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Large carnivore science: non-experimental studies are useful, but experiments are better

Benjamin L. Allen[a,*], Lee R. Allen[b], Henrik Andrén[c], Guy Ballard[d], Luigi Boitani[e], Richard M. Engeman[f], Peter J.S. Fleming[g], Adam T. Ford[h], Peter M. Haswell[i], Rafał Kowalczyk[j], John D.C. Linnell[k], L. David Mech[l], Daniel M. Parker[m]

[a] University of Southern Queensland, Institute for Agriculture and the Environment, Toowoomba, Queensland 4350, Australia
[b] Robert Wicks Pest Animal Research Centre, Biosecurity Queensland, Queensland Department of Agriculture and Fisheries, Toowoomba, Queensland 4350, Australia
[c] Grimsö Wildlife Research Station, Department of Ecology, Swedish University of Agricultural Sciences (SLU), SE-73091 Riddarhyttan, Sweden
[d] Vertebrate Pest Research Unit, New South Wales Department of Primary Industries, The University of New England, Armidale, New South Wales 2351, Australia
[e] Department of Biology and Biotechnologies, University of Rome, Sapienza, 00185 Rome, Italy
[f] National Wildlife Research Centre, US Department of Agriculture, Fort Collins, CO 80521-2154, United States
[g] Vertebrate Pest Research Unit, New South Wales Department of Primary Industries, Orange, New South Wales 2800, Australia
[h] Department of Biology, University of British Columbia, Kelowna, Canada V1V 1V7
[i] School of Biological Sciences, Bangor University, Bangor, Gwynedd, LL57 2DG, United Kingdom
[j] Mammal Research Institute, Polish Academy of Sciences, 17-230 Białowieża, Poland
[k] Norwegian Institute for Nature Research, PO Box 5685 Torgard, NO-7485 Trondheim, Norway
[l] Northern Prairie Wildlife Research Center, United States Geological Survey, 8711 -37th Street, SE, Jamestown, ND 58401-7317, USA
[m] School of Biology and Environmental Sciences, University of Mpumalanga, Nelspruit 1200, South Africa

1. Response to Bruskotter and colleagues

We recently described the following six interrelated issues that justify questioning some of the discourse about the reliability of the literature on the ecological roles of large carnivores (Allen et al., in press):

1. The overall paucity of available data,
2. The reliability of carnivore population sampling techniques,
3. The general disregard for alternative hypotheses to top-down forcing,
4. The lack of applied science studies,
5. The frequent use of logical fallacies,
6. The generalisation of results from relatively pristine systems to those substantially altered by humans.

We thank Bruskotter et al. (2017) for responding to our concerns and engaging with this important issue. We agree completely that non-experimental studies can and do often have great value, and we recognise that in many (most) cases these types of studies may provide the only data that are available. We acknowledge the many challenges of working on large, cryptic, dangerous, and highly-mobile animals in the wild. However, the absence of more robust data and the reality of these challenges do not excuse weak inference or overstating conclusions – a practice apparent in many studies (and communication of those studies) adopting only observational or correlative methods to infer the roles of large carnivores (reviewed in Allen et al., in press).

We advocated in our original article, agree with Bruskotter and colleagues, and reaffirm here, that bringing together studies based on multiple different methods is a powerful way to improve the quality of large carnivore science. But we reaffirm that not all studies are of equal value. Manipulative experiments have far greater inferential power than observational and correlative studies, which should accordingly be valued as ‘weaker’ than manipulative experiments (e.g. Li, 1957; Krebs, 1999; Hone, 2007; Fleming et al., 2013). The need for such experiments may not be as strong where animal numbers are small and more easily observed, study area sizes are small, climates are stable, harvest does not occur, livestock are not present, land use changes are negligible, and past or present human effects are non-existent. In such cases, knowledge obtained from non-experimental studies can be informative. But where these and many other influential factors are present, manipulative experiments can be the only way to tease out the relative effects of all the potential causal factors that may explain our observations. We of course agree with Bruskotter and colleagues that the best situation is when multiple strands of evidence are considered (see also Ford and Goheen, 2015), and we freely recognise that wildlife

* Corresponding author.
E-mail addresses: benjamin.allen@usq.edu.au (B.L. Allen), lee.allen@daf.qld.gov.au (L.R. Allen), Henrik.Andren@slu.se (H. Andrén), guy.ballard@dpi.nsw.gov.au (G. Ballard), luigi.boitani@uniroma1.it (L. Boitani), Richard.M.Engeman@aphis.usda.gov (R.M. Engeman), peter.fleming@dpi.nsw.gov.au (P.J.S. Fleming), adam.ford@ubc.ca (A.T. Ford), p.m.haswell@bangor.ac.uk (P.M. Haswell), rkowal@ibs.bialowieza.pl (R. Kowalczyk), John.Linnell@nina.no (J.D.C. Linnell), david_mech@usgs.gov (L.D. Mech), Daniel.Parker@ump.ac.za (D.M. Parker).

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management decision-making should be informed by more than just scientific knowledge. The challenge lies in the integration of the multiple sources of information, the appropriate weighting or value attached to each, and the way they are used to inform carnivore conservation and management attitudes, policy and practice.

The Behaviourally Mediated Trophic Cascade Hypothesis (BMTCH), the Mesopredator Release Hypothesis (MRH), and the Trophic Cascade Hypothesis (TCH) have seen much public and scientific interest. But reports claiming strong carnivore effects (e.g. Letnic et al., 2017; Newsome et al., 2017) and weak or attenuated carnivore effects (e.g. Pasanen-Mortensen et al., 2017; Rich et al., 2017) both continue to regularly appear in the literature. Calls for these hypotheses to be considered universal and/or important phenomena (e.g. Estes et al., 2011) now appear premature and unsupported (Peterson et al., 2014; but see also Cooke and Soriguer, 2017; Haswell et al., 2017; Morgan et al., 2017). Nevertheless, many people have come to believe that evidence for these ideas is strong, so we fully expect some disagreement with these conclusions. We agree with Bruskotter et al. (2017) that it is not 'equivocal' that predation can have an impact on herbivore abundance, and that over-abundant herbivore populations can have adverse impacts on habitats. What *is* equivocal (see Mech, 2012; Allen et al., in press) is that (1) these simple predator-prey relationships inevitably produce important cascading consequences for entire food webs, (2) these effects are always strong (or one of the strongest) drivers of ecosystem structure, (3) any addition or removal of large carnivores will necessarily have important cascading consequences for ecosystem functions, and (4) large carnivores must be present and abundant for any ecosystem to be considered healthy or resilient. Moreover, the considerable value of large carnivores need not be linked to the demonstration of these things.

Our intention is to increase the degree of reflection among researchers and wildlife managers about the strength and utility of the available evidence for these effects when they seek to bridge the science-policy-practice interface in this explicitly value-laden field of conservation biology. We argue that there is a need for the scientific community to be much more humble and honest about the strength of our inferences and the certainty of our knowledge concerning complex ecological issues. Large carnivore conservation and management efforts are most likely to be successful when scientific evidence is clear, strong, and used in conjunction with other sources of information to support social, economic, and political change.

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