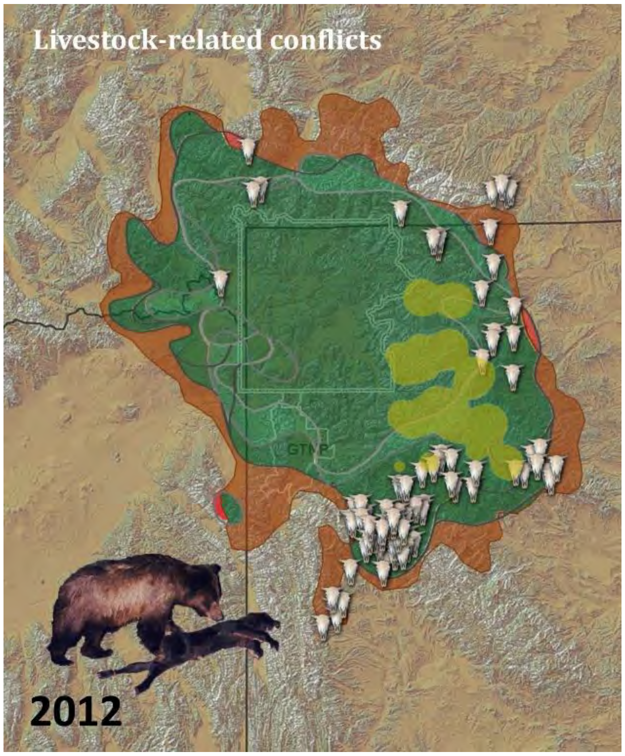


Figure 11.1.2. All of known sites where grizzly bears feed on moths in the Yellowstone ecosystem are shown here encompassed by the yellowish-green blobs. The cattle skulls each represent a location of a grizzly bear-livestock conflict during the emblematic year of 2012. The greenish shaded area represents the distribution of grizzly bears as of circa 2000 and, the orange, areas newly colonized by grizzlies during the last decade or so.

11.2. The Service fails to meaningfully account for declines in elk populations and foreseeable threats to this source of bear food in its assessment of current and prospective threats to Yellowstone's grizzly bear population. This failure further compromises the Service's attempt to disentangle drivers of recent and prospective near-future changes in demography of the bear population.

Meat from terrestrial sources has contributed between 25% and >60% of the energy and nutrients obtained by Yellowstone's grizzly bears from their environment, the range depending on the sex and age cohort and time period (Mattson 1997, Jacoby et al. 1999, Fortin et al. 2013, Schwartz et al. 2014). During the 1970s-early 1990s, elk were the source of the majority (c. 53%) of this meat (Mattson 1997). Moreover, terrestrial meat has been invoked as an important compensatory substitute for losses of cutthroat trout and whitebark pine seeds (Fortin et al. 2013, Middleton et al. 2013, Ebinger et al. 2016; the Rule). For all of these reasons—in addition to the *prima facie* evidence I present under 11.1.1 above for strong effects of elk availability on death rates—past and prospective future trends in elk populations necessarily factor into any assessment of current and future prospects for Yellowstone's grizzly bear population. Negative trends would axiomatically constitute a threat.

Despite this, the Service fails to address the past, present, and future prospects for Yellowstone's elk populations in any meaningful way. Its analysis amounts to nothing more than a patently superficial and rushed treatment on page 13212 of the Rule. Moreover, this failing is non-trivial given unfolding trends and foreseeable threats.

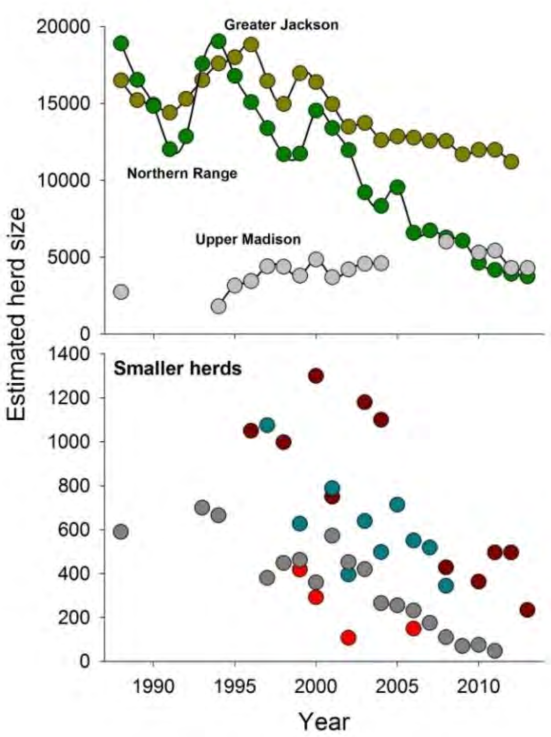


Figure 11.2.1. Trends in size of elk populations in Yellowstone's occupied grizzly bear habitat. Trends for larger herds are shown at top and for smaller herds at bottom. Because estimates are made only irregularly for the smaller herds, annual estimates are shown at dots with each color corresponding to a different herd.

Figure 11.2.1 shows trends in size of all of the elk populations in Yellowstone's occupied grizzly bear habitat (parenthetically, all of these data are publicly available from the National Park Service and the states of Wyoming, Montana, and Idaho). The point is obvious: all of the herds but one (the Upper Madison) have declined, some dramatically—even catastrophically—resulting in probable substantial increases in human-caused grizzly bear mortality (see my point 11.1. above). These herd declines closely track declines in cow:calf ratios, shown in figure 11.2.2. This latter correlation substantiates results presented by Middleton et al. (2013) strongly suggesting that declines in elk numbers are being driven partly by deteriorating summer-range conditions and related diminishment of fat reserves among female elk. Declines have also been linked to a near-tripling of mortality rates among elk calves due largely to increasing levels of bear predation (Middleton et al. 2013). Regardless of the cause, the numeric declines are dramatic and unambiguous, at the same that Yellowstone's grizzly bears have apparently become increasingly dependent on meat from terrestrial sources, while dying in increasing numbers (see my points 17 & 18 below).

Moreover, declining elk populations have almost certainly driven a decline in availability of spring carrion (figure 11.2.3), which historically has been a disproportionately important food for Yellowstone's female grizzly bears (Mattson 1997, 2000). The Service fails altogether to even recognize its own trend data regarding this phenomenon, and instead merely speculates that a reduction in spring carrion could happen, citing two papers that don't directly address these well-documented trends in carrion availability. All of this in service of rush to its ill-substantiated conclusion that "fluctuations" (euphemistically) in ungulate populations are not a threat to GYE grizzly bears.

Looking to the future, prospects are not good for Yellowstone's elk populations. If declines have indeed been driven partly by lower-quality summer forage and increased bear predation, the future can only hold worse to come. Bears are a classic case of subsidized predators when it comes to predation on ungulates calves, best documented in the case of moose and elk (e.g., Gasaway et al. 1992, Ballenberghe & Ballard 1994, Testa 2004, Zager & Beecham 2006). Which means that bear predation can persist at unsustainably high levels even as elk populations continue to decline. Climate change also promises to bring ever worse summer drought (Chang & Hansen 2015)—with predictably negative effects on the quality of elk summer forage. Moreover, despite being dismissed out of hand by the Service on page 13212, Chronic Wasting Disease looms as a potentially major catastrophe for elk and elk scavengers in Yellowstone's ecosystem (e.g., Jennelle et al. 2009; Wyoming Game & Fish Department 2015, 2016). Future prospects for Yellowstone's elk populations are not bright.

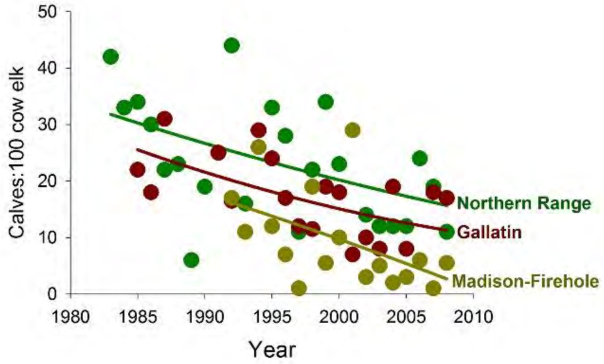


Figure 11.2.2. Cow:calf ratios for three of Yellowstone's elk herds, with each herd denoted by a different color.

That being said, my brief assessment here of status and trends for Yellowstone's elk populations is merely a preview of what is incumbent on the Service in support of this proposed Rule. The Service needs to critically assess past effects of and future prospects for this important grizzly bear food rather than breeze through a superficial analysis apparently on its way to a foregone conclusion.

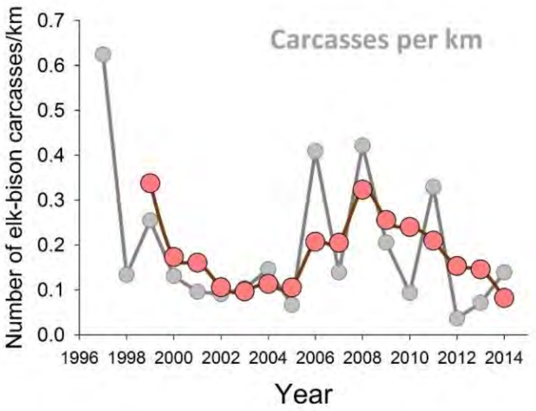


Figure 11.2.3. Ungulates carcasses counted per km of transects surveyed during spring on Yellowstone ungulate winter ranges. Gray dots are annual values and pink dots represent a 3-year running average..

11.3. The Service fails to meaningfully account for changes in bison populations and foreseeable threats to this source of bear food in its assessment of current and prospective threats to Yellowstone's grizzly bear population. This failure further compromises the Service's attempts to disentangle drivers of recent and prospective near-future changes in demography of the bear population.

Mattson (1997) and Green et al. (1997) documented the disproportional importance of bison as a source of meat for Yellowstone's grizzly bears primarily owing to the large mass of and related abundance of meat on bison carcasses. Mattson (1997) estimated that bears obtained nearly a quarter of all ungulate meat from bison. Given the increasing importance of terrestrial meat in the Yellowstone grizzly bear diet (Schwartz et al. 2014, Ebinger et al. 2016), bison have very likely become more rather than less important to this bear population. In fact, even as total number of carcasses from winter-killed elk and bison has decreased on Yellowstone's ungulate winter ranges, bison have comprised an ever-larger percentage of the total (Figure 11.3.1).

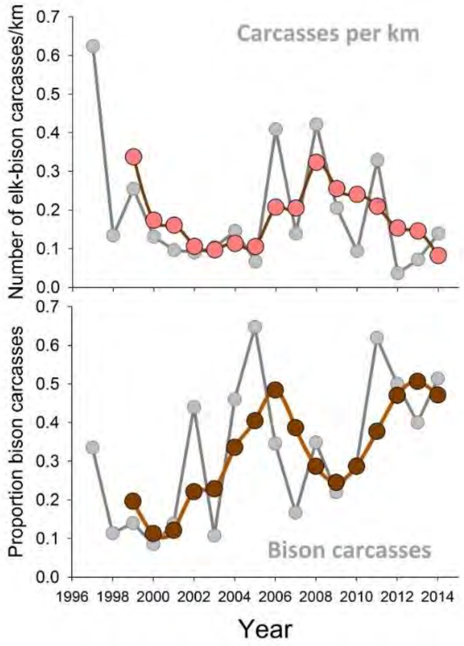


Figure 11.3.1. Results from annual spring surveys of carcasses on Yellowstone National Park winter ranges. The top graph shows number of carcasses detected per km of survey, with annual values in gray and a 3-year running average in pink. The bottom graph shows the proportion of all elk and bison carcasses that were of bison.

As with elk, because bison are such an important bear food, the Service needs to critically assess past, present, and future likely trends for bison as part of its risk assessment for Yellowstone's grizzly bear population. Yet the Service fails to present such an analysis anywhere in the Rule other than its superficial treatment on page 13212, primarily in reference to management of brucellosis in bison.

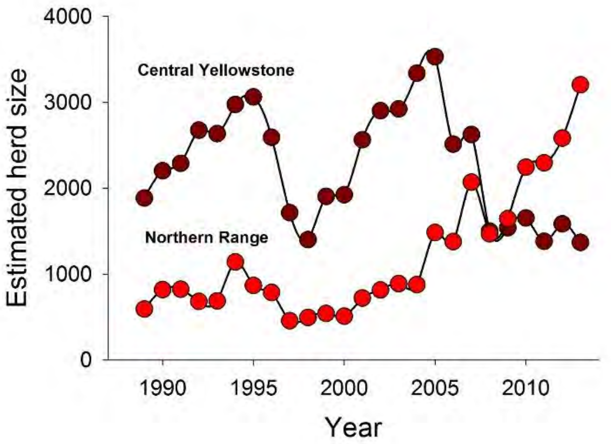


Figure 11.3.2. Trends in size of Yellowstone's two bison herds, the Northern Range herd in red and the Central Yellowstone herd in burgundy.

Elsewhere, the Service offers some offhand claims substantiated only by a distorted reading of the scientific literature, most notably its reference to Fortin et al. (2013) page 13192. Fortin et al. (2013) documented radio-marked bears feeding on bison at 9% of all investigated feeding sites in a study area restricted to the environs of Yellowstone Lake—a figure the Service then uses to dismiss bison as an important grizzly bear food. The Service's logical fallacies are emblematic of its fallacies throughout the Rule and include, first, conflating a small percentage of sites with what is, in fact, a huge amount of high-quality food, second, extrapolating results from a spatially restricted study area to an entire ecosystem, and, third, failing to note that the results of Fortin et al. (2013) pertain to the Central Yellowstone bison herd, which has declined, while altogether missing areas that coincide with the Northern Range bison herd, which has increased (see Figure 11.3.2).

Given the extent to which state and federal management of Yellowstone's bison has been driven by concerns about brucellosis, any assessment of future prospects for bison necessarily needs to meaningfully consider prospective future management regimes for control of brucellosis. The Service's encapsulation of this issue in a blithe and meaningless reference to the objective of the current brucellosis management plan (''maintain a wild, free-ranging population of bison'' [US National Park Service & USDA Animal and Plant Health Inspection Service 2000]), is grossly insufficient to the Service's analytic task. In fact, the dramatically negative recent trend of the Central Yellowstone bison herd no doubt reflects the current brucellosis management regime, more than factors such as decreasing winter severity (Geremia et al. 2011, White et al. 2011). The Service has no basis for its flip dismissal of brucellosis management as a factor in bison abundance and as a potential threat to Yellowstone's grizzly bears.

11.4. The Service fails to meaningfully assess past (and future) threats posed by major declines in populations of Yellowstone Lake cutthroat trout by its use of convoluted logic organized around a distorted and selective representation of the best available science. The Service's distortions and omissions are so strikingly egregious that I devote a considerable amount of space here to rectifying these deficiencies.

On Pages 13212 and 13213 of the Rule the Service devotes 1 paragraph to summarily dismissing the past, present, and prospective future threat posed to Yellowstone's grizzly bears by the functional extirpation of cutthroat trout in Yellowstone Lake. All of this is based on a selective and distorted reading of essentially two scientific references. In fact, an adequately thorough and less distorted review of relevant scientific information reveals a fundamentally different picture in which trout constituted an important bear food; loss of trout resulted in harm to Yellowstone's grizzly bears; with prospects for harm continuing.

Put succinctly, spawning cutthroat trout were a major source of energy and nutrients for grizzly bears living within range of Yellowstone Lake between the mid-1970s and late 1990s (see below). Roughly 15% of the bear population exploited this resource between mid-May and late July (Reinhart & Mattson 1990, Haroldson et al. 2005), although few grizzlies still consume trout. These percentages are based on estimates from the two referenced studies of 45 and 70 trout-consuming bears, and estimates of roughly 325 and 500 total bears in the population for the two corresponding time periods (Eberhardt & Knight 1995, IGBST 2003). Or put another way, the Service's bald assertion that "only a small portion of the GYE grizzly bear population uses cutthroat trout" is mystifying and unsubstantiated, especially given that the Service references the somewhat dated work by Haroldson et al. (2005) as its authority.

Figure 7.1., under Point 7 above, summarizes the estimated monthly contribution of cutthroat trout to dietary energy and nutrients of the Yellowstone grizzly bears between the late 1970s and early 1990s. These estimates are based on an analysis of fecal material ("scats") deposited by grizzly bears throughout the Yellowstone ecosystem during 1977-1993, weighted by average nutrient and energy contents, adjusted for digestibilities, and corrected for differences between ingested and fecal volumes (as per Mattson et al. 2004, Lopez-Alfaro et al. 2015; see my comments under point 7). Monthly variation in total amounts varies substantially because of seasonal differences in overall levels of feeding activity (low in the spring, high in the fall and late summer; Mattson et al. 1991a), as well as differences in numbers of bears out of their dens early and late in the active season (Haroldson et al. 2002).

Figure 7.1 shows that trout were an especially important source of energy and protein, provided in pulses that peaked during June and July. There is little doubt that the functional extirpation of trout in a number of spawning streams has left a major nutritional hole during these months that bears have needed to fill, apparently by preying more heavily on elk calves (which are particularly vulnerable this time of year) and by more heavily consuming lower-quality vegetal foods (Fortin et al. 2013, Middleton et al. 2013, Ebinger et al. 2016).

Nonetheless, it is not altogether clear to what extent cutthroat trout were an important source of energy and nutrients for female versus male grizzlies. The main contradictions arise from the results of Mattson & Reinhart (1995) and that of Felicetti et al. (2004). Felicetti's results suggest that male grizzlies made roughly five times more use of trout compared to female grizzlies (135g/kg versus 26g/kg), whereas Mattson & Reinhart (1995) suggest otherwise. A critically important contextual distinction pertains to the status of trout populations when the research supporting each result happened. The research reported in Mattson & Reinhart (1995) occurred during the peak of Yellowstone's cutthroat trout population (1985-1987); Felicetti's when the population was at low ebb, albeit during a minor resurgence (1997-2000; see my discussion of trends below).

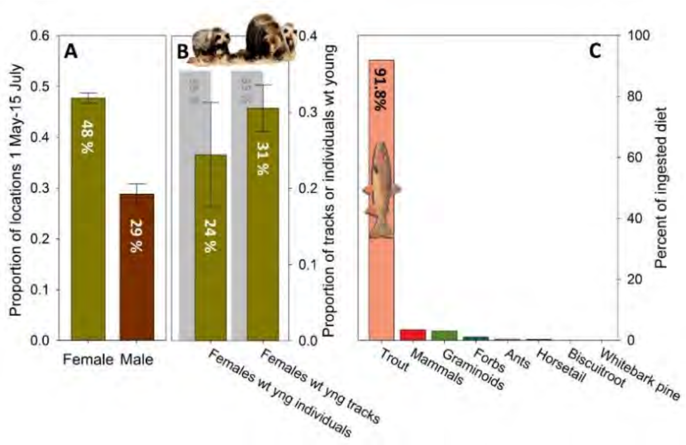


Figure 11.4.1. (A) Proportion of adult female and adult male telemetry locations concentrated within 2-km of Yellowstone during cutthroat trout spawning season, only considering bears with ranges overlapping spawning streams; (B) Proportion of all individuals and tracks documented along spawning streams during spawning season (in green) relative to the proportion of each expected by the total in the population; and (C) percent contribution of various foods to the ingested diet of bears concentrated along spawning streams during spawning season.

Some key results from the mid-1980s are summarized in figure 11.4.1. Of the bears strongly suspected of consuming cutthroat trout, females spent nearly 1.6 as much time near spawning streams as did males (during the spawning season; left above, Panel A), although females with dependent young were present in proportions less than one might expect (indicated by the discrepancy between the gray and green bars in panel B).

Moreover, analysis of scats collected within 500 m of spawning streams suggests that the bears that were present had a diet comprised of 90%+ cutthroat trout (corrected to reflect ingestion; Panel C). Although scats dropped by females couldn't be distinguished from scats dropped by males, the overwhelming conclusion is that any bear near a spawning stream during the spawning season was eating almost wholly cutthroat trout. The logical conclusion is that females were eating more trout than were males, at least during the mid-1980s. The only way this could not be the case is if females were concentrating near spawning streams, but not eating trout, which seems implausible in light of everything know about what motivates the foraging behavior of animals.

Another interesting paradox arises from the fact that Felcetti et al. (2004) estimated the median consumption of trout by bears during 1997-2000 to be around 0.024-1.09 kg per individual, at the same time that Haroldson et al. (2005; from whom she obtained her samples) estimated that bear activity around spawning streams--including fishing--had not dramatically diminished from highs during the mid-1980s (see my discussion of trends below). But the claims by Felcetti and Haroldson find little support in independent observations. For one, trout populations had declined substantially--if not catastrophically--between the 1980s and late 1990s (see below). Given the strong relationship between trout densities and bear fish activity (Reinhart & Mattson 1987), it seems implausible that such a decline in trout populations would have had a minor effect on bear activity. The claim by Felcetti that trout were roughly 5-times more important a source of energy for males compared to females is also in stark contradiction of the results described immediately above. Finally, the fact that grizzly bears have been documented to consume roughly 6 kg of trout in a single 41-minute bout of fishing suggests that any bear spending any amount of time fishing streams under favorable circumstances would have consumed many kg of trout during a single season--not something less than 1 kg.

An explanation for contradictions between the results of the mid-1980s and late-1990s studies potentially takes two forms. One is that the differences are simply a result of starkly different numbers of spawning cutthroat trout (see below), with related changes in relative access to streams by females versus males. There is ample evidence that adult male grizzlies can dominate concentrated food resources such as spawning trout, especially to the exclusion of security-conscious animals such as females with dependent young (the case two years out of three for Yellowstone females). Thus, if prime fishing opportunities had become increasingly spatially restricted, males would have become the primary winners, and females the losers. The result would have been a shift from disproportional use of the trout resource by females to disproportional use by males—leaving females bearing most of the harm arising from losses of cutthroat trout.

And, indeed, Yellowstone Lake's cutthroat trout population has exhibited huge ups and downs since the 1940s, when the Park Service first began to track trout numbers. Consumption of trout by bears has correspondingly varied. The composite figure below (figure 11.4.2) summarizes these key trends.

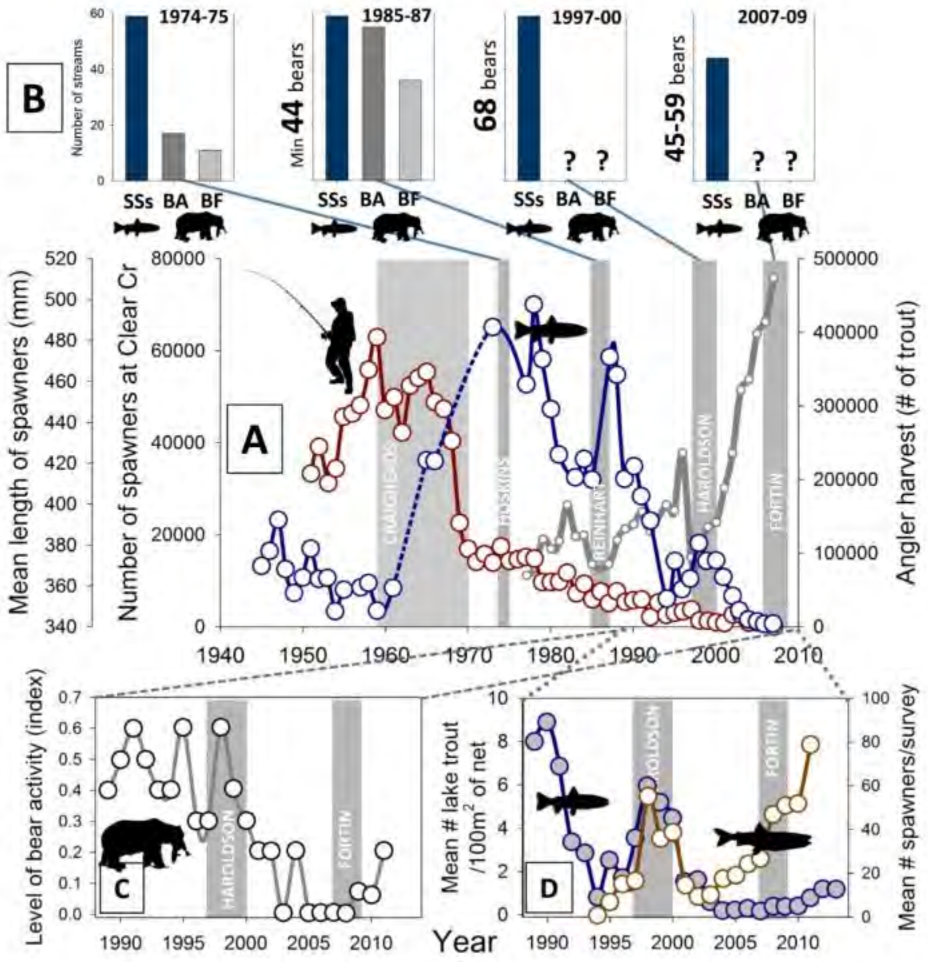


Figure 11.4.2. Trends in numbers of Yellowstone Lake cutthroat trout and related trends in related levels of bear fishing and other activity, with reference to the various studies that I reference here.

Panel A shows trends in numbers of spawning trout censused in Clear Creek, a major spawning stream on the east side of Yellowstone Lake (the blue line); the average length of trout captured in trawl nets (gray line); and numbers of fish killed by human anglers (all of these data can be found in Kaeding 2010 and other public archives). Key trends in numbers of spawning trout have been: a major increase during the 1960s, a substantial dip in the early 1980s, a resurgence in the late 80s, and a subsequent sustained decline to the present. Average size of trout has consistently varied inversely to population trends, indicating lack of recruitment of fish into smaller size classes during periods of decline.

Panel C shows levels of bear activity along spawning streams since 1989 (transects are located on streams concentrated on the west and northwest shores of the Lake; from IGBST Annual Reports). During 1989-1998 bear activity was substantial but varied. However, beginning in 1999, activity declined to the point where, now, bears rarely fish for spawning trout. This decline in bear activity is self-evidently related to the decline in numbers of spawning trout shown in blue in Panel D. Panel D also shows, in brown, numbers of lake trout captured in gill nets, standardized to reflect level of effort. Non-native lake trout were first documented in Yellowstone Lake in 1994, after which their numbers increased, then declined, and then increased again to the present. Lake trout prey on cutthroat trout and are implicated in the decline of cutthroat numbers.

One important feature of Panels A, C, and D is the denotation, as gray vertical bars, of different grizzly bear studies of relevance to documenting consumption of trout around Yellowstone Lake; each study is labeled with the name of the lead investigator(s). The Hoskins, Reinhart, Haroldson, and Fortin (Fortin et al. 2013) studies surveyed tributary streams specifically to document bear activity along with size and duration of spawning runs. Panel B at the very top summarizes the results of each of these studies in terms of: number of streams with spawning runs (SSs, dark blue bar), with bear activity of any sort (BA, dark gray bars), and with sign of bear fishing (BF, light gray bars). As Fortin et al. (2013) and Middleton et al. (2013) document, although there has been a decline in numbers of bears active around Yellowstone Lake, the decline has not been catastrophic.

But, as I point out above, the brunt of declines in trout has probably been born by female grizzly bears, resulting in a turn to eating more terrestrial meat, especially in the form of predated calves, resulting in a probable increase in cub and yearling death rates (see my point 17) and a related overall increase in total grizzly bear mortality, especially during 1993-1997 and again during 2000-2003 (see my point 11.1 above).

Perhaps more importantly, it would stretch the bounds of plausibility to conclude that carrying capacity *has not* declined in areas around Yellowstone Lake occupied by bears that had made heavy use of cutthroat trout—which is what the Service strongly implies. Carrying capacity has almost certainly declined and, more importantly, this decline has occurred in the most protected core of the ecosystem centered on the southern part of Yellowstone National Park. Combined with the effects of increasing moth site use on the eastern periphery of the ecosystem (see below), this geographic redistribution in carrying capacity will predictably draw grizzly bears into more lethal environments outside of the National Parks, in comparatively less protected parts of Wyoming.

Given the picture that I present here based on the preponderance of evidence and associated logical deductions, the Service needs to undertake a more rigorous and less patently biased assessment of the threats posed directly and indirectly by losses of cutthroat trout.

11.5. The Service fails to adequately address the effect of army cutworm moths on birth and death rates of Yellowstone's grizzly bears, and thereby fails to account for the past effects of dramatic increases in consumption of moths by bears and prospective future effects of losses of this food on Yellowstone's grizzly bear population.

Army cutworm moths are amongst the highest-quality of grizzly bear foods in the Yellowstone ecosystem (Erlenbach et al. 2014). By summer's end, over-summering moths can consist of 50-80% fat (Kevan & Kendall 1997, White et al. 1998b). This high concentration of fats predictably accelerates accumulation of body fat amongst female grizzly bears that eat them (Robbins et al. 2007, Robbins et al. 2012, Erlenbach et al. 2014) and, with that, the odds that these females will achieve the substantial body fat—in excess of 20%--needed to produce and sustain cubs (Farley & Robbins 1995, Hilderbrand et al. 2000, Robbins et al. 2012).

Moreover, the sites where grizzly bears consume moths are located in areas that are amongst the most remote from people of any in the Yellowstone ecosystem (Mattson et al. 1991b, French et al. 1994, Gunther et al. 2014). And remoteness from people has repeatedly been shown by researchers to be one of the most important spatial factors affecting survival of adult grizzly bears in and near the Yellowstone ecosystem (Mattson et al. 1996; Merrill et al. 1999; Carroll et al. 2001, 2003; Merrill & Mattson 2003; Johnson et al. 2004; Schwartz et al. 2010).

Consumption of moths by grizzly bears, which occurs primarily between mid-July and mid-September, has increased dramatically from essentially nil during the mid-1980s to high levels that have been sustained since the late 1990s (Mattson et al. 1991b, IGBST 2015:42-43). Individual sightings of grizzly bears on moth sites have numbered between 240 and 350 during the last three years (IGBST 2015:42-43), which strongly suggests that a substantial number of individual bears use this food resource—perhaps the majority of those living in eastern portions of the ecosystem. All of the 31 known sites where grizzlies consume moths are located on US Forest Service lands in the Absaroka Mountains east and southeast of Yellowstone National Park (Gunther et al. 2014). Six of these sites are located outside of the Primary Conservation Area (Van Manen, F.; personal communication).

Nowhere in the proposed Rule or CS does the Service address the prospective effects of past increases and future losses of army cutworm moths on grizzly bear demography, other than through patently convoluted logic transparently designed to sidestep this issue. None of the demographic analyses relied upon and referenced by the Service (e.g., Schwartz et al. 2006, Van Manen et al. 2015) have in any way explicitly incorporated the effects of army cutworm moth consumption in their analyses of grizzly bear birth or death rates. Lacking this foundation in analysis of prospective past effects, the Service has no basis for representing or judging what those effects have been or might be in the future. Nor do they have any basis for differentiating effects that they attribute to "density," as such, from effects of increasing consumption of cutworm moths given that both are highly correlated in time, as well as correlated in space (Gunther et al. 2014, IGBST 2015:42-43, Van Manen et al. 2015)

Lack of attention to this issue by Schwartz et al. (2006) and Van Manen et al. (2015) does not obviate the burden on the Service to consider the available relevant science, which is summarized here. Using this available science, and in the absence of anything more definitive, the preponderance of evidence supports concluding that increased availability and consumption of moths has very likely led to increased birth rates and decreased late-summer deaths rates amongst bears with access to this food, and that future loss of moths would lead to the opposite. Certainly, this conclusion is better supported than either of the two alternatives: that availability and consumption of moths has resulted in either no effect or negative effects, and that past and future abundance of this food is of little or no consequence to long-term conservation—which is essentially what the Service has claimed.

12. The Service disregards and misrepresents the best available scientific information in its assessment of the impact of wolves on Yellowstone's grizzly bears on page 13212 of the Rule—in fact, the Service essentially disregards wolves altogether as a factor to be considered in their own right.

On page 13212 of the Rule the Service presents a superficial and otherwise inadequate mini-assessment of the past and prospective future impacts of wolves on Yellowstone's grizzly bears in an apparent rush to reach its hasty conclusion that "fluctuations" in ungulate populations have not nor will ever threaten grizzly bears in this ecosystem. In fact, a clear-eyed reading of the best available science shows that wolves have very likely harmed Yellowstone's grizzly bears, albeit to an unknown extent.

Female grizzly bears historically consumed a disproportionate amount of the terrestrial meat that they ate by scavenging carrion during the spring, in contrast to adult male bears that ate disproportionately more meat during summer and fall and, of that, more by outright predation (Mattson 1997, 2000). Since the reintroduction of wolves, amounts of spring carrion have declined (figure 12.1), primarily as a result of declining elk populations (see my point 11.2 above). These declines in elk have probably been driven by a combination of unsustainable sport harvest, adverse weather, grizzly bear predation on calves, and wolf predation on elk of all sex and age classes (Vucetich et al. 2005, Evans et al. 2006, Eberhardt et al. 2007, Barber-Meyer et al. 2008, Griffin et al. 2011, Brodie et al. 2013, Proffitt et al. 2014). The point being that wolf predation has very likely played a part in very real declines in spring carrion, especially because of selection by wolves for the older and weaker elk that almost certainly would have otherwise died overwinter and been available to female grizzlies as carrion (Evans et al. 2006, Wright et al. 2006).

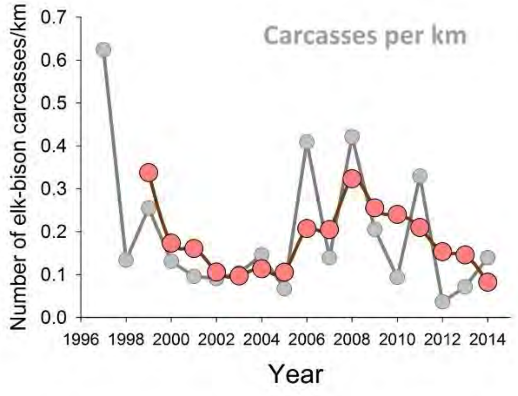


Figure 12.1. Ungulate carcasses counted per km of transects surveyed during spring on Yellowstone ungulate winter ranges. Gray dots are annual values and pink dots represent a 3-year running average.

The Service ignores this probable indirect role of wolf predation on female grizzly bears, along with an effect that is more direct. Specifically, wolves have been known to kill grizzly bear cubs (see page 13205 of the Rule), a phenomenon that is notoriously difficult to detect and thus almost certainly under-documented (see my point 17). There is no way that this predation by wolves on grizzly bear cubs can be construed as a neutral much less positive effect.

The Service concludes its putative mini-assessment of wolves by asserting that grizzly bears have probably benefitted from the extent to which they have been able to appropriate wolf kills for their own consumption, citing research conducted in Alaska (Ballard et al. 2003), while entirely overlooking highly relevant research undertaken in Yellowstone (Gunther & Smith 2004). These Yellowstone researchers found that females very rarely if ever usurped wolf kills. In fact, adult male grizzlies—which are of little relevance to population-level productivity—were essentially the sole beneficiaries of wolf kills. In other words, whatever meat wolves were directly provisioning for bears was going almost entirely into the bellies of adult males.

In short, wolves have been and will probably continue to be a detrimental feature of Yellowstone's grizzly bear habitat, thereby constituting a threat; and the Service utterly fails, first, to give this issue due regard and, second, to sufficiently and accurately represent the relevant best available science. If considered properly, wolves will probably threaten grizzly bears directly through predation on young bears and indirectly through effects on ungulate populations, now and for the foreseeable future.

13. The Service fails throughout the Rule to account for the effects of climate change on past, present, and prospective future changes in habitat and demography of Yellowstone's grizzly bears. The Service neglects most of the relevant best available science; distorts what little it does cite; deploys otherwise faulty logic; and relies, instead, largely on a single workshop publication and related assertions regarding the opinions of an amorphous "majority" of bear scientists.

The Service is remarkably obtuse in its assessments of how climate change has, is, and will foreseeably affect Yellowstone's grizzly bear population. Much of its argument is presented on pages 13197, 13216, 13217 of the Rule, from which I have extracted the following synoptic quotes:

> On page 13197, the Service cursorily states its central conclusion: "Most grizzly bear biologists in the United States and Canada do not expect habitat changes predicted under climate change scenarios to directly threaten grizzly bears (Servheen and Cross 2010, p. 4). These effects may even make habitat more suitable and food sources more abundant."
>
> On page 13216 and 13217 the Service marches through several paragraphs in which it acknowledges the obvious fact that unprecedented climate change is happening; that the western United States will be warmer; and that hydrologic regimes will change. After this pro forma recitation, the Service then leaps to again conclude: "Most grizzly bear biologists in the United States and Canada do not expect habitat changes predicted under climate change scenarios to directly threaten grizzly bears (Servheen and Cross 2010, p. 4). These changes may even make habitat more suitable and food sources more abundant."
>
> On page 13217 the Service perambulates through another pro forma acknowledgment that climate change will, indeed, affect the abundance and distribution of plant and animal species only to repeat a variant of its emerging mantra: "While the extent and rate to which individual plant species will be impacted is difficult to foresee with any level of confidence (in their entirety: Walther et al. 2002; Fagre et al. 2003), there is general consensus that grizzly bears are flexible enough in their dietary needs that they will not be impacted directly by ecological constraints such as shifts in food distributions and abundance (Servheen and Cross 2010, p. 4; IGBST 2013, p. 35).
>
> Finally, on page 13217 the Service again marches through yet another pro forma discussion of prospective changes in fire regimes to reach its master conclusion regarding prospective effects of climate change on Yellowstone's grizzly bears: "Because grizzly bears have shown resiliency to changes in vegetation resulting from fires, we do not anticipate altered fire regimes predicted under most climate change scenarios will have significant negative impacts on grizzly bear survival or reproduction, despite its potential effects on vegetation. Therefore, we conclude that the effects of climate change do not constitute a threat to the GYE grizzly bear DPS now, nor are they anticipated to in the future."

The short-comings of the Service's treatment of climate change are striking—and consistent with a track record of disregard recently highlighted by the US Montana District Court's ruling on the Service's failure to list wolverines under the ESA.

13.1. The Service fails to adequately represent the best available science of direct relevance to projecting environmental change in the Yellowstone ecosystem.

The Service's at times lengthy recitation of literature that forecasts generic effects of climate change is clearly *pro forma*, as evidenced by the fact that the Service doesn't even pretend to explicitly link such effects to the particulars of Yellowstone's bears and bear habitat.

Emblematic of its failure to address Yellowstone's specific conditions, the Service (barring once) almost totally ignores the large body of research that directly addresses climate change in Yellowstone, including projections for climate, vegetation, and wildfire. In the one instance where the Service does cite regionally-specific research (i.e., Romme & Turner 1991), the results are misrepresented in an apparent attempt to dismiss the threat posed by climate change to army cutworm moths (for more on this, see my comments specific to moths). But perhaps most problematic, the Service fails to address what we do know about grizzly bear behavior, diet, and habitat in the Yellowstone ecosystem and instead substitutes platitudes such as "most grizzly bear biologists" and "general consensus."

Briefly, climate has warmed and will continue to warm substantially in the Yellowstone ecosystem, with a net projected increase in growing season drought, albeit mitigated by some projected increase in precipitation. Without being exhaustive, Pedersen et al. (2010), Rice et al. (2012), Chang & Hansen (2015), and Tercek et al. (2015) provide what is probably the most relevant and up-to-date information on past and prospective future changes in climate of the Yellowstone ecosystem. Temperatures have already increased substantially, especially since the late 1940s, and will continue to increase at a rapid rate under all plausible forecast scenarios (Figure 13.1.1). Total precipitation has been stable during the last 60 years or so, but is projected to increase modestly with climate change, with offsets by warming resulting in a net projected increase in droughty conditions, especially late in the growing season. For various reasons, snowpack has declined significantly since 1980, but in large measure due to warmer winter and spring conditions—consistent with broad-scale west-wide trends (Clow 2010, Pederson et al. 2013). The resulting hydrologic changes have been detrimental to cold-water fish such as cutthroat trout (Williams et al. 2009; Isaak et al. 2010, 2012, 2015; Wenger et al. 2011). The magnitude of these historical and forecasted changes is non-trivial and potentially even catastrophic—and at variance with implications made by the Service in the Rule that uncertainty is perhaps a defining feature. It is not. The forecasts are thematically unambiguous, convergent, and consistent with recent historical trends.

Numerous researchers have projected climate-driven changes in the distributions of either vegetation formations or individual plants species at a resolution meaningful to regional assessments for the Yellowstone ecosystem (e.g., Romme & Turner 1991; Bartlien et al. 1997; Shafer et al. 2001; Schrag et al. 2007; Crookston et al. 2010; Coops & Waring 2011; Diaz & Eischeid 2007; Westerling et al. 2011; Bell et al. 2013, 2014; Gray & Hamann 2013; Chang et al. 2014; Hansen & Phillips 2015; Rehfeldt et al. 2006, 2012). Again, unlike some of the Service's innuendo in the Rule, the projections are unambiguous and consilient. We can expect massive changes in the distributions and abundance of plant species, including the prospective loss of most (90% plus) alpine and high subalpine environments. Among the species expected to experience major declines are subalpine fir, Engelmann spruce, and whitebark pine, with invasion of higher elevations by Douglas-fir and lodgepole pine—continuing trends that have been underway for the last 13,000 years (Iglesias et al. 2015).

Fire regimes are expected to change substantially. Westerling et al. (2011), Luo et al. (2013), Stavros et al. (2014), Barbero et al. (2015), and others project an increase in fire frequency and/or extent perhaps sufficient to essentially eliminate forest conditions from most of the Yellowstone ecosystem—albeit with a potentially progressive lessening of fire intensity (Parks et al. 2016). These future fire regimes will likely amplify the rote effects of climate change in driving an increase in the extent of non-forest conditions (Rice et al. 2012, Chang & Hansen 2015), but with an additional likely CO2-driven trend towards increasing concentration of biomass on shrubs such as *Artemisia* sp. (e.g., Harte & Shaw 1995, Polley et al. 1997, Morgan et al. 2007).

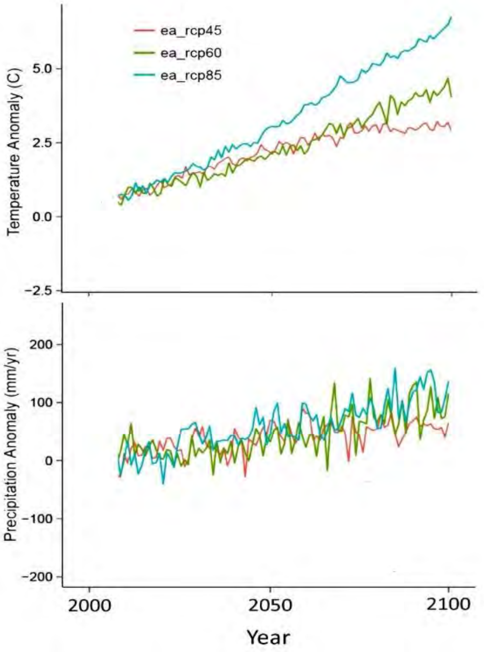


Figure 13.1.1. Projected increases in temperature and precipitation specific to the Yellowstone ecosystem (Chang & Hansen 2014).

This very brief review emphasizes the extent to which the Rule neglects high-relevant regionally-specific research, provides a backdrop of prospective change, and provides an opportunity to actually examine (in brief) what climate change might bring—and has already brought—for Yellowstone's grizzly bears; and with reference to the particulars of grizzly bear research in this ecosystem instead of resort to platitudes and figurative arm-waving.

13.2. The Service fails to adequately account for the prospective terminal loss of whitebark pine as a functional part of Yellowstone's grizzly bear habitat.

There is little doubt that climate warming has been a major driver of devastating—even catastrophic—losses of mature whitebark pine to bark beetles, especially since the early 2000s (Logan et al. 2010, Macfarlane et al. 2013). In fact, these losses are the very premise of the IGBST research that the Service cites so extensively (e.g., Bjornlie et al. 2014, Van Manen et al. 2015; but see my other comments regarding the fatal flaws of this work). Looking to the future, virtually every study that has been done projects terminal losses of virtually all whitebark pine in the Yellowstone ecoregion from a combination of climate warming, increased fires, insidious spread of white pine blister rust, continued outbreaks of bark beetles, and competition from lower-elevation species such as lodgepole pine and Douglas-fir (e.g., Romme & Turner 1991, Bartlein et al. 1997, Warwell et al. 2006, Schrag et al. 2007, Coops & Waring 2011, Coops et al. 2011, Hatala et al. 2011, Jewett et al. 2011, Simard et al. 2012, Gray & Hamann 2013, Weed et al. 2013, Chang et al. 2014, Miller et al. 2015). The preponderance of evidence unambiguously supports the conclusion that whitebark pine will continue to be lost and will not return as a functional part of Yellowstone's grizzly bear habitat. This conclusion holds, the apologia of Mahalovich (2013) not-withstanding, especially given that this author would have us believe that something never before achieved is possible: i.e., the artificial selection (or engineering) of a new species that is simultaneously highly resistant to blister rust and beetles, a better competitor under warmer drier conditions, with continued production of ample nutritious seeds, and, as a practical matter, able to be propagated in sufficient numbers to make any functional difference. Unlikely at best.

And recent, on-going, and foreseeable losses of whitebark pine have had and will almost certainly continue to have major negative impacts on Yellowstone's grizzly bears. As I point out elsewhere, bear mortality, in general, and in relation to use of human-associated meat, has sky-rocketed coincident with terminal declines in whitebark pine seed availability beginning 2006-2007. Moreover, whitebark pine seeds were unambiguously an important source of dietary fat—a critical diet nutrient—and, historically, of roughly twice the importance to females as to males (Mattson 2000, Felicetti et al. 2003).

13.3. The Service fails to adequately account for the prospective terminal loss of army cutworm moths as a functional part of Yellowstone's grizzly bear habitat.

On page 13213 of the Rule, the Service invokes a single publication to claim that, because "vegetation types in Yellowstone have a wide elevational range," alpine tundra and other alpine communities will not be adversely affected by climate warming. They then go on to claim, based on an additional single citation, that army cutworm moths will not be adversely by climate warming, not only because alpine environments will somehow survive intact, but also because moths will somehow otherwise adapt. These claims are not substantiated nor are they logical.

For one, alpine environments are, in fact, defined by a very specific climatic envelope, as is the corresponding domain of the plants that comprise alpine tundra (Lütz 2011, Körner 2013). Moreover, every single scientific projection for alpine environments under different plausible scenarios of climate change predicts (or projects) major if not catastrophic losses (e.g., Grace et al. 2002, Pauli et al. 2003, Walther et al. 2005, Grabherr et al. 2010, Dullinger et al. 2012, Elmendorf et al. 2012, Gottfried et al. 2012). Projections specific to the alpine zone in the western United States are no exception (Romme & Turner 1991, Diaz & Eischeid 2007). These projections are consistent with the climatic and physiological factors that sustain alpine plants in the face of competition from other species better adapted to less harsh lower-elevation environments (for example, see a rich scientific literature dating back to Tranquillini [1964] and Billings & Mooney [1968]).

On top of this, every instance where bears have been documented to feed on aggregations of army cutworm moths in the northern Rocky Mountains ha been restricted to alpine fellfields near alpine tundra: the Mission Mountains, Scapegoat Mountains, Glacier

National Park, and the Absaroka Mountains (Chapman et al 1955, Craighead et al. 1982, Mattson et al. 1991b, French et al. 1994, O'Brien & Lindzey 1998, White et al. 1998a). As a corollary, every study of summer cutworm moth ecology has demonstrated that the moths subsist on the nectar of tundra flowers (French et al. 1994, O'Brien & Lindzey 1994, Kevan & Kendall 1997, White et al. 1998b). It is somewhat mysterious, then, as to how and why grizzly bears would shift to feeding on moths in an environment where feeding on moths has never before been documented and where moths have never before been documented to thrive. The Service not only fails to reference any of the highly relevant literature listed here, but also convolutes and contradicts all available evidence.

Whereas it is true that no one can say for sure how over-summering army cutworm will respond to loss of alpine environments, this does not in any way substantiate the assertions made by the Service that alpine environments will somehow be unaffected by climate warming, that moths will somehow adapt, and that grizzly bear foraging will be minimally affected. Such claims, in fact, defy not only the weight of evidence, but also everything we do know about grizzly bears, cutworm moths, and prospective effects of climate change on the habitats that over-summering moths currently depend on.

13.4. The Service fails to adequately account for the prospective terminal loss of cutthroat trout as a functional part of Yellowstone's grizzly bear habitat.

Yellowstone cutthroat trout are a cold-water-adapted species that is, together with bull trout, amongst the most vulnerable of any fish to prospective warming of aquatic habitats (Bear et al. 2007, Kaeding 2012). Although cutthroat trout will grow more quickly in warmer waters (Bear et al. 2007, Kaeding 2012, Al-Chokhachy et al. 2013), this advantage is offset by elevated competition, incidence of disease, spread of non-native species, and worsening flow regimes that typically accompany warming—which is also often coincident with drying (Koel et al. 2006, Gresswell 2009). In Yellowstone Lake, the primary threats from non-natives species are in the form of whirling disease (at a 20% plus infection rate among native cutthroat trout), New Zealand mud snail (as near as the Madison River), and Lake trout—a highly piscivorous predator on cutthroat trout first detected in Yellowstone Lake during 1994.

There is no doubt that Yellowstone Lake cutthroat trout populations have declined catastrophically since the late 1970s, to the point where this species has been functionally extirpated as a source of food for Yellowstone's grizzly bears (see my comments under above). There is likewise no doubt that predation by Lake trout played a major role in this decline (Ruzycki et al. 2003, Koel et al. 2005). However the role of worsening hydrologic conditions is under-appreciated. Kaeding (2010) presents convincing evidence that hydrologic conditions (indexed by total annual air degree-days) drove declines in cutthroat trout populations perhaps even more than Lake trout predation, explaining the fact that the beginning of substantial declines in the cutthroat trout population predated detection of Lake trout by roughly 15 years.

Looking to the future, there is also little doubt that climate change will bring worsening rather than improving conditions for Yellowstone Lake cutthroat trout, including elevated water temperatures, early peak flows in spawning streams, worsening late-summer low flows, increasing blockage of spawning stream outlet channels, and less thermal buffering because of more prevalent wildfires (e.g., Williams et al. 2009; Isaak et al. 2010, 2012, 2015; Wenger et al. 2011; Tercek et al. 2015). The basis for this prognosis is consistent and strong. Insofar as threats from non-native species are concerned, even though there is evidence for the efficacies of recent efforts to control Lake trout in Yellowstone Lake (Syslo et al. 2011, Koel et al. 2015), such efforts will likely need to be sustained indefinitely, with virtually no prospect of ever completely eliminating Lake trout (Koel et al. 2006, 2015; Gresswell 2009). Moreover, there is no plan in place with prospects of ever controlling whirling disease or preventing the eventual introduction of New Zealand mud snails. In other words, the long-term prospects for Yellowstone Lake cutthroat trout are bleak, albeit with the possibility of modest positive trends in the next several decades. More certainly, cutthroat trout will not return as a major grizzly bear food in the most protected core of the ecosystem.

13.5. The Service fails to adequately account for the prospective effects of climate change on populations of elk and bison in the Yellowstone ecosystem.

Belying the cursory treatment of elk and bison by the Service on page 13212 of the Rule, an assessment of how climate change might affect these ungulates, along with consequent effects on grizzly bears, is perhaps the most complicated of any for foods of recent importance to bears. Certainly, the Service's hasty conclusion that "fluctuations in the availability of ungulates are not a threat to the GYE grizzly bear population now, or in future" is irresponsible and simple-minded.

Elk populations in the northern Rocky Mountains, including the Yellowstone ecosystem, are limited and regulated by a combination of factors, most notably sport hunting, winter severity, summer forage, and predation. The Service would claim that sport hunting is discretionary and therefore entirely under the control of managers. Without questioning any assertions about control over sport harvest, climate *is not* controllable by wildlife managers.

Not surprisingly, a number of publications have implicated winter severity as a limiter of elk populations, primarily through effects on survival of short-yearling and senescent animals (Garrott et al. 2003, Lubow & Smith 2004, Vuvetich et al. 2005, Brodie et al. 2013, Proffitt et al 2014)—something that the Service acknowledges in the Rule. But an even larger body of research implicates summer forage conditions, primarily through effects on female pregnancy and early calf survival (Merrill & Boyce 1991, Coughenour & Singer 1996, Cook et al. 2004, Lubow & Smith 2004, Vucetich et al. 2995m Parker et al. 2009, Griffin et al. 2011, Middleton et al. 2013, Proffitt et al. 2014) —something the Service does *not* acknowledge. And calf survival is increasingly recognized to be a driver of elk population growth (Raithel et al. 2007).

Compared to elk, much less is known about the limitation or regulation of bison populations, especially in Yellowstone's unique environment. Perhaps the best synopses of this information can be found in the Elsevier Press book "The Ecology of Large Mammals in Central Yellowstone," specifically in Bruggeman et al. (2009), Fuller et al. (2009), Geremia et al. (2009). All of these researchers found that winter severity reduced bison survival rates, but in a way that entailed complex interactions with anthropogenic factors and bison population densities.

In short, winter severity (e.g., Snow-Water-Content [SWE]) and summer weather (e.g., late-season forage conditions) have strong effects on elk populations, whereas winter severity seems to be the dominant climate/weather effect on bison. With those themes in mind, it is relevant to look at projections for SWE on Yellowstone's elk and bison winter ranges, drought (i.e., summer precipitation and temperature) effects on summer ranges, and the likely extent and nature of non-forest conditions—which, on the face of it, would be tied to the extent of favorable forage conditions for both ungulates.

As it turns out, the amount and extent of severe winter conditions, at least as indexed by SWE, are not projected to change much, if at all, on Yellowstone's winter ranges for the next 75 years or so (Marcus et al. 2012: 126-127)—this because most nearer-term effects of winter warming will be experienced at lower elevations below the rising elevational threshold of the rain-snow transition (Klos et al. 2014). Thus, at least for the foreseeable future, there is not likely to be much mitigation of limiting effects attributable to winter severity—and, thus, no related positive population responses.

The forecast for summer drought and related decreases in forage quality is less certain for the Yellowstone ecoregion (Rice et al. 2012, Chang & Hansen 2015). But, to the extent that drought effects manifest later in the growing season, and are driven by an offset of less certain precipitation forecasts by more certain temperature ones (Marcus et al. 2012: 126-127), odds are better that incidence and severity of drought will increase rather than decrease. In fact, such a prognosis is consistent with West-wide spatially-explicit forecasts of drought (Guzler & Robbins 2011, Gai 2012), especially the incidence of severe episodes (Strzepek et al. 2010).

This weight-of-evidence prognosis needs to be considered in the balance with likely increases in non-forest conditions. As I note earlier, this increase probably will not be of a simple nature, with the prospect of more biomass being concentrated on woody shrubs compared to forbs and grasses, to the detriment of both elk and bison.

All of this constitutes a basis for reaching some provisional conclusions regarding climate-driven prospects for elk and bison in the Yellowstone ecoregion. Carrying capacity for bison will probably not increase in the foreseeable future simply because winter severity will not likely diminish. By contrast, carrying capacity for elk will likely increase, primarily as a function of an increase in non-forest conditions, but to an extent strongly conditioned on the prevalence of growing-season drought. As a bottom line, there is no simple prognosis for elk and bison, especially given that populations of both are strongly affected by anthropogenic forces such as sport hunting (elk) and, increasingly, incidence and management of disease (elk and bison). In other words, there is no basis here for the Service's blithe conclusion.

13.6. The Service fails to adequately account for nature, quality, and potential effects of alternate foods that may be used more heavily by Yellowstone's grizzly bears in response to losses of currently-important foods.

All of this begs the question whether Yellowstone's grizzly bears will find alternative foods to eat that are of sufficient quality and quantity to offset past and prospective future losses of foods that were (and are) known to be important sources of energy and nutrients: whitebark pine seeds, cutthroat trout, army cutworm moths, elk, and bison. Perhaps as important, will bears likely end up eating these alternative foods under circumstances where risk of death is elevated? Answering these questions requires more than what the Service currently offers in the Rule, which amounts to assertions and platitudes based on invocations of "omnivory," "resilience," and the ill-framed opinions of bear researchers who probably know little about climate change and even less about the Yellowstone ecosystem (for more on this see my comments related to nutritional ecology, point 7).

So, what evidence is there to draw on? Perhaps the most comprehensive evidenced-based forecast of climate-driven dietary changes for Yellowstone's grizzly bears was done by Mattson (2000). Because this is the sole example of such an effort, I quote extensively from it as follows, noting that some citations are dated simply because of this dissertation's 2000 publication date:

> "There is evidence from this study that grizzly bear foraging is temperature and moisture sensitive. The abundance and related use of ants by grizzly bears clearly seem to be dependent on ambient warmth [see also Mattson 2001] as, to a lesser extent, do growth and use of dandelions and thistles. It is likely that use of these foods by bears will increase with climate warming, assuming that moisture relations remain unchanged. However, use of many vegetal foods was sensitive to amounts of precipitation. The likelihood that bears would excavate a food declined during dry months, as did the likelihood that they would graze many of the forbs and grasses. All else equal, it is likely that grazing and root grubbing would be less common if climate warming caused drier soils during the growing season. In addition, the sensitivities of grubbing for biscuitroots and rodents to total winter precipitation suggest that use of biscuitroots would decline and use of rodents would increase if winters became drier.
>
> Currently, it is strictly a matter of speculation whether total amounts of vegetal foods would change in bear range, aside from whether these foods would be used by bears or not as a function of proximal conditions. Compared to use of biscuitroots, use of yampa is conceivably more sensitive to widespread drying because yampa is restricted to moist sites (Mueggler & Stewart, 1980; Mattson, 1984) and is typically more difficult to extract (Mattson et. al. 1999 [2004]). It also is likely that consumption of clover by Yellowstone's grizzly bears will be more sensitive to changes in ambient conditions than many other bear activities because use of this food is so strongly associated with dense swards on moist soils. Otherwise, use of mushrooms and *Sheperdia* may increase because bear use of these foods is strongly linked to low-elevation lodgepole pine-dominated types (LPICO and LPIEN; Table 3; [see also Mattson 1997]). These types will likely become more extensive under warmer conditions (Romme & Turner, 1991).

> Fire had a number of effects on the behavior of Yellowstone's grizzly bears that would likely ramify if climate warming induced more frequent and extensive burns. Grizzly bears were more likely to graze dandelions and early-season graminoids after the 1988 wildfires compared to before. On the other hand, the likelihood that they would excavate osmorhiza roots or whitebark pine seeds declined substantially along with the intensity of excavations for pine seeds. The former activities were more likely to occur in recent burns, while the latter were not (Blanchard & Knight, 1990; Mattson, 1997a; Table 3). All of these consequences are logically related to fire-caused increases or declines in these foods (Blanchard & Knight, 1990; Singer & Harter, 1996; Mattson, 1997a). The post-1988 decline in bear use of rodents and rodent food caches during Spring and Estrus could have been a consequence either of fire-caused pocket gopher mortality or bears choosing to graze instead. Unfortunately, there is little research on the consequences of fires to pocket gophers that might provide insight into which was a greater effect. Even so, the increase in post-fire use of rodents during Hyperphagia suggests that pocket gopher mortality was not a factor and that the explanation lies in trade-offs with opportunities to graze graminoids, year-round."

As a bottom line, though, any invocation of ants, hornets, foliage, rodents, or roots as substitutes for foods that have been lost—and will prospectively continue to be lost—has no *prima facie* merit simply on the basis of nutritional and energetic considerations (e.g., Mattson et al. 2004). I cover this in more detail under my comments related to nutritional ecology. In other words, contrary to assertions (or innuendo) by both the Service and Gunther et al. (2014), dandelions or roots of various types quite simply will not provide compensation, especially at a population level. Likewise, remarks such as those by Fortin et al. (2012) suggesting that fungi have, and will, compensate for on-going losses of foods such as cutthroat trout and whitebark pine seeds has little merit. Fungi offer little fat (a critical nutrient for bears), and consumption of false truffles (and other mushrooms) has largely been confined to a limited portion of the ecosystem concentrated in lodgepole pine forests on the rhyolite plateaus of Yellowstone National Park (Mattson 1997, 2000; Mattson et al. 2002, 2004; Fortin et al. 2013). And, when fungi in the feces collected by Fortin et al. (2012) are corrected for the differential passage and digestibilities of various foods, the overall dietary contribution of false truffles and other fungi is trivial (Lopez-Alfaro et al. 2015).

There is a chance that high-quality foods from warmer climes could migrate to the Yellowstone ecosystem with time. The most obvious candidate is Gambel's oak—a source of acorns and a high-quality bear food in areas where it does occur, the nearest of which is roughly 130 miles south of Yellowstone's occupied grizzly bear habitat. In fact, several projections suggest that suitable climatic environments will emerge for Gambel's oak in the Yellowstone ecosystem over the next 100 years (e.g., Rehfeldt et al. 2006). That still begs the question of how long it would take oaks to colonize newly opened environments over a 100 miles distant, especially given that migration rates have emerged as a major prospective consideration amongst those projecting changes in plant distributions (e.g., Pearson 2006). Perhaps more to the point of these deliberations: we can be more certain of what will happen with *in situ* foods compared to speculative foods relocating from a considerable distance.

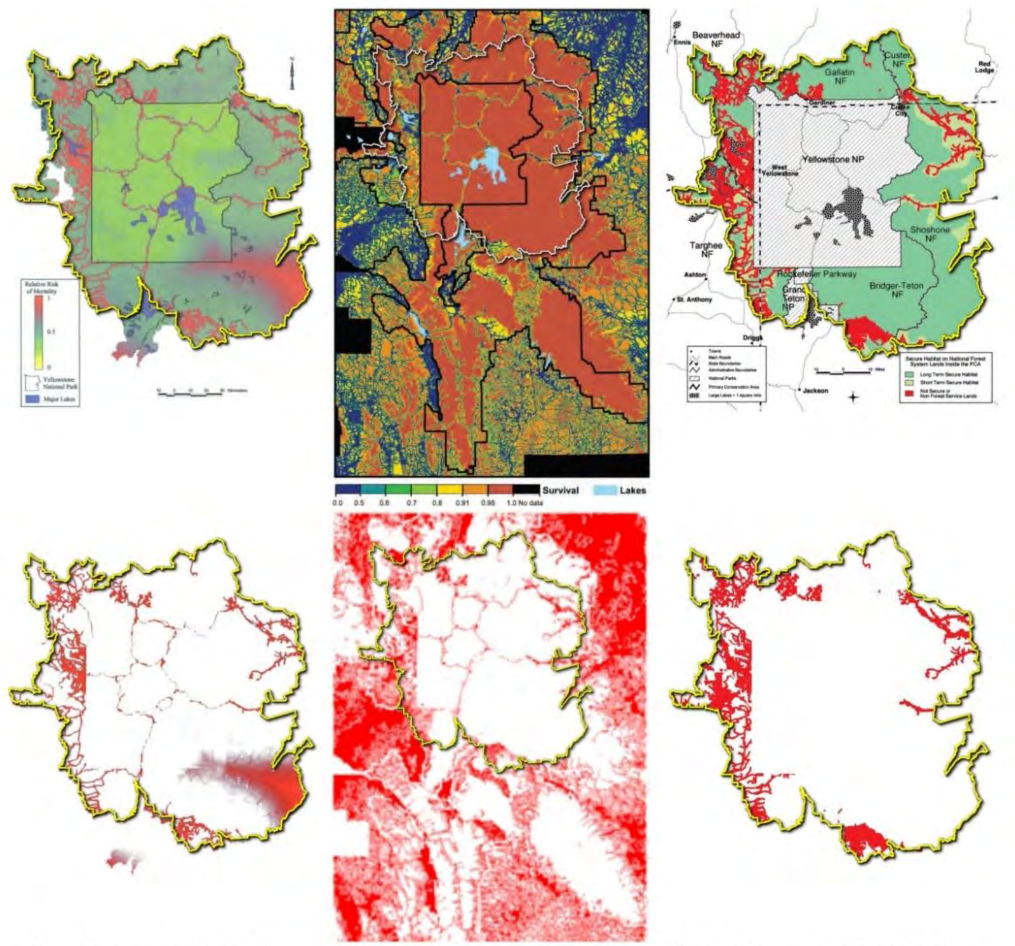
13.7. The bottom line when it comes to the prospective effects of climate change:

The Services does a grotesquely inadequate job of assessing the prospective effects of climate change on Yellowstone's grizzly bears and, to the extent that it does attend to this task, the Service's intent seems to be out-of-hand dismissal of such effects in service of rushing to a preordained/pre-decisional conclusion. When given due regard, the net assessment is one of continued major losses of known high-quality foods, dietary shifts among surviving bears to greater reliance on lower-quality foods, and a substantial decline in unit area carrying capacity of the Yellowstone ecosystem. Moreover, grizzly bears will likely be spending more time foraging in high-risk environments, especially to the extent that they eat more foods at lower elevations nearer people—or foods such as livestock that bring them into greater conflict with people. It is certainly the case that past, on-going, and prospective future losses of whitebark pine and moth foraging sites will deprive grizzly bears of foods in some of the most remote and secure parts of the Yellowstone ecosystem. By contrast, essentially all prospective replacement foods (including Gambel's oak, should it ever arrive) tend to occur in more hazardous lower-elevation habitats. The preponderance of evidence clearly supports a different conclusion from that reached by the Service. Climate change has had and will continue to have devastating impacts on Yellowstone's grizzly bears.

---

14. The Service's assertion on page 13197 of the Rule that "there are no data to indicate habitat fragmentation within this population is occurring [sic]" is wrong because it contradicts the totality of best available science. There is, in fact, ample evidence of habitat fragmentation within the current distribution of Yellowstone's grizzly bears, which renders the Service's assertion both wrong and arbitrary.

On Page 13197 of the Rule the Service asserts that "The GYE grizzly bear population is currently a contiguous population across its range, and there are no data to indicate habitat fragmentation within this population is occurring [sic]." Realizing that fragmentation and contiguity often come in degrees rather than as absolutes, this statement by the Service flatly contradicts the best available science as well as its own representations of "secure" habitat. In other words, this assertion is unsubstantiated and wrong.



A. Johnson et al. (2004) **B.** Schwartz et al. (2010) **C.** U.S. Forest Service (2006)

Figure 14.1. This figure presents three maps extracted from the most recent assessments of habitat fragmentation and impairment in the Yellowstone ecosystem (top), with the areas of fragmented and impaired habitat extracted and uniformly shown in red in the bottom array (e.g., Schwartz et al. [2010] invert their color scheme and show the most impaired habitat in blue and the least impaired in red). The PCA boundary is shown in yellow in the three bottom maps.

Figure 14.1, immediately above, shows the mapped results of three out of the total of six studies that have explicitly looked at contiguity and fragmentation of grizzly bear habitat in the Yellowstone ecosystem. The six include Merrill et al. (1999), Carroll et al. (2001), Merrill & Mattson (2003), Johnson et al. (2004), U.S. Forest Service (2006), and Schwartz et al. (2010). The results above are from the three latest studies, with areas of fragmentation or contiguous impaired habitat extracted and shown in red in the three images arrayed left to right at the bottom. Parenthetically, the map produced by Schwartz et al. (2010) purports to represent odds of survival for grizzly bears, with the red denoting all of the areas were modeled survival rates are well below what is considered to be sustainable. The map produced by the U.S. Forest Service is based explicitly on delineations of "secure" habitat as defined by the Service. The PCA is delineated in yellow in the three bottom images.

The results of all of these studies constitute a remarkable consensus, including the three not explicitly represented in figure 14.1. All show high degrees of fragmentation and substantial areas of impaired or otherwise deficient habitat. Within the PCA, these areas are concentrated in the west on the Targhee and Gallatin National Forests (NFs), in the far south on the Bridger-Teton NF, and in the northeast on the Shoshone and Gallatin NFs. Immediately adjacent to the PCA, there is a substantial zone of fragmented habitat separating the PCA from a sizable chunk of suitable habitat in the Wind River Range. Not coincidently, a large fraction of conflicts between livestock and grizzly bears are currently concentrated in this fracture.

Even allowing for different and shaded definitions of fragmentation, the Service is egregiously deficient in its representation of this substantial body of convergent scientific results. The Service needs to acknowledge and adequately represent this literature, provide a functional and justified definition of "fragmentation," and then reconcile that definition with the scientific results referenced here.

---

15. The Service's assertion that "we do not expect such development [of mining claims] inside the PCA will constitute a threat to the GYE grizzly bear DPS now, or in the future" is unsubstantiated, counter to the primacy of the 1872 General Mining Law, and thus arbitrary.

The Service asserts on page 13196 of the Rule that the CS ensures that habitat security will not be compromised or "threatened" by the development of mining claims. The rule goes on to state that there are 28 mining claims with operating plans in the PCA. The Rule then asserts that federal land and minerals managers (primarily the U.S. Forest Service) will somehow allow for operation of only one new mine at a time, forestalling the operations of any others—and thus "ensuring" no net loss of habitat security. At the end of the section devoted to Mineral and Energy Development, the Service concludes "we do not expect such development [of mining claims] inside the PCA will constitute a threat to the GYE grizzly bear DPS now, or in the future."

These claims and assertions are unsubstantiated and belie the history of mining claim development, including the impacts on grizzly bears that were anticipated and documented by regulatory agencies during previous evaluations of proposed mining operations. For one, the CS will not trump provisions of the 1872 General Mining Law. As the U.S. Forest Service states in its 2006 EIS covering Forest Plan revisions for management of Yellowstone's grizzly bear habitat: "Projects would be permitted according to the requirements of the 1872 Mining Law" (p. 208) and "Processing of mineral operations under the 1872 General Mining Law is not discretionary" (p. 271). Barring major political intervention, any proposed mining operations will proceed. And there are currently two mining operations in the process of development in and near the PCA (the Crevice Mine and the Emigrant Mine). The New World Mine dating from the early 1990s was deemed to have major potential impacts on grizzly bears (e.g., Mattson 1995) and did not pass regulatory review only because of

Presidential intervention and a special Congressional allocation to purchase the entailed mineral claims.

In short, a long history of legal precedent, together with realities on the ground, cannot be over-turned or otherwise magically transformed simply by the Service's unsubstantiated assertion. The Service needs to recognize the primacy of the 1982 Mining Law, the implications of this primacy, the fact that mining claims are being and will continue being developed, and that these realities will likely constitute some degree of threat to the Yellowstone grizzly bear population.

16. The definition of "secure" habitat adopted by the Service is unsubstantiated by any referenced scientific research, roughly 72-times smaller than the compilation of best available science would recommend, and thus arbitrary. This gross underestimation of the dimensions of a secure area leads to inflated estimates of total secure habitat in Yellowstone's Bear Management Units. Moreover, the Service argues against its own definition of secure habitat in excluding certain areas from being deemed "suitable."

The Service defines "secure habitat" on page 13194 of the Rule as "those areas with no motorized access that are at least 4 ha (10 ac) in size and more than 500 m (1,650 ft) from a motorized access route or recurring helicopter flight line…" The cited authority for this definition is the U.S. Forest Service's 2006 EIS for revision of Forest Plans in the Greater Yellowstone Ecosystem. Yet this document contains no justification for the adopted definition of secure habitat other than a reiteration of the dimensions given in the Rule. As a result, the Service does not provide nor reference any justification grounded in the best available science for this key facet of current and prospective habitat management under terms of the CS. Moreover, the asserted definition of secure habitat is ill-conceived and substantially at odds with the best available science, and thus amounts to an arbitrary determination.

By contrast, a compilation of the best available science by Mattson (1993), referenced to a thoroughly articulated justification, recommends that "microscale" security areas contain a core roughly 290 ha (716 ac) in size, roughly 2-4 km from the nearest road or other human facility. The resulting area, including core and buffer, would be 28.3 km^2 (c. 7000 ac) in size. The recommended core would be roughly 72-times larger than the 4 ha used by the Service and roughly 4-8 times farther from the nearest significant human facility. The secure core recommended by Mattson (1993) corresponds to the size of 24-48 hr foraging areas documented for Yellowstone's grizzly bears, whereas the buffering distance from human facilities attends to not only to the extent of characteristic habitat alienation, but also the characteristic zone within which human-caused mortality has been documented to concentrate. There has been essentially no science since 1993 that would support a change in the recommendations made by Mattson (1993).

Perhaps as important, the standards developed by Mattson (1993) have been codified through litigation and through a US Fish & Wildlife Service Biological Opinion on the 1997 Revised Forest Plan for the Targhee National Forest. The Service fails to address this discrepancy between its past and present deliberations.

Interestingly, the Service tacitly refutes its own definition of secure habitat in its argument on page 13185 of the Rule for excluding fragments of habitat adjoining sheep allotments in the Wind River Range from "suitable" habitat. The Service's argument rests on an ill-defined invocation of "edge effects"; i.e., that edges of an unspecified dimension are somehow unsecure enough to warrant exclusion from "suitable" habitat. Without being privy to the exact dimensions of these "edges," they are almost certainly larger, each, than the 4 ha (plus 500 m buffer) threshold that the Service argues elsewhere is sufficient to ensure "security." The Service thus presents us with a prospective logical contradiction that needs to be reconciled.

In short, given the importance of habitat security to grizzly bear conservation, the Service needs to provide a readily-accessible and coherent justification for its definition that is grounded in the best available science rather than bald assertion. If such a justification is not forthcoming, the Service needs to revise its standard for secure habitat to reflect the best available science and then uniformly apply that definition in its delineations and deliberations.

---

17. The Service disregards and misrepresents the best available scientific information in its dismissal of natural predation as a threat to Yellowstone's grizzly bears on page 13205 of the Rule.

The Service claims that natural predation of grizzly bears is rare and then recites the number of bears known to have been killed by other bears (28 between 1986-2012, roughly 1 per year) and by wolves (a total of 8 during an unspecified period of time) in the Yellowstone ecosystem as a basis for then concluding "…this source of mortality does not constitute a threat to the GYE grizzly bear DPS now, or in the future." These statements constitute a selective and distorted representation of the best available science which would, in fact, support a different conclusion.

Most of the grizzly bears that die because of natural predation are cubs and yearlings (for substantiation, see the Service's own referenced literature). Most of these deaths are not documented because of the very nature of natural predation, which is very opaque to researchers. In the large majority of cases, young bears are noted to have disappeared between one sighting of the mother and the next, without any clue as to the cause. Only rarely do investigators get on the ground in a timely enough way to "document" the natural cause of death, which is almost invariably predation—rarely senescence or a natural accident. Nonetheless, a large number of cubs and yearlings disappear, almost all likely because of predation.

When all potential natural deaths are considered—again, most likely attributable to predation—a different picture emerges compared to the one painted by the Service. Drawing on IGBST data, the median number of bears that likely or almost certainly died from predation is actually nearer 3 per year rather than 1 per year for the period 1986-2015. Perhaps of more relevance to the Rule, these numbers have increased substantially over time, as shown by figure 17.1. Whereas the per annum median was 1 between 1986 and 1996, the median since 2010 has been 6. Throughout the period 1986-2015, 66% were cubs and yearlings. The dramatic and non-trivial jump in deaths potentially attributable to predation corresponds with loss of foods such as whitebark pine and cutthroat trout (see my point under 11.1) and a probable shift by many of the affected bears to eating more meat from ungulates—which is likely to be particularly hazardous for the offspring of involved adult females (see my comments elsewhere, including the inadequate conceptual frame used by the Service to assess hazards associated with bear consumption of different foods [X]).

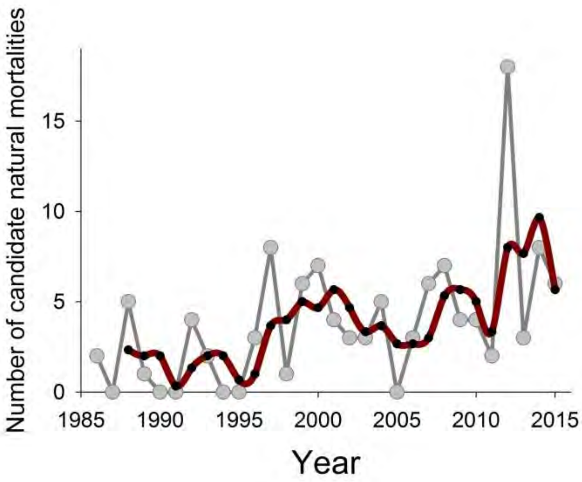


Figure 17.1. Number of known and probable mortalities likely to have been caused by natural predation, 1986-2015. Annual numbers are shown by the gray dots, a running three-average by the thicker red line. Data were provided by the IGBST and the Service.

In fact, this jump in known and probable deaths, likely attributable to predation, coincides almost exactly with the dramatic decline in cub and yearling survival rates documented by Van Manen et al. (2015), which they also attribute to bear predation (but under the rubric of "density-dependence," a causal claim that is unsubstantiated for reasons that I articulate elsewhere). Regardless of the cause, Van Manen et al. (2015)

attribute the stalling of population growth beginning in the early 2000s in large part to this jump in deaths of young bears. In other words, this increase in deaths has had non-trivial effects on population trajectory.

As a bottom line, weight of evidence supports concluding that natural predation on especially cubs and yearlings has increased at the same time that survival rates of these younger bears has substantially declined, all with demonstrable effects on population growth rate. As such, weight of evidence would support concluding that natural predation does, in fact, pose a "threat" to the population, especially given that weight of evidence further supports concluding that increases in natural predation are due primarily to a deteriorating environment—shifts in diet driven by losses of key foods such as whitebark pine seeds.

18. The claim by the Service on page 13207 of the Rule that "Because human-caused mortality has been reduced…this source of mortality does not constitute a threat to the GYE grizzly bear DPS now, or in the future" is a completely arbitrary unempirical assertion.

The Service deploys a number of bald assertions together with varying degrees of tortured logic on page 13206 and 13207 of the Rule in an effort to dismiss the threat posed by human-caused mortality to Yellowstone's grizzly bear population as a basis for then concluding that ""Because human-caused mortality has been reduced…this source of mortality does not constitute a threat to the GYE grizzly bear DPS now, or in the future." This conclusion and its supporting arguments are not only unsubstantiated by any empirical evidence, but also contradict the best available scientific information.

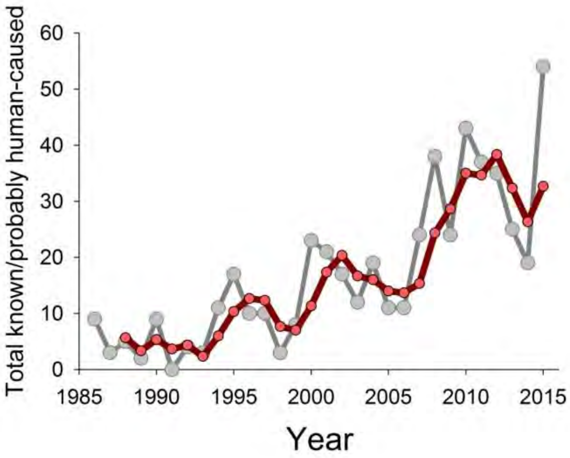


Figure 18.1. Total known and probable human-caused mortalities, by year, for the proposed Yellowstone grizzly bear DPS. The gray line and dots shown annual counts and the red line a 3-year running average. All data were obtained from the IGBST and the Service.

My claim is illustrated by a simple graph and derivative calculation using publicly-available scientific information obtained from the IGBST and the Service. Figure 18.1. shows the number of human-caused grizzly bear deaths in the Yellowstone ecosystem (i.e., the PCA, DMA, and proposed DPS) for each year, 1986-2015. The gray dots represent tallies for each year and the red line a 3-year running average of the same to emphasize trend. The result is unambiguous and in stark contrast to claims made by the Service in the Rule.

Total human-caused mortality has risen steadily since roughly 1994, and risen dramatically since 2007. More succinctly, human-caused mortality rose by 8.9% per year 1986-2015 and, even more remarkably, by 7.0% per year 2002-2015. This most recent period is especially relevant because the Service states in multiple places that "the population stabilized" during this approximate 15-year period. In other words, at the same time that the Service claims that the population did not increase, human-caused mortality was increasing by 7% per annum. If true, then human-caused mortality rates axiomatically substantially increased between 2002 and the present.

Parenthetically, if mortality rates were increasing during this 15-year period, one would expect the beginnings of a population decline. Elsewhere I present evidence that such a decline is, in fact, evident. But my point here takes the Service's assertions regarding population growth and the data pertaining to human-caused mortality at face value. The inescapable conclusion is that all of the management actions referenced by the Service on pages 13206 and 13207 of the Rule at best kept increases in human-caused mortality from being worse than they already were, but not enough to prevent the major observed increases.

As a bottom line, the only defensible conclusion to be drawn from the best available scientific information is that past (and prospective future) management actions were not sufficient to curb increases in human-caused mortality and, for that reason, human-caused mortality constitutes a threat to the GYE grizzly bear DPS now and in the future.

---

19. The Service employs methods for monitoring population trend and mortality rates that are unreliable, optimistically biased, insensitive to unfolding conditions, and prone to producing nonsensical results. These methods do not provide dependable information regarding status and trend of Yellowstone's grizzly bear population now or when prospectively applied after proposed removal of ESA protections. Moreover, the Service's methods are likely to allow for over-killing.

19.1. The Chao2 method adopted by the Service for monitoring population trend does not represent the best available science and is, moreover, beset by biases that have introduced systematically inflated and overly-optimistic estimates of trend for the Yellowstone grizzly bear population.

The Service repeatedly asserts throughout the Rule, CS, and Recovery Plan Appendix that the so-called Chao2 method is "the best available science" for estimating population size and, from that, population trend. These assertions are unsubstantiated and contradicted by what is, in fact, the best available science. Even more mystifying and problematic, the Service fails to acknowledge ample of evidence of bias affecting the Chao2 method, which results in systematically inflated estimates of trend for the Yellowstone grizzly bear population.

The Chao2 method is driven by counts of unduplicated females with cubs-of-the-years (Females with COY). Once these counts are ascertained, the Chao2 calculation presumes to account for all of the females with COY that were not detected (unknown, unrecorded). Once the known and unknown females with COY are added together and summed over a three-year period, this presumed estimate of total numbers of reproductive females in the population (given a 3-year reproductive interval) is multiplied by various factors to account for other sex- and age-classes of grizzly bears, thereby yielding a purported estimate of total population size.

Doak and Cutler (2014a, 2014b) present a compelling critique of the Service's approach to estimating population trend based on counts of females with COY, adjusted by the Chao2 method. Doak and Cutler show that essentially all of the positive population trend presumably exhibited by Yellowstone's grizzly bear population between the early 1990s and the present is likely to have been an artifact of biases introduced by increased search effort and increased intrinsic sightability of bears. Van Manen et al. (2014) attempted to refute Doak and Cutler's original critique, but were refuted, in turn, by Doak and Cutler's more recently published paper (2014b), which constitutes the last word insofar as the best available science is concerned. Interestingly, the IGBST itself admits to the very bias identified by Doak and Culter in Table 2.1 of the Workshop Report (IGBST 2012) that the Service invokes throughout the Rule.

The figures below are illustrative of the major problems besetting the Service's Chao2 method. For one, the Chao2 calculation introduces an implausibly small adjustment to presumably account for unseen undocumented females with COY—on average, only an additional 6, or 19% more, per year. In other words, the Service is essentially claiming that roughly 81% of all females with COY are seen and documented, which is *prima facie*, unlikely. Similarly, as figure 19.1.1 illustrates, adjusted and underlying counts are 0.92 correlated, which means, not only that underlying counts of females with COY explain 84% of the total variation in resulting estimates of the cohort total, but also that the Chao2-adjustment introduces essentially no additional information. As a bottom line, it is highly unlikely that the Chao2-adjustment corrects for much of anything, meaning that the resulting estimates of total population size are driven almost entirely by counts of females with COY alone. Importantly, these underlying counts are based on all sightings from all sources, without accounting for any factors that might influence such sightings.

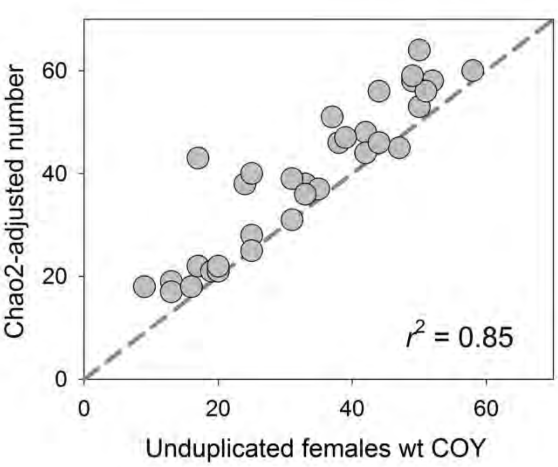


Figure 19.1.1. The relationship between annual numbers of unduplicated females with COY (x-axis) and the total number of females with COY after adjustments introduced by the Chao2 estimator (y-axis). Each gray dot represents one year's data; the dashed line represents a perfect 1:1 relationship.

Figure 19.1.2., to the right, illustrates perhaps the central problem with the Chao2/females with COY method. The top graphic in this figure shows long-term trends in numbers of females with COY (dark gray dots) along with the minor adjustments introduced by the Chao2 calculation (the light gray line above). This trend is the primary (but not sole; see my comments below) basis for the Service's claims regarding increases in Yellowstone's grizzly bear population. Then notice the trend lines in the graphic at bottom. The burgundy dots show the numbers of hours flown by researchers and managers in efforts explicitly designed to sight females with COY, which matters because roughly 66% of all sightings of females with COY are made from the air. The yellowish-green dots show the number of sites known to be used by bears feeding on army cutworm moths, which matters because, unlike any other feeding activity, essentially all of the bears engaged in this activity are seen by aerial observers (O'Brien & Lindzey 1998). In other words, levels of feeding on moth sites are indicative of intrinsic sightability of the bears being sought out by researchers and managers.

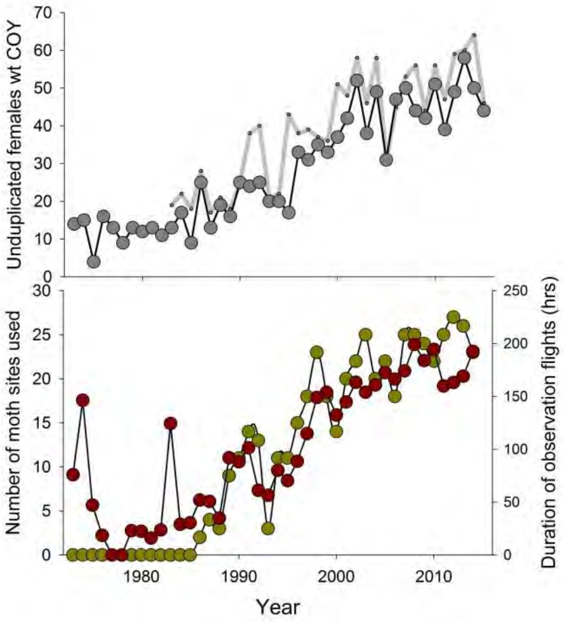


Figure 19.1.2. The top graphic shows annual counts of females with COY (gray dots) and presumed adjustments by the Chao2 method to account for unseen undocumented females. The bottom graphic shows aerial search effort by managers and researchers as part of observation flights (burgundy dots) and number of moth sites used by grizzly bears (yellow-green dots).

The parallels are striking. Presumed trends in counts of females with COY almost perfectly mirror search effort and moth site use, the latter (as I point out immediately above) an indicator of overall sightability. Figure 19.1.3 puts this in more literal terms by relating annual counts of females with COY to aerial effort (top) and number of exploited moth sites (bottom). The take-away here is that, depending on which relationship you want to consider, search effort could explain 70% and moth site use 80% of the total variation in annual counts of females with COY—which leaves little residual variation to reflect much of anything happening with underlying true population trend. These relationships simply reiterate in graphic form the main critique of the Chao2 method made by Doak and Cutler (2014a, 2014b).

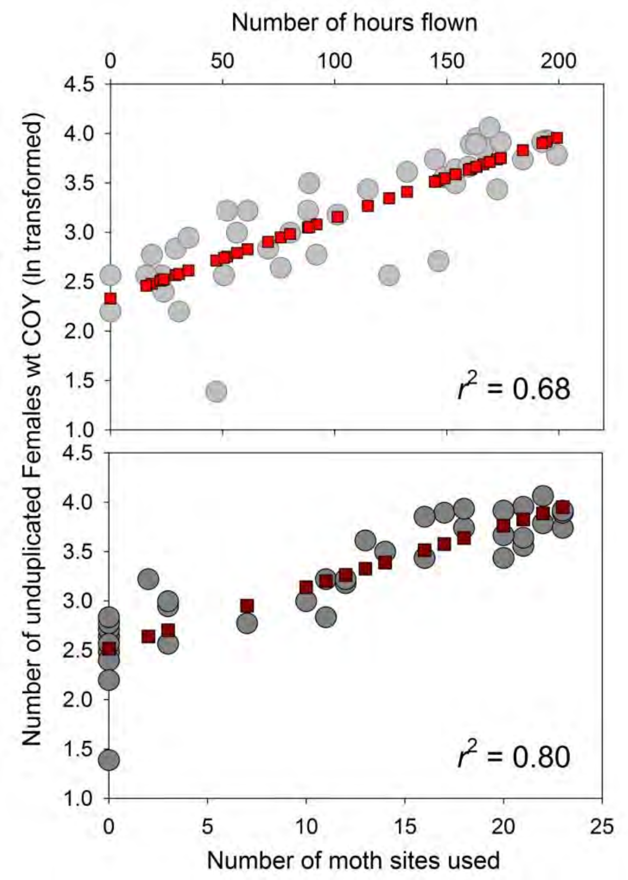


Figure 19.1.3. The figure at left shows relations between annual counts of females with COY (natural-log transformed) and aerial search effort (top) and number of moth sites used by grizzly bears (bottom). The gray dots correspond to data for a given year and the red squares to the predicted value given the modeled relationship.

Unfortunately, Chao2 estimates and underlying counts of females with COY have continued to be contaminated with bias during the last 20 years from increases in search effort and sightability—despite claims by the Service and IGBST that search effort has more-or-less stabilized. Illustrative of my point, figure 19.1.4. shows trends in aerial search effort, moth site use, counts of females with COY, and Chao2 adjustments for this period.

Parenthetically, the IGBST has argued that aerial search effort increased simply as a function of the increased distribution of Yellowstone's grizzly bear population. As it turns out, even when standardized to distribution of the population at any given point in time, aerial search effort per unit area doubled between the mid-1990s and late 2000s.

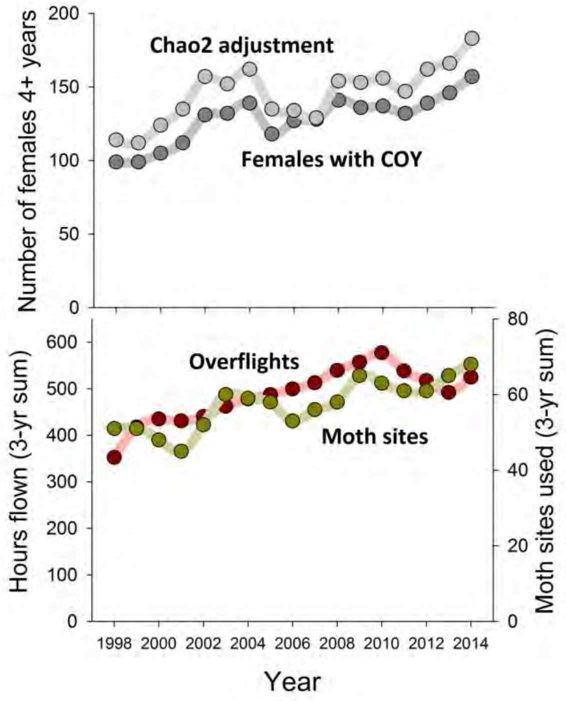


Figure 19.1.4. Three-year sums of females with COY and related Chao2 adjustments (top) and 3-year sums of aerial search effort and moth site use by grizzly bears (bottom) for the period 1995-2014.

In short, if you have a compelling argument showing that most variation in annual estimates produced by the Chao2 method is an artifact of bias—as I have demonstrated here—any assertion that the method produces reliable and useful indicators of trend is essentially arbitrary, if not capricious—as with the Service's assertions that this method is reliable and "the best available science."

Having made this point, deficiencies in the Chao2-based approach could be partly remedied by having the Service insure that search effort and search distribution remain constant in the future, while at the same time relinquishing any claims to being able to estimate past trend in population size using this method (see my comments 20.4 and 20.5).

19.2. The so-called “model-averaged” approach adopted by the Service to produce estimates of population size and, from that, estimates of population trend, is insensitive to unfolding problematic conditions. Estimates of trend from this approach are also vulnerable to manipulation depending on the time period adopted for model specification.

Even taking the egregiously deficient Chao2 method at face value, the so-called “model-averaged” approach adopted by the Service contributes to an overall method that is remarkably insensitive to rapidly unfolding conditions in the Yellowstone ecosystem. This matters because, as I document extensively in my comments, environmental conditions are, at best, rapidly changing and, much more likely, substantially deteriorating.

The model-averaged approach basically fits a regression model to Chao2-adjusted annual estimates of total females with COY, and then uses the intercept and slope from the model to, in turn, estimate the current year’s total. This approach presumably “smooths” short-term trends. Moreover, the regression model is fitted to data going back to 1983, presumably to produce a “reliable” estimate of trend with narrower confidence intervals, this as a consequence of employing a larger *n*.

The logic behind this approach is inane. The Service is basically substituting statistical precision for ecological relevance by inflating sample size through inclusion of annual data that have long since become irrelevant to judging status of the population. Why include annual counts of females with COY from 1983-1995, or even from 1995-2000, given the dramatic changes that have occurred and continue to occur in the Yellowstone ecosystem since the mid-1990s and early 2000s (see my comments elsewhere)? This conflation of precision with ecological importance is an error that most textbooks on biostatistics warn against—but apparently to no avail with the Service. More specifically, for the regression methods employed by the Service (and IGBST) to be valid, the distributional relationship of females with COY to time is assumed to be “stationary”—in essence meaning that underlying system dynamics are not changing. This assumption clearly does not hold here.

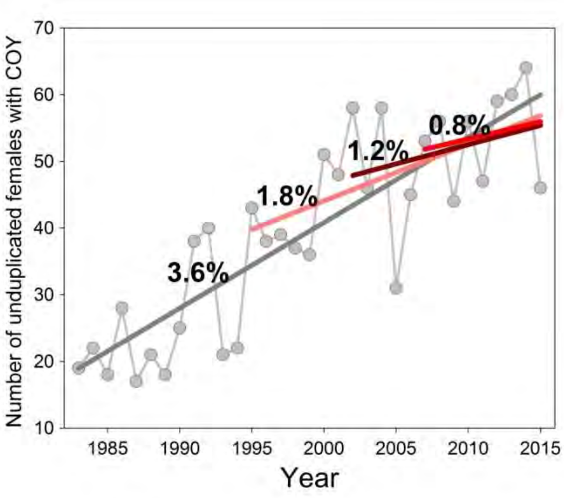


Figure 19.2.1. This graph shows trend lines fit to data for numbers of females with COY in the Yellowstone ecosystem, with each line representing a fit to a different span of years. The percent change per annum is shown for each trend line, emphasizing that estimates of trend change and become progressively approach zero as the time interval is truncated closer to the present.

Moreover, results of the “model-averaged” approach depend substantially on the time period being modeled—not only in generating an estimate of trend, but also in producing an estimate of population size (as a function of the β coefficient indicating trend). This matters because, the farther back you go in time with Yellowstone’s grizzly bear data, the more optimistically you bias your results. So the Service (or whomever) can more-or-less arbitrarily generate different results by basing estimates on different periods of time.

Figure 19.2.1 illustrates this problem. I show annual Chao2-adjusted estimates of females with COY as gray dots in the background. I also show trend lines fit by regression to natural-log transformed values, with each line corresponding to results using different relevant periods of time: 1983-2015 (the Service’s default); 1995-2015 (when we began to see major declines in trout and elk as per my comments elsewhere); 2000-2015 (when we first started to see major beetle-caused whitebark pine mortality); and 2007-2015 (when we saw the terminal decline in availability of whitebark pine seeds, also as per my earlier comments). The numbers above each line are the trends estimated for each period, represented as annual percent change. The point is pretty basic: population trend declines as you progressively truncate the included years, which, in turn, yields a lower estimate of current total population size.

Finally, the Service's use of a model-averaged approach is flawed because it makes a further nonsensical assumption: that there is some theoretically-justified intrinsic relationship between counts of females with COY and time that can be specified mathematically and statistically in terms of an underlying model. A relationship between numbers and density? Perhaps. A relationship between numbers and food abundance? Yes. But not a relationship between numbers and the mere passage of time.

Even if one were to buy off on this unsupported assumption, one then needs to justify the form of the model adopted for estimating trend as a basis, in turn, for estimating total numbers of females with COY. Linear? Quadratic? Cubic? Asymptotic? Logistic? A different choice will yield a different model-based estimate of current population size. And yet the Service employs linear and quadratic models, without statistical or theoretical justification. In short, this aspect of the method, like choice of time frame, entails arbitrary and capricious decisions on the part of the Service.

The Service needs to abandon its "model-based average" approach given the insensitivity of this method to rapidly changing environmental conditions in Yellowstone and because it does not constitute the best available science. An approach based on a running average of annual growth rate over a sensible number of preceding years—say six—would probably service the purpose instead.

19.3. The mortality rates presented by the Service in Tables 1 and 3 of the Rule as benchmarks for managing mortality are liberal and thereby allow for over-killing of grizzly bears relative to objectives keyed to different population sizes.

The basis for this concern arises from the recent critique of IGBST estimates of population trend by Doak & Cutler (2014a, 2014b). They showed that there was good reason to think that population growth rate had been over-estimated by the IGBST using both the Chao2-based method and more complex calculations using death and birth rates estimated from known fates of radio-marked grizzly bears. More specifically, Doak & Cutler found that Schwartz et al. (2006) and IGBST estimations thereafter had not accounted for senescence in both birth and death rates of female grizzly bears, and that when accounted for, female survival rates and population growth rate (as estimated by Harris et al. 2006) dropped significantly.

Van Manen et al. (2014) claimed to have rebutted the original Doak & Culer (2014a) analysis. Aptly enough, the response of Doak & Cutler (2014b) to this presumed rebuttal was entitled "Doth Protest too Much." More specifically, Doak & Cutler found that the Van Manen et al. response was larger confirmatory of their original critiques. To quote Doak & Cutler: "[the Van Manen et al.] results show that incorporating senescence sharply reduces previously estimated population growth from 1983 to 2001"; and "[the Van Manen et al.] study shows that there is so much uncertainty in population estimates that inferences about population trends are extremely weak." In other words, the collective results presented, not only by Doak & Cutler (2014a, 2014b), but also by Van Manen et al. (2014) show that population growth rate from 1983-2001 was over-estimated by the IGBST, and that all of the methods used by the IGBST for monitoring the Yellowstone grizzly bear population provide an "extremely weak" basis for inference.

Insofar as the Rule is concerned, the best available science shows that the mortality benchmarks in Tables 1 and 3 are too liberal by virtue of being linked to inflated estimates of population growth—which means that purported management objectives will not be achieved by employing these rates. More specifically, a 7.6% and 15% mortality rate for females and males, respectively, will not maintain a stable population, but rather yield unintended declines. And given all of the other problems with methods for monitoring and responding to mortality rates that I describe in my comments 19 and 20, there is a good chance that such declines will not be detected nor addressed in a timely manner, and certainly not in a way that would allow a meaningful response to prospective lags between environmental degradation and demographic responses (my point 5).

Given these considerations, the Service needs to: (1) acknowledge rather than glibly dismiss the unreliability of all its current indicators of mortality and population growth rates; (2) revise all of the benchmark mortality rates in Tables 1 and 3 downward by several percentage points to acknowledge and account for the liberal bias of the rates they currently use—for example, from 7.6% down to 5.6% for females at a population size of ≤674; and (3) put its move to delist Yellowstone's grizzly bear population on hold until it has a better basis for managing mortality.

19.4. The Service employs a method for indexing annual mortality rates that has no known or unbiased relationship to the mortality standards/thresholds presented in Tables 1 and 3 of the Rule. As a result, allowable levels of mortality calculated using the Service's proposed methods for post-delisting management entail a non-trivial risk of over-killing Yellowstone's grizzly bears, and thereby pose a threat.

The mortality rates codified in tables 1 and 3 of the Rule are presented by the Service as if they were reliable guidelines for managing grizzly bear mortality to achieve either population stability, increase, or even declines. The rates associated with maintaining a stable population (for example, 7.6% per annum for independent-aged females) are assumed to be sufficient for the purpose because they are associated with presumed population increases. But there are major problems with this approach sufficient to nullify it as a reliable guide for management.

The Service proposes to manage grizzly bear mortality so as to achieve various population goals by comparing the ratio for a given year of estimated total dead to total live bears against a benchmark rate calculated from the known fates of radio-marked bears. In other words, an estimated total number of dead bears (*D*) is divided by an estimated total population size (*N*) for a given year to yield a purported estimate of death rate (i.e., a ratio of dead to live bears)—this for each of the monitored sex-age classes of grizzly bears. Total dead bears are estimated using methods described in Cherry et al. (2002) and total live bears using the Chao2-based estimation of total reproductive females, coupled with multipliers to account for dependent young, pre-reproductive females, and independent males. This purported rate is then compared against a benchmark rate billed as being selected so as to achieve the management purposes attached to a given population size: e.g., ≤675, 675-747, or >747.

The benchmark rate was calculated as (essentially) the probability that a given radio-marked bear would have died during a given year at a given age—in other words, based on known fates of bears that had been captured and ratio-tracked. Any given rate based on known fates is related to a prospective population objective (growth, stasis, or reduction) based on simulations of population growth using a range of birth and death rates. In other words, if the population was estimated to be growing or stable, then the death rates computed from fates of radio-marked bears are estimated to be those compatible with any future population growth or stasis.

One key assumption in the management approach described in the Rule and MOA is that there is equivalence between population growth rate and death rates. This assumption is tenuous at best because it does not deal with variation in birth rates: population growth is, axiomatically, the difference between birth and death rates. In other words, one half of the equation is left out of any explicit consideration. The only presumed corrective is allowances made in both the Rule and MOA for a reevaluation of demographic rates by the IGBST should the grizzly bear population be declining for reasons that don't comport with applied death rate guidelines. And this is to happen only if prescribed death rates are exceeded 3 years in a row (for problems with that provision see my comments under 20.3).

But the other important assumption is that the calculation used to estimate annual death rates ($\hat{D}$ / $\hat{N}$) correlates perfectly with the benchmarks estimated from known fates of radio-marked bears—and with a 1:1 slope. As it turns out, the exact relationship between the benchmarks and the index being used to monitor death rates is unknown. Certainly—emphatically—there is no equivalence between the methods underlying each. Put another way, even if we calculate a putative death rate of 7.6% for adult females in the population during a given year, we don't know whether that 7.6% is either "real" or unambiguously equivalent to the rate being proffered as a standard and a guideline.

This is a case where apples are being compared to oranges and where, moreover, the Service is either not identifying this discrepancy or even cognizant of it in the first place. But, again, as with lack of attention to birth rates, the presumed corrective will be some sort of management review if and when a mysterious population decline were to occur—but then almost certainly much delayed because of the current provision for review only if mortality standards are violated three years in a row (and, again, see my point below).

What makes this situation all the more risk-ridden is the *fact that the methods by which total deaths and total live bears are calculated are prone to substantial bias (* $\hat{D}$ *and* $\hat{N}$ are merely biased estimators)—of a nature that can both amplify or dampen bias in a relationship with benchmarks that is already unknown. Hence, the nature and magnitude of change in bias from one year to the next is, and will continue to be, unknown, with the distinct possibility of unintentionally over-killing bears.

More specifically, as the IGBST (2012) notes in table 2.1 of a recent review of population monitoring, the method for estimating total mortality is "Slightly Low (slightly more deaths may have occurred than estimated because heterogeneity in data greater than accounted for in estimator; effect would lead to underestimating total mortality)" (see my comment immediately below). And, if "Low" is approximately of the same magnitude as "Low" (in the same report) for the Chao2 estimator, then there are major problems. Likewise, the Chao2-based estimator of total population size is likely to vary with search effort (as I document in my point 19.1). In other words, if there is little effort invested by managers in finding bears, then estimates of population size will probably be increasingly biased low, in ways that could partially offset or mitigate for underestimates of total mortality. But—importantly—the Service doesn't know exactly how these biases interplay. And search effort to document females with COY has increased dramatically, so the bias towards a low estimate of population size is lessening at the same time that our estimates of total mortality are varying in unknown ways relative to biases introduced by "heterogeneity" (Cherry et al. 2002, USGS 2012). In other words, bias is varying all over the place in unknown and undocumented ways, with potentially major effects on a metric central to monitoring the Yellowstone grizzly bear population, now, and in the future.

Finally, estimates of total population size are substantially affected by the multipliers used to account for dependent young, pre-reproductive independent females, and independent males, all with the potential for introducing yet more bias. As I point out below (my point 19.4), the multiplier used to account for independent males was increased substantially during and after 2012, according to the IGBST because death rates for independent males had decreased—substantially. Yet, as I point out below (point 19.4), survival rates of independent males very likely did not increase, but rather decreased—the opposite. Which is to say that a bogus multiplier was applied resulting in an artificial inflation of total population size by around 100 bears, with this biased estimate of total population size then used as the denominator for indexing death rate—with predictably yields a lower "rate." And this on top of the systematic inflation of total population estimates introduced by continuing increases in search effort and sightability of bears (see my point 19.1).

As a bottom line, the method being billed by the Service as insurance against over-"harvest" of grizzly bears is beset by a substantial amount of uncorrectable bias that introduces non-trivial risk of over-killing bears. This arises from multiple causes, including (1) using a method for estimating death rates that has no known relationship to the standards being used to guide management (the apples and oranges problem); (2) substantial biases affecting methods for estimating both total numbers of annual deaths (the death rate numerator) and females with COY during a given year (the root of the death rate denominator); and (3) bias and outright error affecting multipliers used to derive estimates of total population size. The chain of potentially compounding errors debars any confidence in the Service's method for monitoring grizzly bear death rates.

If there is a corrective, it entails, at a minimum: (1) standardizing search effort for females with COY (as per my points 19.1 & 20.5); (2) further developing the method for estimating total numbers of deaths so as to account for biasing "heterogeneity"; (3) rigorously account for variation in birth rates as part of an on-going (versus discretionary and episodic) analytic process; and (4) employing a precautionary (rather than incautious) approach to developing multipliers to account for sex-age cohorts other than reproductive females. Meanwhile, the Service needs to remove the inflated claims that it makes in the Rule for the efficacies of its current approach to managing mortality.

19.5. The Service produces inflated estimates of population size and trend that are largely an artifact of implausibly high estimates of survival rates for male and female bears 2+ years old. Moreover, these survival rates are also insensitive to rapidly changing conditions. Together, these short-comings constitute a methodological threat to the Yellowstone grizzly bear population.

As I preview in my comments above under point 19.3., the Service uses estimates of death rates derived from fates of radio-marked bears at several critical junctures in its proposed and current methods for monitoring and managing mortality of Yellowstone's grizzly bears. For one, these rates are the basis for the seminal benchmarks presented in Tables 1 and 3 of the Rule for achieving either population increase, stasis, or decline—depending on estimated population size. The mortality rates are also directly used to determine the proportions of different sex-age classes in the population, in turn, the basis for multipliers used to arrive at total population estimates. The estimated proportions of independent males, pre-reproductive independent females, and dependent offspring are essentially inverted and each used to multiply annual Chao2-based estimates of total reproductive females.

These multipliers obviously can have major effects on total population estimates. For example, beginning in 2012 the IGBST began using a larger multiplier to account for number of independent males, this because they claimed that the most recent estimate of death rates for males, using data from 2002-2011, had decreased. Hence there were presumably more males in the population than had been thought. In fact, the difference between population estimates using the old and new multipliers averaged 107 for 2012-2014, which amounted to an instantaneous 17% increase in total population size. Axiomatically, this substantial increase in the denominator for calculating the index of mortality rate translated into decrease in this rate—by roughly 15%. In other words, depending on the multipliers, you can be either substantially over or under a given mortality rate threshold such as those presented in Tables 1 and 3 of the Rule.

For these reasons the estimates of death rates derived from fates of radio-marked bears need to be unimpeachable. But there are two major problems with these rates, one of which is chronic, and the other particularly evident during the last 15 years.

The chronic problem has to do with the inherent extent to which death (and birth) rates calculated from fates of radio-marked bears are insensitive to rapidly changing conditions. This arises from the fact that reliable estimates depend on large sample sizes, and the only way one can come up with a large sample size is to include data that span a number of years—a decade or so. In other words, these death rates (or, inversely, survival rates) are slaved to the past and, in an environment such as Yellowstone's, 10 years can rapidly become irrelevant to the present and near future. In fact, this problem holds for all estimates obtained from fates of radio-marked bears, including birth rates. As a result, episodic future reviews of demography by the IGBST, through the lens of data from ratio-marked bears, will stand little chance of offering critical insights needed to remedy deteriorating population-level conditions—contrary to assertions in both the Rule and the MOA.

But the more important problem is the extent to which death rates derived from fates of radio-marked bears are not only discrepant from, but also fundamentally at odds with, straight-forward and unambiguous trends in numbers of grizzly bears dying in the Yellowstone ecosystem.

Figure 19.4.1 illustrates this problem. The red lines show three-year running averages for numbers of known-probably deaths of female (top) and male (bottom)grizzly bears >2 years old. The black dashed line towards the top of each graph shows what is probably our most reliable annual estimates of total population size derived from the Mark-Resight method (a less biased although less precise estimator compared to Chao2). I show the two trends in juxtapose for good reason given that death rate (as above) is essentially the number of bears dying as a fraction of the number of bears alive. In other words, if the number of live bears is static or declining at the same time numbers of dead bears are increasing dramatically, then death rates *must* be increasing. And, in fact, numbers of male and female deaths were increasing at an astounding rate of 9-10% per annum for the period 2002-2011, at the same time that total population size was essentially static. In other words, death rate *must* have been increasing dramatically during this period of time for both sexes.

Yet, as I noted above, the IGBST claims (in its 2012-2014 Annual Reports), not only that death rates of male bears were *decreasing* during 2002-2011, but also that death rates of female bears *remained unchanged*. It is as if the data I present in figure 19.4.1 and the data the IGBST used to calculate revised death rates were drawn from two different universes. Which are to be believed? I would argue that the data I present here (all from IGBST databases and Annual Reports) are straight-forward and virtually impossible to refute, whereas the death rates estimated by the IGBST from fates of radio-collared bears derive from assumption-ridden, complex, and refutable models. There is good reason to believe the unambiguous trend data.

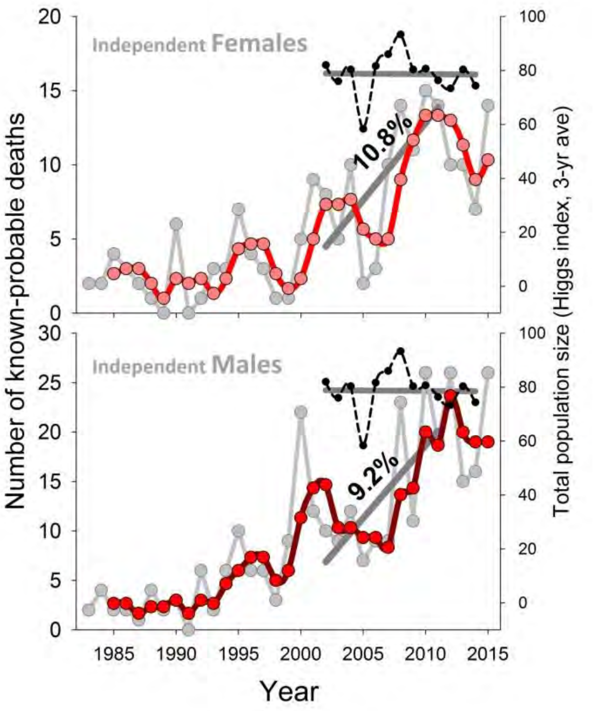


Figure 19.4.1. Trends in annual numbers of known-probable grizzly bear deaths for females (top) and males (bottom) juxtaposed with trends in annual estimates of total population size using the Higgs et al. (2013) Mark-Resight method. The red lines are 3-yr running averages of deaths and the light gray lines behind the annual numbers. Total population size is shown by the dashed black line. I've also included trends lines and associated estimates for deaths, 2002-2011.

In short, the death rates that the Service draws on so heavily in the Rule and that the states employ so centrally in the MOA stand impeached. At the very least the Service needs to adequately explain the contradictions that I high-light here. Moreover, the Service's frequent assertions that current mortality rates do not threaten Yellowstone's grizzly bear population are unsubstantiated and, in fact, contradicted by the best available science.

19.6. The method adopted by the Service for estimating total numbers of grizzly bear mortalities during a given year tends to under-estimate this total and is insensitive to unfolding trends that have likely increased rather than decreased the magnitude of this under-estimation.

The Service repeatedly invokes the method developed by Cherry et al. (2002) as its basis for estimating total number of grizzly bear deaths, a value which is then used in the numerator of the calculation used to annually index death rates in the Yellowstone ecosystem. This rate index is compared to the benchmarks in Tables 1 and 3 of the Rule to determine whether numbers of deaths were compatible with different management objectives. I describe other problems with the Service's overall method for monitoring and managing mortality under other points but focus here on problems with its adopted method for estimating total numbers of dead bears.

There are two basic and relatively well-recognized problems with the Cherry et al. (2002) method. First is a tendency to under-estimate total mortality. Second is a related vulnerability to systematic bias interjected over time as a result of changes in cause of death as well as capture and radio-collaring efforts.

The risk-inflating tendency of this method to under-estimate mortality is noted in Table 2.1 of the seminal 2012 IGBST Workshop Report: "Slightly Low (slightly more deaths may have occurred than estimated because heterogeneity in data greater than accounted for in estimator; effect would lead to underestimating total mortality)." This is a chronic problem that is exacerbated or mitigated by changes in bias affecting estimates of total population size—which is the denominator in calculations of annual indices of death rate.

But there is a second problem, prominently noted by Cherry et al. (2002): "The assumption of a constant reporting rate for radio-collared bears over time was important in our specification of the prior and in application of the method to the count data. This assumption could be violated if, for example, the probability of a death being reported depended on the cause of death and these causes changed over time. Mattson (1998) argued that this has in fact occurred…there is some evidence that reporting rates have declined in recent years." In other words, if causes of death are trending towards those that are less likely to be reported, then the Cherry et al. (2002) method will be prey to an ever-increasing under-estimation of total deaths.

Moreover, estimates of total deaths will be further biased by level of effort to capture and radio-collar grizzly bears in the Yellowstone ecosystem. This bias arises from the fact that deaths of radio-collared bears are not subject to the multiplier introduced by Cherry et al. (2002) to presumably account for unreported-unknown mortalities. In other words, if ever more effort is being exerted to capture bears, resulting in ever-more radio-marked bears, you are likely to end up with a larger number of deaths each year attributable to marked bears and thus not subject to any adjustment to account for unknown mortalities—this simply as a function of effort on the part of researchers and managers and without any intrinsic relationship to numbers of bears dying.

Figure 19.5.1. shows that grizzly bear captures have in fact increased at a far more rapid rate than any probable change in total population size, which is consistent with capture effort alone magnifying the extent to which total grizzly bear mortality is being under-estimated. As shown by the yellowish-green dots, numbers of bears captured and then monitored have increased at 5% per annum since 2002, at the same time that estimates of total population size based on the Mark-Resight method have essentially not changed (the gray dots). Ergo, a larger fraction of the population is being marked, predictably yielding a larger number of dead bears that were radio-collared—which is, in fact, the case, at a rate of roughly 4% per year since 2002.

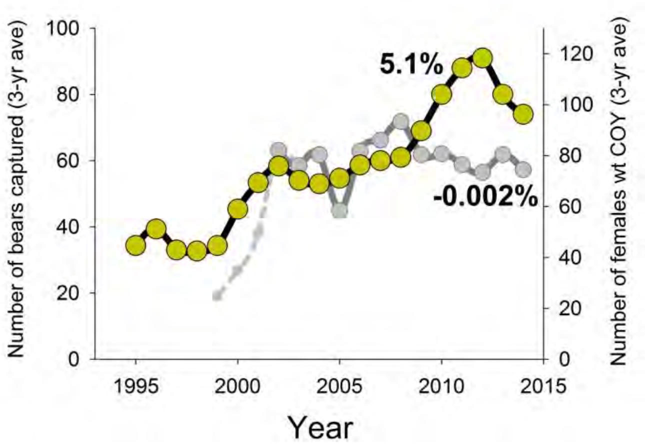


Figure 19.5.1. Trends in numbers of bears captured each year (yellowish-green dots) and estimated total population size (gray dots, based on Mark-Resight). Both values are presented as 3-year running averages. along with estimates of annual change in each time series by the numbers shown for each.

In short, the Service fails to acknowledge the several problematic biases affecting its promoted method for estimating total numbers of dead bears in the Yellowstone grizzly bear population. On top of this, the Service fails to acknowledge that these biases amplify risk and, perhaps more importantly, that these biases have probably increased over time resulting in an ever more pronounced under-estimation of total mortality. As a result, death rate is probably being increasingly under-estimated, leading to increased errors by managers regarding the sustainability of current levels of mortality.

The Service needs to acknowledge these biases affecting estimation of total grizzly bear deaths and the risks that they bring. Related, the Service needs to surrender any of its claims regarding past trends in mortality rates and focus, instead, on efforts to improve methods so as to reduce bias. At a minimum these include upgrading the current Cherry et al. (2002) method to account for changes in cause of death for unmarked bears, and standardizing capture efforts so as to not introduce bias arising simply from increased exertions on the part of managers and researchers.

20. The Rule and the accompanying Memorandum of Agreement (MOA) developed by the states for managing Yellowstone’s grizzly bears post-delisting are inadequate in provisions for calculating and managing total allowable mortality. Moreover, both the MOA and the Rule are deficient in terminology and provisions for managing grizzly bear distribution.

20.1. The methods described in the Rule to account for ‘background’ mortality are not only discrepant with methods described in the MOA, but also fail to account for unknown-unreported grizzly bear deaths, thus constituting a major methodological threat to the Yellowstone grizzly bear population.

On page 13203 of the Rule the Service walks the reader through an example of how ‘discretionary’ mortality will be calculated—of which presumably all could be provisionally allocated by the states at the beginning of a given year for sport hunting. The example calculations purport to account for ‘background’ mortality, which is defined in the previous paragraph as including various causes, including “unknown/unreported calculations.” Yet the example given by the Service, fails, in fact to account for “unknown/unreported” mortalities, leading to an inflated estimate of the number of bears candidate for ‘discretionary’ mortality. This is a major error given that discretionary mortality for independent females and males ends up being over-estimated by approximately 75% and 200%, respectively. In other words, this is a non-trivial even fatal mistake on the part of the Service.

The Service needs to account for unknown-unreported mortalities in its calculations. Given the current IGBST method used to estimate unknown-unreported deaths, this foreseeable cause needs to be subtracted before other foreseeable ‘non-discretionary’ deaths are deducted. Given that the unknown-unreported fraction during 2010-2014 has averaged 39% of the total known-probable deaths for independent females and 37% of the same for independent males, these fractions need to be the first to be subtracted from the initial calculation of allowable mortality. After that, other ‘non-discretionary’ causes can be deducted.

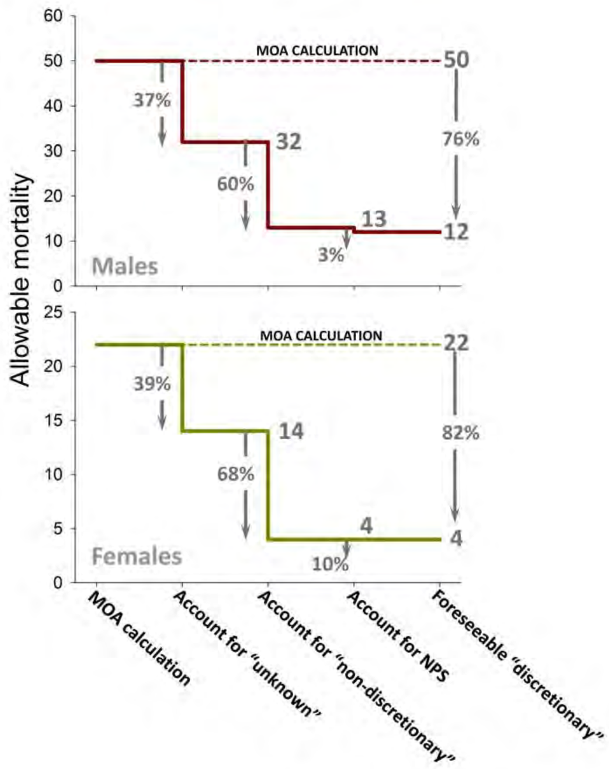


Figure 20.1.1. An illustration of reductions in allowable mortality that need to happen if unknown-unreported and other ‘non-discretionary’ deaths are to accounted for, in addition to ‘discretionary’ deaths foreseeably occurring on NPS jurisdictions—prior to any determinations regarding bears available for ‘discretionary’ management on non-NPS lands.

The other major missing piece in the Service’s proposed method for estimating and allocating ‘discretionary’ mortality is its lack of provision for the National Park Service. This is especially glaring in the MOA, which presumably further codifies methods described in the Rule. In other words, all of the ‘discretionary’ mortality is assumed to go to jurisdictions managed by the states of Wyoming, Montana, and Idaho, and none to National Park Service-managed lands. Recent history would suggest that this NPS fraction is roughly 10% of the ‘discretionary’ total. In other words, explicit provision needs to be made for this foreseeable mortality occurring on NPS jurisdictions, with corresponding deductions from the states' share of 'discretionary' up for grabs as a prospective sport hunt.

By way of clarification, figure 20.1.1, illustrates the nature of the stepdown that needs to occur in the Service's calculations; first accounting for unknown-unreported, then other 'non-discretionary', then the NPS share—this for a population of roughly 717. The percentages I use are approximations. Exact percentages (or at least transparent methods for calculating exact percentages) for each stepdown need to be calculated and presented by the Service

Finally, the methods described in the MOA for calculating 'discretionary' mortality are considerably discrepant from those described in the Rule. This discrepancy needs to be corrected, along with other corrections that I have outlined here.

20.2. A guideline for total mortality rate, with the intent of producing population growth, is needed in both the Rule and the MOA for an estimated population size of ≤600 bears.

The rationale for this recommendation is pretty straight-forward. Managers should set a goal, expressed in terms of total mortality rates, that will likely produce growth in the Yellowstone grizzly bear population should it drop below the basement number of 600. Even taken at face value, the mortality guidelines currently set for any population less than or equal to 674 are, by the Service's own admission, rates associated with a stable population at best.

The agencies may argue that they already have a provision for eliminating virtually all 'Discretionary' mortality once 600 is reached—tantamount to a drop in total mortality rate. This is not enough. An explicitly named target rate would provide incentive to reduce what the agencies are currently calling 'Non-discretionary' mortality once the population is at or near 600; and this mortality rate should be several percentage points below the 7.6% and 15% caps applied to males and females when the population is <675. Given that there are a 1.4% and 5% drops in the benchmark mortality rates for females and males, respectively, once the population drops below 675, it would be logical to apply a commensurate drop in benchmark rates once the population drops below 600, to around 6% for females and 10% for males. (Also, see my 20.6, below, for more on the problems of using 'Discretionary' and 'Non-discretionary').

20.3. Review of current management approaches should be mandated whenever mortality guidelines are exceeded during any two consecutive years, for any of the three specified cohorts of bears, rather than the standard of three consecutive years specified in the current Rule and MOA.

Both the Rule and the MOA specify that a review of management leading to potential changes in harvest would occur only if recommended mortality rates were exceeded in 3 consecutive years. If the sequencing was right (e.g., 2 years of excess followed by one year within bounds, followed by two more sequences of this nature), this protocol would allow the states to kill bears in excess of recommended guidelines for 7 out of 10 years, which, taking all of the other elements of the MOA at face value, would be a recipe for producing a declining population—and without provision for introducing a timely change in management. Under the current approach, a check would only be introduced if estimated population size dropped below 600, at which point, options for reversing course would be intrinsically limited.

With the change recommended here (review after 2 rather than 3 successive years of violated mortality rate targets), timely review would be triggered much more often and with the prospect of actually reversing course prior to excessive declines in the bear population. Certainly, the current proposed approach is not precautionary nor in any other way conservative.

20.4. Both the Rule and the MOA need to commit to resetting or recalibrating all aspects of the methods used to monitor trend, calculate allowable total mortality, and trigger various outside reviews if and when new methods are adopted for estimating total population size. Without this provision, the existing approach constitutes a methodological threat to the population

The Rule and MOA describe methods for calculating total allowable mortality (‘Discretionary’ plus ‘Non-discretionary’) that are highly sensitive to estimates of total population size. Given that different estimators of total population size can yield numbers that vary by as much as 40%—even using the same inputs (e.g., Mark-Resight versus Chao2; Higgs et al. [2013] and IGBST [2015])—the Service needs to include language in the Rule that explicitly guards against state agencies introducing a new method that dramatically increases estimates of total population size without any commensurate adjustments in reckonings of trend, methods for estimating total allowable mortality, or thresholds that trigger outside reviews—all of which is currently allowed in both the Rule and the MOA.

As is, state management agencies could (for example) adopt the existing Mark-Resight method for estimating total population size as soon as Yellowstone’s grizzly bears are delisted, and produce a dramatic purported “increase” in the population. This would instantaneously translate into a markedly positive increase in putative population trend along with numbers of bears available for ‘discretionary’ mortality—without any change whatsoever in the underlying population or on-the-ground conditions.

Allowance for such a scenario not only introduces substantial risk, but also, even more importantly, emasculates and otherwise renders immaterial all of the presumed safeguards against over-exploitation described by the Service in the current Rule. A population that the Service currently represents as numbering around 675 could suddenly be inflated to over 925, thereby allowing for a potential sport harvest of, not 15, but rather nearer 25, a 67% increase. Likewise, a population at a threshold of 600 that would debar all sport harvest could be inflated to over 800, with instantaneous allowance for harvest of 15 or so bears. And, even more problematic, a population at the threshold of 500, that the Rule claims might trigger a status review by the Service, could be suddenly increased to 700, well above such a trigger.

Whether such scenarios came to pass intentionally or unintentionally, they would unambiguously pose a serious threat to the population embedded in methods currently contained in the Rule. The Service needs to remedy this unacceptable risk. There are several options, amongst which the least ambiguous and straight-forward would be to commit to continued use of the Chao2 method for estimating total population size, but with an accompanying commitment to rigorously standardize search effort and distribution (see my comment 20.5 below). This would help control for the bias that besets the Chao2 method. Another option would be to commit in the Rule and MOA to use the lower bound of uncertainty intervals for estimates of total population size generated by the Mark-Resight method, should it be adopted. This would presumably mitigate for the major short-coming of this method identified by IGBST (2015), which is the large uncertainty in annual estimates.

20.5. The MOA and the Rule need to explicitly specify that population monitoring will continue indefinitely at the same intensity (neither more nor less) and according to the same design as occurred during the 5 years prior to delisting.

Given vulnerabilities of the Chao2 method—or any other foreseeable method—to bias introduced by search effort and intrinsic sightability of bears (see my comments under 19.1), the Rule and MOA both need to contain a commitment to maintaining the current exact intensity and distribution of search effort devoted to documenting the presence of females with COY. Such a commitment would help curb any tendencies on the part of management agencies to temporarily inflate population estimates through increased search effort, especially if such an increase were coupled with greater orientation towards areas where grizzly bears are most easily seen.

20.6. Terminology for referring to bear mortality should be changed in the Rule and MOA from 'Discretionary' versus 'Non-discretionary' to 'Management' versus 'Other'.

The semantics of the current distinction between 'Discretionary' and 'Non-discretionary' mortality in the Rule and the MOA lead to confusion. Moreover, the distinction is disingenuous. By using these terms, the Service and state managers lead both themselves and their readers to assume that they have no control or influence over so-called 'Non-discretionary' mortalities—that this category of mortalities "needs" to happen or is the result of some act of God. This is not the case. History has shown (as the current Rule would claim) that managers do, in fact, have substantial influence over the so-called 'Non-discretionary' mortalities through activities such as law enforcement, education, and sanitation. The Rule even strongly implies that managers have influence over "natural" mortalities to the extent that sport harvest of specific cohorts of bears can amplify or dampen levels of mortality caused by conspecifics—especially infanticide (e.g., Swenson et al. 2001a, 2001b; , Bellemain et al. 2006; Bischof et al. 2009; Gardner et al. 2014). In other words, 'Non-discretionary' mortalities can, in fact, be 'Discretionary'.

When you look at the more concrete categories of bear deaths that the Rule and MOA are allocating to 'Discretionary' versus 'Non-discretionary', it turns out that the distinction is fairly straight-forward distinction. 'Discretionary' deaths are simply those that will be directly sanctioned by managers and meted out by either uniformed employees of a state agency, by Wildlife Services, or by those licensed to act on a state agency's behalf (e.g., licensed hunters). 'Non-discretionary' deaths are simply all others resulting from the actions of those (including other animals) who are not explicitly and directly authorized, in any immediate sense, to kill grizzly bears.

In fact, the current category of 'Discretionary' correlates closely with historical deaths of grizzly bears caused by managers responding to conflict situations, including threats to human safety—in other words, 'management' removals. 'Non-discretionary' correlates with all of the other historical categories. For the sake of clarity and in service of reducing ambiguity, I recommend that the Service revise the Rule so as to refer to 'Discretionary' kills as 'Management' kills and 'Non-discretionary kills' as simply 'Other'.

---

21. Occupancy provisions for adult females need to apply to all portions of the DMA, not just the PCA, stratified on the basis of what are currently called 'Flight areas'.

The current approach outlined in the Rule and the MOA essentially loads all of the 'Discretionary' mortality allotted for independent females on those without dependent young (lone females) outside of National Parks. Under current provisions, no sport harvest of females accompanied by dependent young would be allowed. This amounts to the brunt of 'discretionary' human-caused deaths among females being borne by lone bears on the periphery of the DMA.

On average, only 1 of 3 adult females will be without young during a given year. Moreover, some percentage of these lone females will be inside National Parks where they will not be subject to hunting. As a result, something less than 33% of the adult females in the population (say, 25%), all concentrated on the ecosystem periphery, will be subject to most of the planned killing each year. And, importantly, the current approach essentially uses females inside National Parks to subsidize calculations of allowable sport harvest outside.

The end result will be patently unsustainable killing of females on the periphery. Source-sink population dynamics would also certainly be amplified which, according to Doak (1995), could lead to increased vulnerability of the population to unintended and long-lagged declines.

Preferentially killing females that would otherwise have given birth to cubs the following year could introduce yet other unpredictable amplifications of population trends. On the face of it, amplified oscillations might be curbed by the fact that, with fewer females giving birth to cubs any following year, estimates of total population size based on observations of females with COY would be smaller, which might then lead to a lower 'Discretionary' kill the year after. But there would then be a pulse of cubs from females that were subsequently subject to a light harvest, which would lead to an inflated estimate of population size and a resulting inflated sport harvest the year after…and so on. In short, the approach described in the Rule and the MOA will lead to fewer females living on the ecosystem periphery and less predictability regarding the consequences of management actions.

A requirement by the Service for occupancy of all management units by reproductive females, including 'flight areas' outside the DMA (see figure 21.1 below), would introduce a curb on excesses built into the Rule and MOA, which currently have punitive implications for females outside National Parks. Alleviating these current excesses would result in proportionately greater numbers of females on the periphery, which would foster eventual connectivity between Yellowstone and the NCDE.

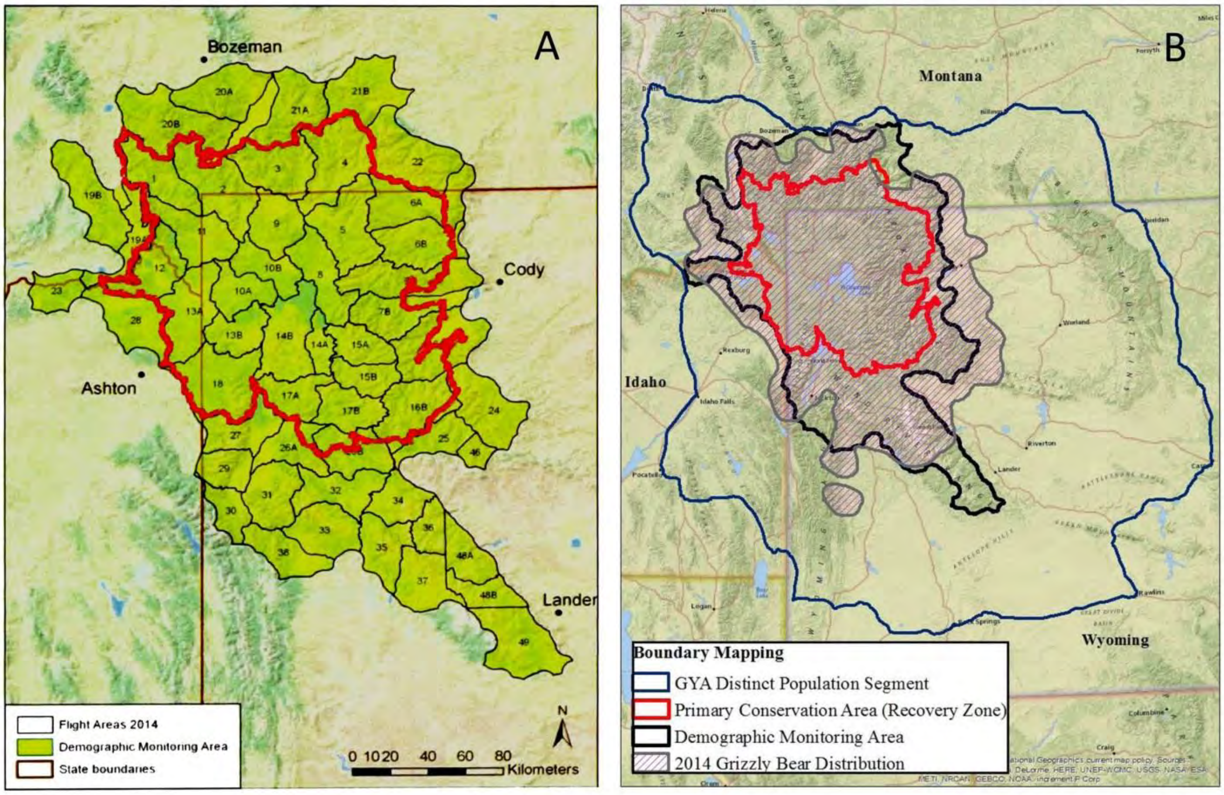


Figure 21.1. Map A, above, shows the PCA (delineated by red, as shown in map B) relative to the full extent and partitioning of the DMA (in yellow). The Rule and current MOA only provide for insurance of occupancy by adult females within the units contained by the PCA and leaves occupancy of all the other units ('Flight Areas') in the DMA up in the air when it comes to presence of reproductive females.