



## COLORADO

### Parks and Wildlife

Department of Natural Resources

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TO: Parks and Wildlife Commissioners  
FROM: Mat Alldredge, Chuck Anderson, Brian Dreher, and Jeff Ver Steeg  
DATE: December 9, 2016  
SUBJECT: CPW's response to "Open letter from scientists and scholars on the proposed CPW mule deer strategy studies"

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As you know, managing predators is controversial. Even conducting research on predators generates considerable controversy, especially when there is a paradigm shift from assumptions of ecosystems being driven from the bottom up (e.g., habitat-limited), to a recognition that top down factors (e.g., predators) play an important role. These systems are dynamic; what drives one may not drive another. Furthermore, what drives one today may not drive it next year, or in the next decade.

When we consider predator-prey systems we must consider the history of the systems. Historically, predators were treated as vermin and persecuted to near disappearance. In the 1970's states began to manage predators and in the west, especially for black bears and cougars, that management has been overwhelmingly successful. Bear and cougar populations are thriving, especially in Colorado. Given that the relationship between predator and prey is dynamic, we should not expect that 1) the relationship from the days of persecution would be the same as that during population recovery or 2) either of those are the same as what we observe today with thriving predator populations. The Piceance Basin deer herd is a prime example. In the 1980's populations were high and malnutrition, especially among winter fawns, was common. However, as we set out to better understand the effects of oil and gas development on deer, we found that winter habitat was no longer the driving factor. We learned, unexpectedly, that predation on newborn fawns appears to be the primary factor limiting the population today. We are proposing to continue to develop our knowledge of these dynamic systems as they exist today so that we can successfully manage our wildlife now and into the future. Not surprisingly, as we work to understand these changing dynamics and suggest a paradigm shift, controversy and criticism arises. Throughout this process we have listened to the concerns brought forth and attempted to address as many as we reasonably can. Here we respond to recent criticisms about the study design in the Piceance Basin and Arkansas River predator studies.

Criticism of wildlife ecology and management studies for deficiencies in design or analysis have been debated in the literature for well over 50 years. Clearly, manipulative experiments with controls, randomization and replication in space and time are desirable as they provide powerful ways to understand and demonstrate causal relationships in natural systems. However, such experiments are rare in our field because of the difficulty in studying such dynamic systems and the spatial and temporal extent that is required. Wildlife ecology has suffered from studies that lack "strong inference" (Platt 1964), suffer from pseudoreplication (Hurlbert 1984), confuse correlation with causation (Eberhardt 1970), use indices (Anderson 2001), or use inappropriate statistics (Rexstad et al. 1988, Johnson 1995). Conversely, despite all of the difficulties in studying natural systems and potential statistical shortcomings, as a profession, we have been very successful with species recovery efforts and maintaining viable and thriving populations (Thomas 2000, Johnson 2002).



Like most wildlife professionals, we are not working in a laboratory where multiple controls and treatments are available, assignment can be random and replication is simple (Conroy 2002, Williams et al. 2002:102-106). There are other limitations as well. Most predator studies suffer from small spatial scales (< 380 mi<sup>2</sup>) and limited time frames (a few years), especially when they fit into graduate research projects at universities. What CPW is proposing is unique in that we intend to conduct this research at larger spatial scales and longer time frames to account for ecosystem dynamics and environmental variability. To our knowledge, this has not been done before, but there are still constraints. Observational studies and sample surveys are common and provide useful knowledge for managing wildlife, especially with appropriate design and analysis. A major consideration in all of this is the inference that can be made at the conclusion of a study. An observational study can be very useful to understanding a particular system, but the ability to extrapolate those results to other systems is limited. Manipulative experiments with controls, random treatment assignments and replication also provide a great understanding of a particular system but, in general, results can be extrapolated to other systems with greater confidence or fewer assumptions and limitations.

Johnson (2002) suggests that more important than design and analysis of individual studies is metareplication or the replication of entire studies in different areas by different investigators. In this approach a greater confidence in the generality of findings can be obtained when similar conclusions are found across studies of the same phenomenon conducted under different conditions. In fact, this is what we were attempting to do with the original Piceance study in examining the effects of oil and gas development on mule deer, expanding on the behavioral influences addressed by Sawyer et al. (2009) by examining how the behavioral effects influenced demographic parameters.

Given this general background, we wanted to address the specific criticisms that allege the proposed projects lack scientific rigor:

#### 1. Lack of proper control

- a. *A proper set of controls and treatments would require at least 3 of each to achieve statistical robustness. Furthermore, the control sites must not experience cougar killing (legal and illegal take) and must experience every intrusion except cougars dying, e.g., the same number and intensity of intrusions as in treatment sites but no cougars killed. The current plan to allow ±10% cougar harvest in control areas is indefensible scientifically. Currently the design is flawed, just as if it were a biomedical clinical trial in which the researchers said, "experts don't know what effect this pill will have, so the control will be a low-dose and in the treatment will be a high dose."*

**Response:** In an ideal world, 3 controls and 3 treatments would achieve greater statistical robustness. Without doubt, the more something is demonstrated under slightly different situations or areas the more confidence one can have about the general applicability of conclusions. However, this is rarely possible, especially when studying population-level responses of large, free-ranging mammals at landscape scales. Most/all studies of cougars to date lack the power to detect an effect of predation because of limited sample sizes, or small study area size and very few have treatment and control areas let alone any form of replication. Cougars have large home ranges (those of males sometimes exceed 193 mi<sup>2</sup>, and females typically are around 39 mi<sup>2</sup>), and individuals can disperse long distances, yet most studies are conducted on areas less than 380 mi<sup>2</sup>, which is small relative to cougar movement. Cougar dispersal and movement patterns make it impossible to manipulate

cougar density at small scales. Thus, there must be a tradeoff between replication and study area size. In this situation our only option is to use a large study area coupled with a cross-over statistical design to get the best inference about predator-prey dynamics. In such a way, especially over a long time frame, we will develop a large amount of knowledge about the system under study and potentially be able to extrapolate to other areas with similarities. We can also use other research in a metareplication framework to enhance the reliability of results or suggest how dynamics differ between systems.

Maintaining some level of harvest on the control population should not be an issue and will not render results indefensible. As with a drug trial, when you know the drug has an effect, it is common to test different doses to determine the most appropriate dose for the desired effect because if the high dose does not differ from the low dose one might as well use the low dose. We know that cougars kill deer, but we want to investigate the effect of the cougar dose, or density, on deer survival. If cougar mortality on deer is compensatory (deer would die from other causes if cougars didn't kill them) then cougar density will not affect deer survival. However, if cougar mortality on deer is additive (deer would not have died if cougars didn't kill them) then cougar density will affect deer survival.

- b. *The Piceance Basin study, which involves killing black bears and mountain lions on one parcel of land and then comparing that to an area with no predator control from 2010-2012, is called a pseudo-control or false control. The other area was studied at a different time and place under very different conditions than today.*

**Response:** Actually both study areas were studied from 2010-2012 with no predator control, which provides baseline data prior to the experiment. During the experiment one area will receive predator control while the other does not, which provides a control and treatment area over the same time frame. This actually is a BACI (before-after control impact) statistical design.

- c. *Under the current design, each spatial unit is a single replicate. Events within a unit are not independent of other events within that unit. A more robust design would reverse-treatment within each unit, which receives a treatment by random-assignment, not by researcher selection of sites for treatments. Although the Arkansas River study looks more robust, it remains a sample size of 4 and the lack of a true control will make the results impossible to interpret scientifically.*

**Response:** Certainly each spatial unit is a single replicate, and if we consider the event as cause-specific survival within each unit, then we will have a single estimate each year. Over the nine years of the study these data can be examined longitudinally with respect to cougar density (and other covariates) to determine effects of predation on deer survival. On the surface, adding a reverse-treatment within each spatial unit would be more robust, but would also reduce the size of the study areas by half. As previously discussed, given the movement patterns of cougars it is not feasible to reduce study area sizes and effectively accomplish the desired treatment or effect on cougar density. Therefore, we must maintain study area size and acknowledge limitations in data when we interpret results. The design implemented in the Arkansas study is very robust compared to those in the published literature and will provide considerable information about the dynamics between cougars and mule deer that will improve our future management of these species.

To our knowledge, no other study has used a cross-over design, which will allow us to separate out study area influences from the response variable of interest, cause specific mortality. Additionally, comparing our results to other studies will strengthen the level of inference that can be made or identify areas that need further study.

## 2. Selection bias

*The Upper Arkansas River proposal states, "Deer data analysis unit (DAU) D-16 (Figure 1) was identified as an area where cougar suppression could be beneficial to the deer population."<sup>iii</sup> This subjective decision will invalidate the scientific value of the proposed study in a single step. When treatments are assigned according to the response variable that one wishes to measure, you have guaranteed a sampling bias that would invalidate the study. Remember, a treatment is a hypothesized solution. If one designs a study with the assumption that the solution will work, one risks intentional bias in measurement and reporting. Random assignment is far easier and more robust to these biases and protects the researcher from claims of intentional bias.*

**Response:** Certainly bias is something that we need to be aware of in research, but it does not render results invalid. Acknowledging where biases potentially exist and presenting the results in a manner to allow the practitioner to assess bias is common. Colorado has 5 mule deer monitoring populations that allow for some assessment of factors influencing deer survival. Of those 5, D-16 has documented predation as the leading cause of known deer mortality and low malnutrition rates, which would suggest that the area does not appear to be habitat limited. If other monitoring populations also showed this pattern then it certainly would have been feasible to randomly select among populations that applied. However, the other populations appear to be habitat limited and therefore cannot be part of a random selection and information on other populations does not exist. Thus, we used available data to select a study area where it made sense to study predator-prey dynamics in a system that we believe is not currently limited by habitat. We also selected the adjacent study area where there is no knowledge of the dynamics, only that it is similar in environmental characteristics. There is no assumption in the study design that the treatment is going to work, only that the characteristics are such that it is worth studying.

## 3. Small sample size

*With fewer than 6 study units (3 control and 3 treatment), there is no statistical test that can reliably confirm or reject the research hypothesis. That requirement for 6 or more arises because each unit is a single replicate. Events within a unit, such as the survival of a marked mule deer, are not statistically independent of other events (i.e., another mule deer's survival) in that same unit. They have all experienced the same treatment and confounding variables associated with that unit. We suggest a reverse-treatment design to increase the sample size but that recommendation MUST be accompanied by random-assignment or it can produce another form of bias (treatment bias). Although the Arkansas River study looks more robust because of the crossover design (reverse-treatment), it does not have random assignment and the low-level of cougar killing throughout both units and throughout the study creates a pseudo-control that invalidates the experiment. Given the four units chosen for the studies, the CPW could achieve a sample size of 8 if they are willing to assign treatment and*

*control randomly and then reverse the treatment in each unit in the following phase of the study.*

**Response:** The authors of the open letter are not correct on this point. If you have two estimates and associated variance then a statistical test (say a t-test) can be performed. We believe that the authors do not understand the analyses that are intended. If they are assuming that from each study area we will get a proportion alive or a proportion killed by cougars and want to compare those then they are absolutely correct and we would have a sample size issue. However, we are using Cox proportional hazards models (Heisey and Patterson 2006) and treating each study area as the population of inference so that we can estimate cause-specific mortality rates for each population and the associated error. These types of analyses are very common in the biomedical literature survival studies, which are typically done at the county level (survival estimates and associated error at the county level). Covariates will be included in the analyses to account for broad scale environmental factors and study area factors (i.e., cougar density, study area, year, etc.). Thus, the confounding variables that they mentioned will be accounted for in the statistical model. We have addressed the random assignment and low level of cougar harvest issue above. We have also addressed the issue of doing a reverse-treatment within each study area creating unrealistically small areas.

**References to research design and narrative explaining the principles:**

*In 1964, in the journal Science, Platt hypothesized about scientific progress with the deceptively simple title "Strong Inference" [2]. Platt hypothesized that certain fields advance slowly and others quickly because their practitioners varied in the efficiency with which they tested between alternative, opposed hypotheses. He observed that the slower fields of his time had become bogged down by the perception that their topic was too complex for simple tests. Platt [2] anticipated the argument and countered that their models were becoming too complex to be falsifiable. Falsifiability is a foundational principle of good science. Platt also predicted that slower fields had become bogged down by a focus on methods, as opposed to rapidly advancing fields that had focused on incisive experiments that forced alternative hypotheses into divergent predictions [2]. Subsequent writers have echoed his views in their particular fields (biomedical research, paleo-sciences, and population biology, among others) [3-6].*

**Response:** CPW is well aware of these statistical papers, such as Platt (1964) and a score of others documenting the deficiencies in wildlife research, including pseudoreplication (Hurlbert 1984), correlation versus causation (Eberhardt 1970), the use of indices (Anderson 2001), inappropriate statistics (Rexstad et al 1988, Johnson 1995), and the use of known false null hypotheses (Johnson 1999). Platt (1964) defined his process of "strong inference" by applying the following steps:

1. Devising alternative hypotheses:
2. Devising a crucial experiment (or several of them), with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses;

3. Carrying out the experiment so as to get a clean result;
4. Recycling the procedure, making subhypotheses or sequential hypotheses to refine the possibilities that remain.

CPW's research group is very well respected for the scientific contributions that it makes and the caliber of its personnel. These very principles, alluded to above, drive CPW's research and that is evident in both the Piceance Basin and Arkansas River research projects being proposed. Granted these may not be the "crucial" experiment, but none are when performed in natural ecosystems. We have spent decades studying ungulates and predators throughout the state of Colorado, specifically addressing questions of concern that arise while managing these dynamic ecosystems. An alternative hypothesis, the potential additive effects of bear and cougar predation on mule deer populations, has come to light through our management and research efforts. As good scientists we have developed rigorous experiments to examine the predator-prey dynamics as an alternative hypothesis while still incorporating and testing the standard hypothesis of habitat/nutrition effects.

#### References to research design and narrative explaining the principles: (continued)

*In ecology today, we see examples of both of Platt's hypothesized brakes on progress when one hears that ecosystems are too complex to manipulate experimentally, rather than calls for elegant ecological experiments as we saw decades ago [7-10]. The field of predator ecology is at that crossroads. The traditional hypothesis is that killing predators equals more prey. That view has been disputed as long ago as Leopold (1949) who proposed the alternative that functional predator populations keep ecosystems healthier. CPW is facing this question today. However salutary efforts emerged recently by predator-prey ecologists who had conducted careful experimental manipulations to exclude or include predators from complex ecological systems [11]. We see the salutary effects today in important arguments over whether wolves - and other large carnivores such as big cats - strongly shaped biodiversity by scaring herbivores and feeding on herbivores [12-14]. Resolving that scientific debate will demand strong inference. The strong inference espoused by Platt [2] is best served by gold standard experiments using random assignment to control and treatment with sufficient sample sizes to overcome random variation that may confound an elegant test of an important hypothesis.*

**Response:** The Hairston (1989) reference is a book on ecological experimental design that provides some general information. However, the author does acknowledge the conundrum that we often face. "Ecologists conducting experiments face choices, in that providing confidence in a relevant ecological process may preclude its general application, or the requirements of a sophisticated experimental design may severely decrease realism, or the use of an elaborate design in the field may put the necessary amount of replication beyond the resources of the investigator." This is certainly a dilemma often debated within CPW's research group, with the standard being study designs that will achieve confidence in the results through robust statistical designs that will produce in-depth knowledge of the system and prove generally applicable to management within the state. Of the remaining 3 papers the authors refer to as "elegant ecological experiments," one was a literature review of food supplementation, one was a study of sleeping vigilance in birds, and one was a study of predation on partridges. The study of predation on partridges is worth noting because they

employed a cross-over design, just as we are proposing. They used two similar study areas with similar partridge densities and managed one by reducing the rate of predation, while at the same time comparing the annual results with the other not subject to predation control. This was done for a 3-year period and then the treatment was reversed to the next 3 years.

Finally, CPW is well aware of the importance of predators in ecological systems, as documented in Kricher's (2009) book, and our track record demonstrates our appreciation for the value and role of these species (e.g., the reintroductions of Canada lynx, and black-footed ferrets). We are proposing brief manipulations of thriving predator populations in order to gather valuable information for the future management of both predator and prey. With the exception of Middleton et al. (2013), the remaining articles referred to by the authors are theoretical works that propose hypotheses about how predator removal has shaped current ecosystems, but those studies do not provide any experimental basis. The Middleton et al. (2013) paper documented that there were consumptive effects by wolves on prey (elk) survival, but no non-consumptive effects such as changes in body condition or pregnancy rates, suggesting wolves could be a limiting factor.

#### 4. Legal and ethical considerations

*Wildlife are a public trust asset and the proposed studies preferentially serve a narrow community of mule deer hunters and cougar hunters, while ignoring the broad public interest in healthy ecosystems, unimpaired wildlife populations, and transparent accounting for wildlife assets. If CPW is held accountable in court or by the legislature for its management of cougars and black bears, the proposed studies will not survive the legal test for a prudent trustee of the public interest in wildlife. The Colorado Supreme Court characterized the public trust in wildlife, and the privilege of hunting wildlife granted by the state, in similar language:*

*The ownership of wild game is in the state for the benefit of all the people. The right to kill game is a boon or privilege granted, either expressly or impliedly, by the sovereign authority, and is not a right inhering in any individual. The power of the state to make regulations tending to conserve the game within its jurisdiction is based largely on the circumstance that the property right to the wild game within its borders is vested in the people of the state in their sovereign capacity; and, as an exercise of its police powers and to protect its property for the benefit of its citizens, it is not only the right but it is the duty of the state to take such steps as shall preserve the game from the greed of hunters.<sup>iv</sup>*

**Response:** CPW recognizes the public's interest in wildlife and its responsibility to manage wildlife for the use and enjoyment of all the people of this state and its visitors. CPW believes these research projects are entirely consistent with that responsibility. Any reasonable definition of wildlife management should include research and these research projects have been designed to provide CPW with useful information for the understanding and future management of state wildlife resources. There is no preconceived notion of what the result of the research will be or what future management actions might be taken in response to it, if any. Rather, CPW simply seeks additional information regarding the interaction of predator and prey species in Colorado, which it believes will be of value for consideration as part of future science-based management decisions.

Nor are these research projects designed in any way to serve some narrow community of hunters; quite to the contrary. In fact, in the Piceance Basin study, only a relatively small number of bears and cougars will be removed, and only by the U.S. Department of Agriculture APHIS Wildlife Services, which is not providing any benefit to hunters. And in the Arkansas study the removal of cougars will be manipulated within the study years, but the overall number of cougars that will be removed over the 9 years of the study is expected to be similar to the number normally harvested by hunters in that area over a 9-year period without the research, so again there is no additional benefit to hunters. Moreover, there is a possibility that hunting opportunities may actually decrease during some years of the study to ensure the desired level of lion removal is maintained.

CPW's ongoing management of Colorado's wildlife has contributed to healthy ecosystems and wildlife populations. Bears and cougars provide a great example of this as currently both have thriving populations throughout the state, and in fact conflicts with humans have been on the rise.

**In conclusion**, published field research runs the gamut from observational studies to experimental manipulations, which can provide limited information specific to the study area or more compelling information that can be applied to a greater spectrum of situations. Many studies fall short because of limited time frames, limited study area size, small sample sizes, studying either the predator or the prey but not both, or poor design. The studies proposed by CPW are unique in that they are well designed, cover large areas and long time-frames, monitor both the predator and the prey (Arkansas study) and have been specifically designed to answer the question of how predators are impacting prey at a management levels using past information to focus research on pertinent management questions.

Science and knowledge progress through experimentations, analysis, interpretation, replication and valid criticism. In this response, we have attempted to illustrate the scientific rigor embodied in the planning and design of both the Piceance Basin and the Arkansas River studies. We have also made a concerted effort to carefully and logically consider the technical concerns and criticisms of earlier proposals and responded to those in a rigorous, scientific manner.

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