THE UNIVERSITY of WISCONSIN MADISON

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Dear Editors of The Journal of Wildlife Management,

We write to express concern about an article published in your journal entitled, "Effect of preventive coyote hunting on sheep losses to coyote predation" 63:600-612 [1]. Despite the many years since publication of [1], we believe the scientific literature should be corrected by retraction of this article, because it omitted essential information on methods and experimental design that were essential to fair peer review. Also, the paper contains shortcomings in design and analysis that are so severe, correction and another peer review seem necessary.

Despite the age of the article and two published criticisms of the paper [2, 3], it continues to be cited positively. Also, the article continues to be used to justify policy and management interventions by wildlife agencies, including most notably, the U.S. federal government in a 2016 Environmental Assessment (EA) document [4], which became the focus of a federal court case [5]. That EA was recently vacated by the United States District Court for the District of Idaho [5]. The court documents reveal that the lead author of [1], who is also an author of the EA [4], presented novel information about the methods in the original paper published in JWM; information that was glaringly missing during the peer review process and that most likely would alter the review of this paper. Moreover, the new information exposes a logical inconsistency in the research design of the original paper.

The revelations in the EA [4] must be treated as true, given they represent statements on the public record during a legal proceeding in two sets of legal documents [4, 5]. We infer the original article [1], was not complete or transparent about its Methods. We summarize the new information here from the EA [4]. For full quotations from the EA [4], see our Appendix. Below we indicate how the statements in the EA [4] and to the court add to the methods in the original article or pose other problems of transparency, objectivity, or reproducibility of the original article.

1. The authors reported that untreated pastures were subject to twice the "corrective summer predator management (SPM)" (p. 606), [1]. The authors treat SPM as a response variable in both the original article [1] and in [4]. However, the EA [4] reveals that the authors had authority over the SPM and its implementers. Recall, that SPM was also an intervention in response to lamb losses in[1], therefore information on SPM was a materially important element of the Methods. Yet, details about the implementers, their identity, numbers, instructions, guidelines, or approach to SPM were not explained in the original Methods (p. 608), [1]. Nor did the authors explain whether the

implementers were blinded to the treatments of aerial gunning or participated in them and no potential conflicts of interest were disclosed [1].

2. Livestock-guarding dogs (LGDs) were apparently matched between treated and untreated pastures. LGDs have been shown effective in protecting sheep from coyotes in many situations. The number of LGDs and the authors' approach to matching them were not presented in the original article, which only stated, "we paired pastures based on the use or absence of livestock guarding dogs." (p. 607), [1]. However, such matching is not logically consistent with the sequence of events – aerial gunning occurred in the winter before LGDs were released into pastures the following summers – unless the authors matched *after the fact*? Matching pastures after the fact should have been mentioned explicitly because it permits selection bias to slip in (see next point). Furthermore, matching after the fact would mean the authors could have had reliable data about the LGDs, yet they presented no detail about LGD numbers, husbandry, and their effectiveness in protecting lambs, which would confound the treatment and the SPM (see point 1. above).
3. The matching procedure deserves further scrutiny given it could only have occurred after the fact of treatment (2. above). The original article states, "Each year, pastures with aerial hunting (treated) were paired with similar pastures (untreated) that had suitable terrain and sufficient losses to justify aerial hunting but did not receive treatment for logistical reasons (limited funds, availability of aircraft, conditions unsuited to aerial hunting)." (p. 607), [1]. First, we wish to point out this is called a convenience sample and known to be vulnerable to selection bias. But further concerns arise when one reads, "Pairings were first based on similarities in habitat, the proportion of area suitable for aerial hunting, and the proportion of terrain and understory vegetation. We also made certain lambs in both pastures were of similar age, because the size and age of lambs can affect their vulnerability to predators. Lastly, we paired pastures based on the use or absence of livestock guarding dogs." (p. 607), [1]. The authors do not explain how precisely they matched all of these attributes (specially the vague terms "habitat", "suitable", and "terrain", which make the Methods irreproducible). We would have liked more description of lamb ages in in paired pastures. But most worrisome is the matching apparently failed to control the most important confounding variable (pre-treatment losses). The original article reveals that pre-treatment sheep losses were 186% higher in untreated than treated pastures (5.4 versus 2.9 losses on average, Table 2 in [1]). Despite the potential importance of past risk as a confounding variable, the authors made *no* effort to match for it, choosing instead to match treated and untreated pastures in a variety of other ways that are not clearly described. Therefore, it appears the matching was post hoc, irreproducible, and seemingly strongly biased by past losses. The description of the matching procedure appears misleading because it omits the details of post hoc coordinating with shepherds for lamb aging and LGDs, and the convenience sampling by the authors' team(s).
4. Control pastures started with 40% higher sheep densities than treated pastures. Livestock density or herd size has been shown to increase vulnerability to predation by North American canids. The difference in density between control and treatment pastures was not calculated but gross area and sheep numbers were reported in [1]; the

EA reveals the previously undisclosed existence of “established grazing management plans” [4]; see Appendix. The original article only stated, “Sheep in these areas were cared for by a shepherd who remains with the sheep, keeps the sheep band from scattering throughout the pasture, and watches for sick or dead sheep.” (p. 607), [1]. No mention of established grazing plans was made in the original, or an effort to disclose the actual area grazed instead of publishing the entire area of pasture. Moreover, “scattering throughout the pasture“ differs from the shepherd not using the entire area of the pasture across an entire summer as claimed in [4]; see Appendix. Therefore, the original article appears misleading in presenting gross area of pastures if “established grazing management plans” ever indeed existed.

Any one of the above possible biases might have gotten the paper rejected if peer reviewers had been given full information or transparent descriptions of methods. Omission of information on methods and samples, as revealed by the EA [4], represents a serious breach of scientific integrity.

A version of this document is now in the public record because of the court proceedings cited above and possible issues arising from the U.S. Data Quality and False Claims Acts. Our questions and criticisms will be shared with the scientific community, perhaps reaching the original peer reviewers. Naturally, we do not wish the reputation of this journal to be further tarnished by slow action or inaction in this regard. We recommend immediate retraction of the paper.

Sincerely and with thanks for speedy action,

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Appendix: New revelations about [1] from the Final Environmental Assessment: Predator Damage Management in Idaho (November 2016, USDA APHIS WS et al. authored by K.K. Wagner, among others) [4]. **Original text from [4] presented in boldface.**

Authors had control over "corrective predator damage management (SPM)": **"Wagner and Conover (1999) purposefully *allowed* corrective predator damage management to be conducted during the summer following aerial operations"** (p. 450, emphasis added) [4]. Authors argue that SPM is a dependent variable.

> **"The purpose of** [1] **was to determine the impact of preventive aerial operations (independent variable) as currently practiced by the WS program on sheep losses the following summer (dependent variable) AND the need for subsequent corrective predator damage management (i.e., the use of traps snares and M-44s - *also a dependent variabl*e) during the subsequent summer." (**p. 450, emphasis added) [4].

Because the authors and unnamed actors of the same agency had complete control over SPM (above), it might be considered an experimental manipulation super-imposed on their treatment (aerial gunning). After all, SPM is known widely as an intervention, because it is meant to reduce sheep losses. As testament to this, consider research done under Wildlife Services sponsorship and collaboration [6]. Furthermore, taking the above assertion at face value suggests the original journal article lacked very important information about SPM as a response variable. These two issues are elaborated in (a) and (b) respectively below.

a) Had we been peer reviewers of [1], the information above would have prompted a crop of new questions that were not answered in the original article. We would have asked them to clarify the identity of the implementers of SPM, when, where, and under what guidelines the SPM was conducted. Truthful answers to these questions would reveal whether the authors might have intentionally or unintentionally influenced SPM. As peer reviewers, we would be concerned that they unintentionally encouraged SPM at control pastures, which would exaggerate the effect of the treatment (aerial gunning). Indeed, it is a cardinal rule of rigorous scientific experiments that one should not manipulate one's response variable, but the authors appear to have been in a position of authority (see former quotation and the word "allow"), so the temptation might have been great for SPM implementers to please the research team leaders, in addition to other conflicts of interest that should have been explained thoroughly.
b) The authors' claim that SPM was a response to aerial gunning (the treatment) is disingenuous because SPM was first a response to lamb losses (the main response variable). We write, 'first', because We can understand the authors of The Wildlife Services ID EA arguing that SPM was secondarily a variable of interest (did aerial gunning reduce the need for subsequent SPM?). But that does not change the fact that SPM was an intervention that was implemented by Wildlife Services in response to lamb losses. Because SPM was under the influence of Wildlife Services, which collaborated and sponsored the research, SPM was a treatment as understood by serious researchers. Treating SPM as a response variable implies an unknown relationship to the treatment. That is disingenuous as the same agency

was involved in both interventions. The analogy would be a biomedical clinical trial to test if treatment X is a remedy for headaches and reduces patients' needs for aspirin. Only if the aspirin was wholly under patient control without influence from researchers would the clinical trial be able to claim that aspirin use was a response variable. But the Wildlife Services ID EA asks the reader to take it on faith that the implementation of SPM was 'blind' to the treatment. 'Blinding' is the term when subjects and research staff are unaware of whether a subject received treatment or control. We reread the Methods in [1] and we found no evidence that SPM staff were blinded to the treatments or otherwise insulated from the influence of the ongoing experiment.

> **"In the study, sheep were not permitted to *disperse evenly throughout* the grazing allotments, instead, herders move sheep bands through subsections of the allotments in accordance with *established grazing management plans*. Consequently, simply dividing the number of sheep on the allotment by the total size of the allotment, as was done, does not accurately reflect the density of sheep during the study."** (p. 451, emphasis added) [4].

No data on grazing plans were presented to substantiate this claim, in the original article [1] or [4]. The claim that sheep were using subsets of pastures reported in the paper is new information asserted without evidence because [1] presented areas and stock numbers. If they had more precise information on sheep numbers per unit area of pasture, or specifics of use of pastures, they should have presented those data rather than gross area.

Moreover, gross population density is routinely used in ecological studies, because excluding any lacunae of putatively unsuitable habitat would require additional evidence and analyses of suitability and use. Therefore, the quotation above makes an unfalsifiable claim (it cannot be proven or disproven) to rebut our critique. Even if grazing plans existed, the claim could not be falsified (did the sheep actually follow grazing plans?). Unfalsifiable claims are unscientific [https://plato.stanford.edu/entries/popper/, in 7].

> **"Failure to provide data showing that that number of treated allotments with LGDs matched the number of untreated allotments with LGDs does not constitute a reporting bias."** (p. 451) [4]

Returning to the analogy of the headache remedy above, imagine the drug company pairs placebo control and treated patients that use ibuprofen in one group and pairs control and treatment patients that use aspirin in another group. Here the pain reliever is the LGD by analogy because LGDs are a known intervention against predation (see [3] for review and [8] for confirmation). Without measuring the amount of ibuprofen and aspirin used (the number of LGDs), the researchers failed to measure an important confounding variable. Moreover, they must have matched untreated and treated pastures after the fact, so they would have known exactly how many LGDs were deployed in each pasture and how the effect of LGDs might confound their experiment, as the next paragraph reveals.

Aerial gunning occurred in the winter before sheep and LGDs were released on the summer pastures. The matching procedure deserves further scrutiny given it could only have occurred after the fact of treatment (above).

The original article states, “Each year, pastures with aerial hunting (treated) were paired with similar pastures (untreated) that had suitable terrain and sufficient losses to justify aerial hunting but did not receive treatment for logistical reasons (limited funds, availability of aircraft, conditions unsuited to aerial hunting).” (p. 607), [1].

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But further concerns arise when one reads, “Pairings were first based on similarities in habitat, the proportion of area suitable for aerial hunting, and the proportion of terrain and understory vegetation. We also made certain lambs in both pastures were of similar age, because the size and age of lambs can affect their vulnerability to predators. Lastly, we paired pastures based on the use or absence of livestock guarding dogs.” (p. 607), [1].

The authors do not explain how precisely they matched all of these attributes (especially the vague terms “habitat”, “suitable”, and “terrain”, which make the Methods irreproducible). We would have liked more description of lamb ages in in paired pastures. But most worrisome is the matching apparently failed to control the most important confounding variable (pre-treatment losses). To wit, the original article reveals that pre-treatment sheep losses were 186% higher in untreated than treated pastures (5.4 versus 2.9 losses on average, Table 2 in [1]). Despite the potential importance of past risk as a confounding variable, the authors made *no* effort to match for it, choosing instead to match treated and untreated pastures in a variety of other ways that are not clearly described.

Therefore, it appears the matching was post hoc, irreproducible, and seemingly strongly biased by past losses. The description of the matching procedure appears misleading because it omits the details of post hoc coordinating with shepherds for lamb aging and LGDs, and the convenience sampling by the authors’ team(s).

Summary

Finally, readers should consider the history of this article. It was first criticized on statistical grounds in 2004 [2], then on the grounds of research design in 2016 [3]. Instead of correcting the problems, retracting the article, or setting the article aside, the lead author of [1] chose to defend the biased and irreproducible methods in legal documents [4] . When challenged in court, the lead author did not correct or retract the statements [5]. In so doing, the lead author introduced new information and novel interpretations of their own Methods, which were opaque to peer reviewers at the time.

References

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