Received: 17 October 2019 | Revised: 28 October 2019 | Accepted: 30 October 2019

DOI: 10.1111/csp2.135

LETTERS AND COMMENTS

Conservation Science and Practice
A journal of the Society for Conservation Biology
Open Access
WILEY

Distortion of inferences and undue exaggeration of study limitations: Response to Shrotriya et al.

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Abstract

Our study titled "Land sharing potential of large carnivores in human-modified landscapes of western India" used an occupancy framework to provide baseline information on patterns and determinants of large carnivore presence in human-use landscapes. In their letter, Shrotriya et al. raise concerns about our survey design, distort our inferences, and present unwarranted exaggeration of our study limitations. Here, we provide detailed responses to key the issues raised by the authors while reiterating the accurate interpretations of our findings.

We thank Shrotriya et al. for their interest in our study. We agree with their concerns regarding potential misinterpretations of results published in scientific articles. We also freely admit that our conservation philosophy is in tandem to that espoused by the authors. In the spirit of furthering conservation policy rooted in rigorous science, we provide clarifications to the key issues raised by the authors.

In their critique of our survey methodology, Shrotriya et al. suggest that we should minimize the number of nonrespondents. Assuming that "nonrespondents" are people who denied responding, we clarify that all forest department personnel were available for the survey because interview sessions were organized with prior permissions. Contrary to what the authors claim about the average number of respondents (4.5 ± 3.4 *SD*), we have varying interview counts for leopard, wolf and hyena, depending on how many respondents correctly identified the species (5.6 ± 3.50 *SD*; 4.6 ± 3.43 *SD*;4.8 ± 3.14 *SD*, respectively). The authors also allege that our data are sparse, leading to boundary estimates (Welsh, Lindenmayer, & Donnelly, 2013). We note that Welsh et al. (2013) considered an extreme case of small sample sizes, and if adequate sample sizes (both no. of sites and no. of replicates) are maintained, boundary estimates likely represent true occupancy probabilities (Guillera-Arroita, Lahoz-Monfort, MacKenzie, Wintle, & McCarthy, 2014). The sample sizes used in our study are adequate for generating reliable inferences (MacKenzie et al., 2002). Recognizing that unequal sampling effort would influence detection probability (not occupancy probability, as the authors seem to think), we modeled survey effort as a covariate for the detectability parameter.

Shrotriya et al. deliberate on varying sizes of our sample units. Unequal-sized sample units are commonplace in distribution assessments (Madhusudan et al., 2015; Petracca et al., 2018) and we have explicitly described the issues with modeling unit size as an occupancy covariate. We agree that some units are smaller than wolf home-range size. The estimates for these sites may be interpreted as "probability of use" rather than "true occupancy probability" (MacKenzie, 2006); this would not alter the overall results. Given the size of our study landscape and the three focal species, the trade-off between sample unit size and logistical feasibility was inevitable. While the authors fail to clearly state what they mean by "80% of the study area that is human dominated is occupied by wolves," our estimates clearly say it is 64% of the entire study area which is almost entirely human-dominated. The authors suggest using percent land cover over land-cover area as occupancy covariates. This approach would have drawn false equivalences between, say, 10% of grassland cover in a 10 sq. km sample unit and 10% grassland cover in a 100 sq. km sample unit. This would confound the true effect of land cover area on carnivore occupancy. We reiterate that our metric for wild prey abundance is only an index. We assumed that prey species detections by respondents reflect the underlying patterns in presence. The use of "proportion of detections" to quantify a covariate of this kind is not unique to our study (Karanth et al., 2011).

Shrotriya et al. seem to suggest that we attribute human-carnivore coexistence solely to human tolerance and carnivores' resilience, and under estimate habitat modification. Habitat covariates form a major component of our analysis and are discussed extensively. For instance, our results clearly show that urbanization has negatively affected occupancy/use, which the authors seem to have missed altogether. However, people's acceptance of carnivores is undeniably an important contributing factor toward carnivore persistence, often unacknowledged in distribution studies. Habitat refuges or complexity alone cannot allow carnivores to persist in human-use landscapes. For example, Linnell, Swenson, and Andersen (2001), Chapron et al. (2014), and Carter and Linnell (2016) illustrate how large carnivore survival in multiuse human-dominated habitats of Europe is heavily reliant on hunting policies. We also clearly mention that conversion of seasonal agriculture to permanently irrigated agriculture could favor leopards over wolves. We discuss how carnivores are vulnerable to local extinctions given that alleged "wastelands" are diverted for commercial use and urbanization can alter patterns of occupancy. Agricultural landscapes provide connectivity, host domestic prey, and support low human densities, all of which may facilitate carnivore persistence. Our study shows that there are certain agricultural habitats associated with these large carnivores, but we do not imply that natural habitats should be converted into agriculture, or that such conversions would not affect large carnivores.

We openly acknowledge that not including indicators of social acceptance is a caveat of our study. Assessing acceptance and converting it into metrics for quantitative analyses is an entirely separate enquiry involving a different academic discipline. Cursory metrics (like rapid interview surveys) can yield misleading results. We believe the authors have misinterpreted our mention of "modification of human behaviour" with placatory interventions like short-term awareness programs. We would like to point out that monetary compensation, the most common method to redress wildlife-related losses, is also a state instrument aimed at modifying human behavior. Several studies (Balmford & Cowling, 2006; Mascia et al., 2003; Schultz, 2011) highlight how conservation successes are more dependent on fundamental human behavior, and less so on top-down policy instruments and esoteric scientific research. Space-sharing between humans and carnivores cannot be an exception, even if it means that carnivores would be increasingly dependent on livestock.

We completely agree with Shrotriya et al. that flawed methodologies and superficial inferences may cause more damage than benefit to conservation. Our study demonstrates the application of well-established field and analytical methods to assess carnivore distributions in heterogenous landscapes, and we have strived to account for possible biases arising due to our choice of species, sampling units, and target respondents. Nowhere do we stretch our inferences beyond reason to "mislead" conservation planning. In fact, our study calls for India's conservation narrative to be more inclusive with respect to carnivores outside protected areas by highlighting their presence in neglected habitats. We certainly welcome more investigations at finer spatial scales that evaluate the roles of habitat refuges, social acceptance of people, and thresholds of carnivore resilience, that could complement our findings.

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How to cite this article: Majgaonkar I, Vaidyanathan S, Srivathsa A, Shivakumar S, Limaye S, Athreya V. Distortion of inferences and undue exaggeration of study limitations: Response to Shrotriya et al.. *Conservation Science and Practice*. 2019;1:e135. https://doi.org/10.1111/csp2.135