Review

Stuck in the mud: Persistent failure of 'the science' to provide reliable information on the ecological roles of Australian dingoes

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ARTICLE INFO	ABSTRACT
	Apex predators are believed to play important roles in maintaining the structure and function of ecological systems, but actual evidence for mesopredator releases and trophic cascades in terrestrial systems is mixed and equivocal, largely due to the systemic and continued use of weak-inference or correlative study designs to investigate these hypothesised causal processes. Here we critically review the experimental designs of empirical studies examining relationships between dingoes and mesopredators in Australian ecosystems. We found that 83 % (30 out of 36) of recent study designs lacked one or more of the essential experimental design elements needed to assess causal relationships (such as experimental treatments and controls, treatment replication, and/or treatment randomisation), demonstrating that the inferential strength or reliability of 'the science' on this subject remains weak and equivocal. Only five studies published in the last decade ($N = 36$), and eight in total since 1993 (or 11 %, $N = 76$), were capable of assessing dingoes' potential causal roles in mesopredator release; and all eight studies consistently demonstrated that dingoes do not supress mesopredators or initiate trophic cascades through mesopredator release effects at a population level, independent of ecological context. Thus, there is a demonstrable absence of evidence and evidence of absence for dingo suppression of mesopredators in Australia. We encourage large carnivore conservation managers and policymakers to base their decisions on the strongest available science. In this way, researchers and managers will have the best chance of conserving these important and valuable species into the future.

1. Introduction

Terrestrial apex predators are considered to be causal agents of ecosystem structure and function, and a great variety of studies support the general view that conservation and enhancement of their populations produces positive ecological effects (Barbosa and Castellanos, 2005; Ray et al., 2005; Molles, 2012). One of the primary mechanisms thought to produce these effects is their suppression of smaller mesopredators which, in turn, frees populations of smaller prey from mesopredator suppression (Ritchie and Johnson, 2009; Terborgh and Estes, 2010). Conversely, removal or reduction of apex predators is thought to generate a release of mesopredators, which then suppress prey at lower trophic levels. Such trophic cascades have been reported for several large predators (Ripple and Beschta, 2012; Estes et al., 2013) which has led some to the conclusion that apex predator suppression of mesopredators is a strong and ubiquitous ecological force operating across a

variety of ecosystems and species assemblages (Estes et al., 2011; Newsome et al., 2017a). There was a time when such conclusions might have seemed tenable given the surge of concurring publications in the early years that followed the seminal study by Crooks and Soulé (1999), but a large and growing body of evidence has since revealed that apex predators' roles in possible mesopredator release or trophic cascade effects are not as widespread as they were once thought to be (e.g. Sergio et al., 2008; Haswell et al., 2017; Wirsing et al., 2021), are not as strong or important as other forces (e.g. Elmhagen and Rushton, 2007; White, 2013; Kuijper et al., 2016; Morgan et al., 2017; Gigliotti et al., 2020), or might not occur at all in many cases (e.g. Balme et al., 2017; Miller et al., 2018; Comley et al., 2020; Castle et al., 2022; Müller et al., 2022; Ferretti et al., 2023). Even some of the best examples of apex predators' roles in trophic cascades are exaggerated or unclear (MacNulty et al., 2016; Allen et al., 2017; Brice et al., 2022). Periodic syntheses of the literature are required to benchmark the progress of our collective

understanding about these important processes and to assist managers with making evidence-based decisions about the conservation of these charismatic and valuable species.

Evidence for mesopredator suppressive effects is absent for most large predators, but Australian dingoes (*Canis familiaris*) are one of the two large predator species (the other is grey wolves; *Canis lupus*) where evidence for these effects is thought to be strongest (Ripple et al., 2014). Indeed, some have asserted that dingoes exhibit astonishing powers which include the provision of continental-scale refuges from feral cat *Felis catus* (Brook et al., 2012) and European red fox *Vulpes vulpes* predation (Colman et al., 2015), changing soil chemistry by elevating soil nutrients (Morris and Letnic, 2017), preventing shrub encroachment (Gordon et al., 2017a), preserving avian predators (Rees et al., 2019a), shifting entire epigeic arthropod assemblages (Contos and Letnic, 2019), or even altering geological processes (Lyons et al., 2018). Wallach et al. (2010) further asserted that dingoes had greater effects on arid ecosystems than rainfall, and Hunter and Letnic (2022) concluded that dingoes influence mesopredators to a greater degree than the thousands of tonnes of poison baits distributed annually to control them.

The relatively large number of studies adopting these views have led to impassioned advocacy for swift policy and practice changes that better align with 'the science' (e.g. Letnic, 2014; Wallach, 2014; Kennedy, 2016). For example, the Victorian State Government recently proposed the reintroduction of dingoes to a national park for the express purpose of supressing mesopredators and thereby saving extant threatened fauna from mesopredator predation (Eishold, 2022). On at least four occasions others petitioned the Australian Government to list 'the cascading effects of the loss or removal of dingoes from Australian landscapes' as a Key Threatening Process in the *Environment Protection and Biodiversity Conservation Act 1999* (EPBC Act) on grounds that dingo control (for livestock protection purposes) indirectly compromises the conservation of threatened fauna through trophic cascades effects (DEE, 2018). On at least three more occasions international animal rights organisations have led further efforts to list dingoes as a 'threatened species' under the EPBC Act (Kennedy, 2016). In each of these cases, however, such advocacy is getting no policy traction and each of these proposals have ultimately been rejected. Why?

Despite widespread embracing of the aforementioned conclusions by many academics, researchers and lay people alike, the actual data and evidence justifying these views are very weak. In a systematic suite of critical reviews on the subject, Allen (2011) first reported that alternative and more parsimonious interpretations of the available information were possible, and that the predator sampling methods used in most studies were frequently confounded with seasonal effects, habitat effects, and unsupported assumptions (Allen et al., 2011a). Allen et al. (2013b) then showed that the experimental designs of these studies lacked the necessary features that would enable them to make causal inferences about dingoes' ecological roles, regardless of shortcomings in the predator sampling methods. Others objected (e.g. Letnic et al., 2011a; Glen, 2012; Johnson and Ritchie, 2013) and pointed to three 'exceptions to the rule' where they asserted that mesopredator release effects still seemed apparent (i.e. Lundie-Jenkins et al., 1993; Pettigrew, 1993; Burrows et al., 2003). However, subsequent reanalyses of these 'exceptions' by their original authors likewise demonstrated an absence of reliable evidence for mesopredator release effects from these studies too (Allen et al., 2014b). Ignoring methodological issues altogether, close examination of the stated results in the available literature still revealed a widespread lack of supporting evidence for dingo suppression of mesopredators (Allen et al., 2015). A chronological summary of the preceding exchanges is provided (for dingoes, and other large predators in Africa and North America) in Allen et al. (2017).

At this point "there [was] not sufficient evidence" for the mesopredator suppressive effects of dingoes to warrant policy change (DEE, 2018), despite the continued fomentation of a 'dingo debate' which attempted to challenge or undermine this view (Smith, 2015). This clear lack of evidence may have justified earlier decisions to reject proposed changes to dingo management policy, but the volume of literature on the subject has almost doubled since that time (see below), prompting the need for further collation and critical re-evaluation of the literature. Such an evaluation is not only necessary for those concerned about Australian dingoes, but also for those more broadly concerned about the ecological effects of apex predators given dingoes are supposed to be one of the two best examples we have, globally, for the mesopredator suppressive effects of terrestrial apex predators (Ripple et al., 2014).

Here we review the methods and experimental designs underpinning the recent literature on dingo-mesopredator relationships to determine (1) if the inferential strength or reliability of the literature is capable of assessing causality and/or is improving over time, (2) if the spatial and temporal scales of studies, and their resulting sample sizes or quantum of data, permit meaningful conclusions about predator relationships, and (3) whether or not institutional prestige or journal impact factor is a reliable guide for identifying studies with the greatest inferential strength. We further summarise the status of knowledge on dingo-mesopredator relationships from the most inferentially informative studies and offer advice to researchers and managers about necessary improvements to and use of the literature. Our overall goal is to improve the rigour of large carnivore science and benchmark a knowledge base about apex predator effects on mesopredators from which future studies can build upon.

2. Methods

2.1. Scope

We focussed our review on studies of dingo-mesopredator relationships published between 2012 and 2022. Relevant studies conducted prior to 2012 (*N* = 40) were reviewed by Allen et al. (2013b), who showed that 15 (38 %) were potentially weakened by habitat confounding, 16 (40 %) were potentially weakened by seasonal confounding, and 12 (30 %) made unsupported and/or invalid assumptions, with a total of 19 (48 %) studies exhibiting at least one of these methodological flaws. Only four of the studies (10 %) they assessed were true experiments potentially capable of demonstrating cause and effect, and most of the remainder were either quasi- or pseudo-experimental correlative studies. They concluded that the "limitations inherent to most studies do not permit reliable or conclusive evidence for dingoes' ecological roles", further warning that "so long as the production of this type of research continues, our knowledge of dingoes' ecological roles will be no further advanced in the future" (Allen et al., 2013b, pg. 171). Our follow-up review therefore assesses whether or not this type of research has continued in the decade since these warnings.

In their assessment, Allen et al. (2013b) only considered empirical studies that used passive tracking indices (i.e. sand plots) because they were by far the most common predator sampling techniques used to investigate dingo-mesopredator relationships up until that time. But we consider studies that used spotlighting, camera traps, and passive tracking indices to sample predator populations because the use of these other techniques have increased in the last decade (see below). We also considered a small number of studies that monitored predator interactions using GPS collars. At least one of these techniques were used by each of the studies we reviewed. Our review therefore expands the scope of the earlier work described by Allen et al. (2013b) to include studies derived from a wider variety of predator sampling approaches conducted over a longer period of time. Aligning or joining our review to this earlier one required slight adjustment to our reported sample sizes (numbers of studies) to avoid re-reviewing or double-counting studies already reviewed (e.g. Moseby et al., 2012), or to accommodate the conversion of grey literature reports completed and reviewed prior to 2012 later being published after 2012 (e.g. compare Allen, 2005 or Eldridge et al., 2002 with Allen et al., 2013a; see also Edwards et al., 2021).

2.2. Literature search strategy

We sourced relevant literature through four international databases (CSIRO Publishing, Web of Science, EBSCOhost Megafile Ultimate, and Science Direct) and searched these databases using the terms: dingo OR wild dog AND fox OR cat AND mesopredator. CSIRO Publishing yielded 946 articles published in natural environment journals, Web of Science yielded 1109 articles, EBSCOhost Megafile Ultimate yielded 162 articles published in biological and physical science journals, and Science Direct yielded 784 articles. We removed duplicates from the resulting 3001 articles, and then read the abstracts of the remaining articles to determine whether or not they aligned with the scope of our review. Although somewhat relevant to our review, we excluded studies that focussed on predator-habitat relationships (e.g. Arthur et al., 2013), predator-prey relationships (e.g. Contos and Letnic, 2019), or predator diet studies (e.g. McDonald et al., 2018) because they did not report empirical data on dingo-mesopredator relationships (the core mechanism underpinning claims of dingoes roles in trophic cascades). If insufficient content was reported in the abstract, the methods and results sections of each report was read to investigate further. Ultimately, a total of 36 articles examining dingo-mesopredator interactions and published between 2012 and 2022 were identified for review. For some analyses these were combined with the 40 pre-2012 studies reviewed by Allen et al. (2013b) and two additional studies from 2011 that had not been evaluated by Allen and colleagues but were within the scope of the present study ($N = 76$ in total).

2.3. Information extraction

There are a variety of ways to assess the causal strength of a study (Hill, 1965; Holland, 1986; Kimmel et al., 2021; Hone and Krebs, In press), so for consistency, we chose the relatively straightforward approach previously applied by Allen et al. (2013b) to the dingo-mesopredator literature. This approach focusses on the nature of observational, correlative and experimental studies (i.e. 'zero level' and 'first level' studies in Hone and Krebs, In press), which typify the relevant literature. A deeper or more comprehensive investigation into the overall strength of supporting evidence for dingo-mesopredator relationships would include additional assessment of 'second level' and 'third level' studies; such an investigation would require systematic assessment of many other sources of information, such as predator diet studies or studies that attempt to identify and validate unique mechanisms for dingo suppression of mesopredators (i.e. predation, exploitative competition etc.), which was outside the scope of our review.

For each of the studies within our scope we carefully read them to extract information on their experimental design features as a way of ascertaining the quality or strength of the available data contained within them. Each study was then given a 'H score' of 1–16 representing the inferential capacity or causal strength of their experimental designs, as defined in Hone (2007; Table 1), where a H score of 1 represents a very strong experimental design, and a H score of 16 represents a very weak experimental design. According to Hone (2007), the three essential elements of the strongest studies (i.e. classical experiments) are the presence of both treatments and experimental controls, treatment randomisation, and treatment replication. The threshold that separates true experimental studies (i.e. those capable of answering cause and effect questions) from correlative or observational studies (i.e. those incapable of answering cause and effect questions) is a H score of 4 or less (see Table 1); studies with H scores of 5 or more lack at least one of these essential elements and are therefore capable of yielding only observational or correlative data which is inferior to, or less reliable than, those data obtained from stronger experimental designs (Hone, 2007; but see also Johnson, 2002; Shadish et al., 2002; Sutherland et al., 2013; Williams and Brown, 2019; Kimmel et al., 2021). In other words, studies based on designs with an H score of 4 or less estimate measured effects of causes in experiments, in contrast to studies based on designs with an H score of 5 or more which merely infer the causes of effects from observations (Holland, 1986). Where the experimental designs were not clearly stated in the original reports or were designed to answer questions unrelated to dingo-mesopredator relationships, we assigned H scores based on the strongest experimental designs we thought possible given the information that could be extracted from the reports.

We further interrogated each article to extract information on the stated spatial and temporal scales assessed in each study, and their reported predator sampling methods and effort. This was done to assess (1) when and for how long each study was conducted, (2) the spatial scale the study was conducted over, (3) if and/or how frequently predators were repeatedly sampled, (4) which techniques were used to sample predators and how these techniques were applied, and (5) the resulting quantum of data collected in each study, or their available sample sizes (e.g. number of sand plot nights or camera trap nights of data etc.). Sample sizes were not clearly reported or reported at all in many cases. Where these were not reported, we estimated approximate, minimum or maximum sample sizes based on the stated descriptions of the methods. The purpose of assessing this information was to quantify the volume of data present in each study, regardless of their experimental design strengths. We summarised the strengths and weaknesses of each study and identified any other methodological issues that may affect the reliability of the reported data, such as the presence of seasonal confounding, habitat confounding, or unmeasured treatment

Table 1

Classification of experimental design strengths, adapted from Table 1.2 in Hone (2007). Experimental design strengths (H scores) range from 1 (strongest inference, 'classical experiment') to 16 (weakest inference, 'simple observations'). The threshold of evidence required to demonstrate causal effects is a H score of 4, where H scores ≤4 provides evidence of causal effects, and H scores of ≥5 are correlative and cannot provide evidence of causal effects.

H score	Classification	Experimental control used	Treatment randomisation used	Treatment replication used	Analysis conducted
1	Classical experiment	✓	✓	✓	✓
2	Data set awaiting analysis	✓	✓	✓	
3	Un-replicated experiment	✓	✓		✓
4	Un-replicated, unanalysed experiment	✓	✓		
5	Quasi-experiment type l	✓		✓	✓
6	Quasi-experiment type ll	✓		✓	
7	Quasi-experiment type lll	✓			✓
8	Quasi-experiment type lV	✓			
9	Pseudo-experiment type l		✓	✓	✓
10	Pseudo-experiment type ll		✓	✓	
11	Pseudo-experiment type lll		✓		✓
12	Pseudo-experiment type lV		✓		
13	Pseudo-experiment type V			✓	✓
14	Pseudo-experiment type Vl			✓	
15	Pseudo-experiment type Vll				✓
16	Simple observations				

effects, as described earlier by Allen et al. (2011a, 2013b).

To evaluate whether or not the prestige of the research institution was an indicator of the best or strongest studies, we recorded the lead university of each study by identifying the first Australian university reported in the list of author affiliations, except for one study which was not associated with any university (Forsyth et al., 2019). We then used the 2022 university rankings in 'environmental science' from the Scopus SCImago Institutions Rankings database (www.scimagojr.com) to rank each university's prestige in this discipline. The most recent journal impact factor (JIF) for each article was also obtained from SCImago to evaluate whether or not the best or strongest studies were being published in the journals with the highest JIFs. In short, we sought to assess whether or not university or journal prestige were good indicators of study design strength (H scores).

Regression analyses were performed in R (R Core Team, 2021) to assess trends in the inferential strength of studies (H scores) over time, the relationship between the causal strength of a study (H scores) and the university's prestige or rank in environmental science, and the relationship between the causal strength of studies (H scores) and the impact factor of the journals (JIFs) they were published in.

3. Results

Assessment of trends in the causal strength of experimental designs used in studies of dingo-mesopredator relationships indicated that the vast majority (31 of 36, 86 %) of studies conducted over the last decade continued to use primarily correlative or observational study designs (H scores ≥5), and therefore possessed no capacity to assess causal relationships between dingoes and mesopredators (Table 2, Fig. 1). No classical experiments were performed, but there were five unreplicated experiments with H scores of 3 that did meet the threshold for enabling causal inferences about dingo-mesopredator relationships (Allen et al., 2013a; Allen et al., 2014a; Moseby et al., 2019; Castle et al., 2021; Castle et al., 2022). Ignoring experimental design limitations, a total of 23 of 36 studies (64 %) expressed support for the view that dingoes caused mesopredator release or suppression, yet 22 of these 23 (96 %) studies were based on low-inference study designs (H scores ≥5) void of the capacity to make such inferences about such causal processes (Table 2). Allen et al. (2013b) reported that only four studies available prior to 2012 had met the threshold for causal inference (Allen et al., 1998; Eldridge et al., 2002; Allen, 2005; Allen, 2012), although data from some of these unpublished sources have since been combined and can now be found in three published reports (described above), meaning that only eight studies published since 1993 reached the required threshold to assess causal relationships between dingoes and mesopredators (Fig. 1; see also Table 2 and Allen et al., 2013b) and only one published study met the criteria for a classical experimental design (i.e. Allen et al., 2013a). There was, therefore, no improvement in the causal strength of experimental designs over time ($r = 0.13$, df 75, $p = 0.27$).

Most of the studies we assessed collected their data over short time scales. Indeed, 10 out of 36 (28 %) studies were conducted within one year or less, 20 out of 36 (56 %) studies were conducted in two years or less, and 26 out of 36 (72 %) studies were conducted in three years or less (Table 2, Fig. 2). All of the studies that were completed in under three years used correlative or observational study designs. The duration of each of the five studies that met the threshold for causal inference was 5 years (Table 2). Studies with data collection periods exceeding seven years had higher H scores or weaker experimental designs (Table 2). Moreover, some longer-term studies were not based on time series data resulting from repeated sampling at a given study site(s) during the study period, but were instead the result of pooling data from two or more disjunct studies conducted at different places at different times before re-analysis, giving the misleading appearance of a 'long term study' (e.g. Colman et al., 2015).

The amount of sampling effort and quantity of data reported in each study was highly variable (Fig. 3). Fifteen (of the 36, or 42 % of) studies

used passive tracking indices. Of these, only four reported acquiring over 6000 sand plot-nights of data (Allen et al., 2013a; Allen et al., 2014a; Castle et al., 2021; Castle et al., 2022), and only 3 of the 15 camera trap studies reported acquiring >14,000 camera trap-nights of data (Stobo-Wilson et al., 2020a; Stobo-Wilson et al., 2020b; Kreplins et al., 2021). All five of the studies that met the threshold for causal inference were studies that used passive tracking indices, one of which also used spotlighting data for density estimation (Castle et al., 2022). Most studies reported relatively small sample sizes or contained relatively small volumes of data (Fig. 3), indicative of snap-shot correlative or observational studies done over short time frames (Table 2). Six studies reported exceptionally large volumes of data (Fig. 3), though only two of these also used strong experimental designs with H scores ≤4 (Allen et al., 2013a; Allen et al., 2014a).

We found no relationship between the causal strength of a study (H score) and the university's prestige or rank in environmental science ($r = 0.14$, df 35, $p = 0.41$; Fig. 4). The University of Queensland (UQ) was ranked 1st in Australia for environmental science, and the University of Western Sydney was ranked the lowest at 35th. Higher-ranking universities published both weak and strong experimental studies, just as lower-ranking universities published both weak and strong experimental studies (Fig. 4). Thus, a university's rank in environmental science did not reflect the causal strength or reliability of the studies they produced on dingo-mesopredator relationships.

We also found no relationship between the causal strength of studies (H scores) and the impact factor of the journal (JIF) they were published in ($r = 0.10$, df 35, $p = 0.58$; Fig. 5). For example, Letnic et al. (2011b) published their work on dingo-mesopredator relationships in the journal with the highest impact factor (JIF = 7.1, *Global Ecology and Biogeography*) despite being a weak correlative study (H score = 9) conducted in <1 year, whereas McHugh et al. (2020) published their work in the journal with the lowest impact factor (JIF = 2.2, *Ecological Management & Restoration*) despite being a stronger study (H score = 5) conducted over two years (Table 2). Both higher-ranked and lower-ranked journals published studies with both weak and strong experimental designs. The strongest experimental studies were published in journals with JIFs between 2.6 and 4.4 (Table 2, Fig. 5). Thus, a journal's impact factor did not reflect the strength or reliability of the studies they produced on dingo-mesopredator relationships.

Twenty of 36 (56 %) studies were potentially weakened by seasonal or habitat confounding (Table 2). Eleven of 36 (31 %) studies did not measure their control (e.g. did not measure whether or not dingo removal even removed any dingoes). Four of 36 (11 %) studies used less-informative binary or categorical measures to assess predator abundances over more-informative continuous measures, and four of 36 (11 %) studies made invalid or unsupported assumptions (e.g. compared or pooled population index values between species). At least 15 of 36 (42 %) studies reported one or more of these methodological issues (Table 2).

4. Discussion

The ability of apex predators to supress mesopredators is thought to be an important mechanism shaping the structure and function of terrestrial ecosystems (Barbosa and Castellanos, 2005; Ray et al., 2005; Molles, 2012). But supporting data for these mechanisms is deficient for almost all predator species (Ford and Goheen, 2015; Allen et al., 2017; Brice et al., 2022) and Australian dingoes are reported to be one of the only two species for which sufficient supporting data is available (Ripple et al., 2014). Critical reviews completed a decade ago showed that there was no reliable evidence to support the notion that dingoes suppressed mesopredators because the methodologies underpinning the available studies did not permit such strong inference (Allen et al., 2013b), but the amount of literature on the subject has doubled since this time (Fig. 1), warranting further review. Unfortunately, our critical review of the same type of literature published since 2012 found that the vast majority

Table 2

An overview of the experimental design strengths and weaknesses, predator sampling methods, and other characteristics of 36 empirical studies of dingoes-mesopredator relationships published between 2012 and 2022, listed alphabetically. See Allen et al. (2013b) for review of studies conducted prior to 2012, and Allen et al. (2011a) for more detailed explanation of the methodological weaknesses described below. Where applicable, the second H score shown in parentheses represents an alternative H score that might be possible if the reported data could be validly reorganised and reanalysed using a different and stronger experimental design. The 'MRH support' column indicates whether or not the study authors claim their results support (S) or do not support (NS) the Mesopredator Release Hypothesis (MRH).

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
1	Allen et al., 2013a Sand plots, poison baiting	Relationships between predators exposed to lethal control	• Manipulative experiment (baiting) • Large spatial scale • Treatments and controls • Measured control • Random allocation of treatments • Time series data • BACI design	• Non-independence of treatments at 3 of 9 study sites • Non-independence of treatments over time at all sites (predator migration was possible) • Treatment size, intensity and duration varied between sites • Only one survey conducted before commencement of treatments at some sites • Replication used (but not analysed)	• 9 large study sites 45,600 km^2 • 2–5 year study period at each site • Each site comprised of paired treated (baited) and non-treated (unbaited) treatment areas • 92–166 road-based sand plots spaced 1 km apart per study site • 6–23 repeated surveys every few months per study site • Predator activity calculated as the mean number of tracks per plot per night (mean of daily means) • 35,399 sand plot-nights of data	Unreplicated experiment (although could be a classical experiment if re-analysed) 3(1)	NS	3.17	1
2	Allen et al., 2014a Sand plots, poison baiting	Responses of predators and prey to predator control	• Manipulative experiment (baiting) • Large spatial scale • Treatments and controls • Measured control • Random allocation of treatments • Time series data • BACI design	• Non-independence of treatments at 3 of 9 study sites • Non-independence of treatments over time at all sites (predator migration was possible) • Treatment size, intensity and duration varied between sites • Only one survey conducted before commencement of treatments at some sites • Replication used (but not analysed)	• 9 large study sites • 2–5 year study period at each site • Each site comprised of paired treated (baited) and non-treated (unbaited) treatment areas • 92–166 road-based sand plots spaced 1 km apart per study site • 6–23 repeated surveys every few months per study site • Predator activity calculated as the mean number of tracks per plot per night (mean of daily means) • 35,399 sand plot-nights of data	Unreplicated experiment (although could be re-analysed as a classical experiment) 3(1)	NS	3.17	1
3	Bird et al., 2018 Sand plots, fire	Species' association with fire history	• Snapshot study • Stratified random sampling	• No treatments or controls, randomisation or replication • Used a categorical measure of predator activity over continuous measures • Used a categorical measure of fire/ habitat over continuous measures	• 1 large study site • 2 year study period • 76 × 1 ha sand plots spaced at least 1 km apart, within 2 km of a vehicle track, on sandy soils only, and sampled only in the winter months sometime between July 2014 and July 2016 • Predator activity categorised as absent, rare, moderate, or common based on the 'freshness' and number of observed tracks • 1 single survey at	Pseudo-experiment type VII 15	S	5.99	33

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
					each study site • 76 sand plot-nights of data				
4	Brook et al., 2012 Camera traps, poison baiting	Relationship between dingoes and feral cats	• Large spatial scale • Spatial replication	• Unmeasured control • Non-independence of treatments at some sites • Non-random allocation of treatments • Treatment size, intensity and duration varied between sites • Results confounded by historical and current land use, camera type, sampling effort and schedule, survey duration, season, and lure combinations	• 9 large study sites • 3 year study period • 20–40 baited camera traps per study site, deployed in pairs (1 on-road and 1 off-road) and spaced at 2–5 km intervals for a single 5–8 day survey period at each site • 6 of 9 study sites surveyed once only between March and November • 3 of 9 study sites surveyed three times over several months (but the data were pooled) • 5308 camera trap-nights of data	Quasi-experiment type I 5	S	6.53	14
5	Castle et al., 2021 Sand plots, poison baiting	Responses of predators to dingo removal	• Manipulative experiment (dingo eradication) • Large spatial scale • Treatments and controls • Measured control • Random allocation of treatments • Treatment independence • Time series data	• Treatment efficacy varied between study sites • Replication used (but not analysed)	• 2 large study sites • 5 year study period • Each site comprised of paired 'inside fence' and 'outside fence' areas • 94–122 road-based sand plots spaced 1 km apart per study site • 14 repeated surveys every 4 months per study site • Six x 10 km spotlight surveys inside and outside of each cluster repeated 3 times per year is 720 km × 5 years = 3600 km of vehicle tracks spotlighted • Predator activity calculated as the mean number of tracks per plot per night (mean of daily means) • 8484 sand plot nights of data	Unreplicated experiment (although could be re-analysed as a classical experiment) 3(1)	NS	4.38	25
6	Castle et al., 2022 Sand plots, poison baiting	Responses of prey to dingo removal	• Manipulative experiment (dingo eradication) • Large spatial scale • Treatments and controls • Measured control • Random allocation of treatments • Treatment independence • Time series data	• Treatment efficacy varied between study sites • Replication used (but not analysed)	• 2 large study sites • 5 year study period • Each site comprised of paired 'inside fence' and 'outside fence' areas • 94–122 road-based sand plots spaced 1 km apart per study site • 14 repeated surveys every 4 months per study site • Predator activity calculated as the mean number of tracks per plot per night (mean of daily means) • 120 km of spotlight	Unreplicated experiment (although could be re-analysed as a classical experiment) 3(1)	NS	2.62	25

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
					transects (6 × 10 km transects per treatment) at each site, surveyed three times each year • 8484 sand plot nights of data • 3360 km of vehicle track spotlighted				
7	Colman et al., 2015 (but see Catling and Burt, 1994, Catling and Burt, 1997, and Catling et al., 1997) Sand plots	Relationships between predators and prey	• Mensurative study • Large spatial scale • Spatial replication	• No treatments or controls • Non-independence between some sites, and between repeat surveys at some sites • Results confounded by seasonal and habitat differences in predator activity	• 44 small study sites • 10 year study period • 20–35 road-based sand plots, spaced 200 m apart, along transects 4–7 km long, at 13 sites, surveyed twice sometime between October 1989 and April 1992 (Catling and Burt, 1994) • 284 road-based sand plots, spaced 200 m apart, along a series of short transects, at 10 sites, surveyed once sometime between January 1995 and June 1995 (Catling et al., 1997) • 10–35 road-based sand plots (443 in total), spaced 200 m apart, along transects 2–7 km long, at 21 sites, surveyed twice sometime between October 1987 and May 1994 (Catling and Burt, 1997) • Surveys occurred sometime in "late summer / autumn and again in late winter / spring" or "autumn and spring" • Sites spaced at least 3 km apart • 5574 sand plot nights of data (2035 from Catling and Burt, 1994, 822 from Catling et al., 1997, and 2717 from Catling and Burt, 1997)	Pseudo-experiment type V 13	S	5.35	35
8	Colman et al., 2014 Sand plots, poison baiting	Relationships between predators exposed to lethal control	• Treatments and controls • Treatment replication	• Unmeasured control • Non-independence between some study sites • Non-independence between treatments at some study site • Non-random allocation of treatments • Invalid comparison of indices between species • Used a binary measure of predator activity over continuous measures	• 7 small study sites • 18 month study period • 40 road-based sand plots spaced 500 m apart per study site • 1 single survey at each study site • Predator activity calculated as "the percentage of plots on which the tracks were detected during the three-night tracking session" • 840 sand plot nights of data	Quasi-experiment type I 5	S	4.32	35

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
9	Fancourt et al., 2019 Camera traps	Relationship between dingoes and feral cats	• Spatial replication • Measured fine-scale spatial relationships between predators	• Results confounded by season, sampling schedule and timing of baiting • No experimental treatments or controls • Land use varied between study sites	• 2 small study sites • 3 month study period • 90 camera traps used per site • Each site divided into 30 × 4 km^2 grid cells, with 3 cameras (1 on-road and 2 off-road) used in each grid cell • 2 repeated surveys (of at least 21 days duration) spaced ~1 month apart • ~7500 camera trap-nights of data	Pseudo-experiment type V 13	NS	6.53	32
10	Feit et al., 2019 Spotlighting, cross-fence study	Relationships between predators and prey	• Treatments and controls, spatial replication • Time series data	• Non-random allocation of treatments • Treatment replication confounded by land use • Non-independence of treatments over time (predator migration was possible) • Invalidly pooled data across different species and land uses • Measurement of control was possible, but not undertaken • Results confounded by historical and current land use	• 4 small study sites • 6 year study period • 1 spotlight transect per site, each ~30 km long • 18 repeated surveys spaced at ~4 month intervals • ~120 km of vehicle track spotlighted (~9720 km over the study period)	Quasi-experiment type I 5	S	4.28	2
11	Forsyth et al., 2019 Camera traps, density estimation	Evaluation of methods for estimating predator densities and interactions	• Multiple density estimation procedures tested	• No treatments or controls • No randomisation • No replication	• 1 very small study site (<1 km^2) • 64 day study period • 100 baited camera traps • 2 cameras (facing different directions) placed at 50 locations within a single grid • 3200 camera trap-nights of data	Pseudo-experiment type VII 15	S	2.47	Government department
12	Geary et al., 2018 Camera traps, fire	Species' association with fire history	• Stratified random sampling	• No treatments or controls • No replication • Non-independence between survey plots (predator migration was possible) • Results confounded by seasonal differences in predator activity	• 1 large study site • 6 month study period (April to August) • 21 'landscape sites' or survey plots, 12.56 km^2 in size and spaced 2 km apart • 5 baited cameras per survey plot (105 in total), spaced >200 m apart, off-road and away from fire scar edges • 1/3 of cameras were deployed in April, 1/3 deployed in May–June, and 1/3 deployed in July for a minimum of 33 days per deployment	Pseudo-experiment type VII 15	S	6.53	19

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
					• Dingo occupancy information supplemented with two scat surveys spaced 3 months apart at each camera location • ~4000 camera trap-nights of data for predators (or 8369 trap nights of data if small mammal camera traps are included)				
13	Gordon et al., 2015 Spotlighting, sand plots	Relationships between predators and prey	• Time series data • Large spatial scale	• Non-independence of study sites over time (predator migration was possible) • No treatments or controls • No replication • Unmeasured control • Used categorical measures of predator activity over continuous measures • Results confounded by seasonal differences in predator activity	• 3 large study sites • 7 year study period • 1 spotlight transect per site, each 120 km, 136 km, and 241 km long • 4 spotlight surveys conducted in 2007 and again in 2012, 2013, and 2014 • 497 km of vehicle track spotlighted • 47 locations (total across all three sites) surveyed with a single sand plot, 40 m long, at the base of a sand dune, for 2–3 consecutive nights, once only sometime between May and October 2012 • In one analysis, predator activity calculated as the proportion of nights that predators were detected • 18 of the 47 locations included an additional sand plot, 40 m long, at the top of a sand dune, sampled once only sometime between July and August 2012 • In another analysis (with data from only 2 sites), predator activity was calculated as "the total activity between dune top and bottom areas per sampling night" • <160 sand plot nights of data	Pseudo-experiment type III 11	S	5.35	35
14	Gordon et al., 2017a Sand plots, cross-fence study	Relationships between dingoes, prey species, and vegetation change	• Treatment and control • Large spatial scale	• Non-random allocation of treatments • Unmeasured control • No replication • Used a categorical measure of predator activity over continuous measures • Results confounded by historical land use,	• 1 large study site • 1 year study period • 91 locations surveyed with a single sand plot, 40 m long, for 2–3 consecutive nights, once only in either May, July, August, or October 2012 or March 2013 • Predator activity calculated as the proportion of nights	Quasi-experiment type III 7	S	5.09	35

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
				sampling effort and schedule, and season	that predators were detected • <273 sand plot nights of data				
15	Gordon et al., 2017b Spotlighting, cross-fence study	Relationships between predators and quail	• Large spatial scale • Treatments and controls, spatial replication • Time series data	• Basic methodological details unreported • Non-independence of treatments over time (predator migration was possible) • Non-random allocation of treatments • Unequal sampling effort between treatments • Unmeasured control • Results confounded by seasonal and habitat effects, and sampling schedule • Predator scat data pooled across all treatments • Spotlighting surveys and scat surveys spatially separated	• 1 large study site • 2 year study period *Temporal study* • Number and length of spotlight transects unreported • Livestock properties sampled on 9 occasions between May 2012 and June 2014, and conservation reserves sampled on 6 occasions between August 2012 and June 2014 • Unreported sampling effort in temporal study (but probably <500 km of vehicle track spotlighted) *Spatial study* • 14 transects located in livestock areas (192 km in total) • 6 transects located in conservation reserves (72 km in total) • Of these 20 transects, 8 were located inside the fence and 12 were located outside the fence • Transects surveyed once only sometime between May and November 2012 • 264 km of vehicle track spotlighted in spatial study	Quasi-experiment type III (temporal study) 7 Quasi-experiment type I (spatial study) 5	S	3.55	2
16	Greenville et al., 2014 Camera traps	Relationships between predators, prey and rainfall	• Time series data	• No treatments or controls • No replication • Results confounded by seasonal and species differences in activity	• 1 small study site • 2 year study period • 25 camera traps, spaced 1–10 km apart, along vehicle tracks in dune swales • Camera traps deployed continuously during the study period • Data pooled across seasons and years into three periods (bust, boom, decline) of variable length • Rodent data pooled across four different species • 10,260 camera trap-nights of data	Pseudo-experiment type VII 15	S	3.23	8
17	Hernandez-Santin et al., 2016 Camera traps	Interactions between dingoes, cats and quolls	• Time series data • Replication attempted	• Non-independence of study sites over time (predator migration was possible) • Control present, but not used	• 2 large study sites • 2.5 year study period • 7 × 2–4 week sampling periods between March 2013 and July 2015	Pseudo-experiment type V (although could be re-analysed as a quasi-experiment type	S	5.99	1

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
				● Replication present, but not possible with the design used ● Results confounded by seasonal differences in species activity	● 5 transects (each 2.5 km long) per study site ● Each transect comprised of 10 baited (with sardines), road-based camera traps, spaced 250 m apart ● 2761 camera trap-nights of data	I) 13(5)			
18	Hunter and Letnic, 2022 Sand plots	Relationships between dingoes and foxes	● Treatments and controls ● Treatment replication	● Unmeasured control ● Non-independence between some study sites ● Non-independence between treatments at some study sites ● Non-random allocation of treatments ● Results confounded by year, season, sampling schedule, timing of baiting, and data transformation	● 27 small study sites ● 4 year study period ● 10–28 road-based sand plots spaced 500–750 m apart per study site ● 1 single survey at each study site ● Predator activity calculated as "the number of sand plots with tracks [divided by] the number nights", and them transformed by dividing all values by the largest value "so that values fell within the range of 0 to 1" ● 2747 sand plot nights of data	Quasi-experiment type I 5	S	3.90	2
19	Kreplins et al., 2021 Camera traps, poison baiting	Relationships between dingoes and mesopredators	● Treatments and controls ● Time series data	● Location of treatments and controls were alternated back and forth during the study period ● Longer term treatment effect obscured by study design (i.e. 'unbaited' areas had a long history of baiting, and were baited a few months earlier) ● Different camera brands were used	● 2 study sites ● 16 month study period ● Baited camera traps were placed at 1 km intervals along 3–4 road-based transects each 20–30 km long in each treatment, and remained in place during the entire study period ● 92 camera traps were used at one site, and 90 were used at the other site ● 93,002 camera-trap nights of data	Quasi-experiment type III 7	NS	2.62	27
20	Leo et al., 2019 Sand plots, poison baiting	Relationships between predators and prey	● Treatments and controls ● Spatial replication	● Non-independence of study sites over time (predator migration was possible) ● Non-random allocation of treatments ● Unmeasured control ● Results confounded by seasonal differences in species activity	● 7 large study sites ● 2 year study period ● Each site comprised of paired treated (baited) and non-treated (unbaited) sub-sites ● Each site was sampled once only (over a two week period) in the dry season (April to November) sometime between April 2012 and November 2014 (although supplementary material says surveys were conducted between May 2013 and May 2015) ● 24 road-based sand plots spaced 1 km apart at each sub-site	Quasi-experiment type I 5	S	3.90	2

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
					(i.e. 48 sand plots per site, or 336 sand plots in total) • Predator activity was calculated as the 'percentage of plots on which we detected tracks during the three-night tracking session' • 1008 sand plot nights of data				
21	Letnic and Dworjanyn, 2011 Spotlighting, cross-fence study	Relationships between dingoes, foxes and hopping-mice	• Treatments and controls • Replication attempted • Two measures of predator abundance used	• Non-random allocation of treatments • Non-independence of "replicate" spotlighting transects • Unmeasured control • Results confounded by seasonal differences in predator activity	• 1 large study site • 18 month study period • Site divided into 'inside fence' and 'outside fence' treatment areas Abundance assessment • 3 spotlighting transects, each 10–20 km long, established on each side of the fence (and considered as replicates) • Site surveyed on only 2 occasions (September 2007 and March 2009) • ~90 km of vehicle track spotlighted Foraging plot assessment • Predator activity surveyed on a single 30 × 30 [m] track-plot surrounding 96 rodent foraging trays (48 inside, and 48 outside the fence), spaced at least 500 m apart • Surveys conducted over 2 nights during a full moon, and for another 2 nights at 96 different locations during a waning moon a few days later • Predator activity calculated as "the number of nights predator tracks were detected divided by the number of mornings each site was assessed for tracks" • 384 sand plot nights of data	Quasi-experiment type I 5	S	5.99	8
22	Letnic et al., 2011b Sand plots	Relationships between dingoes and foxes	• Spatial replication	• Basic methodological details unreported • No treatments or controls • Results confounded by land use, seasonal, and habitat factors	• 26 small study sites • < 1 year (104 days). • Study period unreported • 25–40 sand plots per site, spaced at 1 km intervals, and checked for 3 consecutive days • Surveys conducted once only • Predator activity	Pseudo-experiment type I 9	S	7.14	35

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
					calculated as "the number of nights a plot was visited by each species of predator divided by the number of nights that the plot was considered valid" • <3120 sand plot nights of data				
23	McHugh et al., 2019 Camera traps	Relationships between predators and prey	• Mensurative study • Spatial replication	• No treatments or controls • Results confounded by land use, seasonal, and habitat factors	• 9 small study sites • 1 year study period • 20–40 road-based and baited camera traps per site (298 in total), spaced 500 m apart • 2 × 21-day survey periods per site • Surveys conducted sometime between May 2016 and August 2016, and again between October 2016 and January 2017 • 12,516 camera trap nights of data	Pseudo-experiment type I 9	S	2.91	31
24	McHugh et al., 2020 Camera traps, fire	Predator and prey responses to fire	• Treatments and controls • Spatial replication • Time series data • BACI design	• Non-random allocation of treatments • Sites burned in different seasons and years • Results confounded by seasonal differences in predator activity	• 2 very small study sites • 2 year study period • Sites divided into paired burnt and unburnt treatments areas • 10 camera traps per treatment, spaced a few hundred m apart • Camera traps repeatedly deployed for 3 × 14 day periods before, 3 × 14 day periods immediately after, and 3 × 14 day periods 3 months after fire at each site (9 × 14 day periods in total) • 5040 camera trap nights of data	Quasi-experiment type I 5	N/A	2.24	31
25	Morris and Letnic, 2017 Spotlighting, cross-fence study	Relationships between dingoes, herbivores, vegetation and soil	• Treatments and controls • Time series data	• Non-random allocation of treatments • No replication • Unmeasured control	• 1 large study site • 4 year study period • Treatments allocated for inside/ outside and reserve/ pastoral areas • A single spotlighting transect per treatment, ~30 km long • 14 repeated surveys conducted every ~4 months between May 2012 and June 2016 • ~120 km of vehicle track spotlighted each survey (or ~ 1500 km of vehicle track spotlighted over 14 surveys)	Quasi-experiment type III 7	S	5.35	2
26	Moseby et al., 2019	Effect of dingoes on mammals	• Manipulative experiment (dingo addition) • Treatment and	• No replication • Small spatial scale • Results obscured and influenced by	• 1 small study site • 4.5 year study period • Site divided into	Unreplicated experiment 3	S	4.22	2

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
	Sand plots, dingo addition		control • Time series data • BACI design	incorporation of unrelated data from other studies	'inside' and 'outside' treatment areas • 77 × 200 m sand plot transects sampled over two consecutive nights • 13 repeated surveys between February 2008 and July 2012 • Species activity calculated as "the number of nights a plot was visited by each species divided by the number of nights that the plot was considered valid" • 2002 sand plot nights of data				
27	Raiter et al., 2018 Camera traps, sand plots	Effects of roads and vegetation on predator activity	• Mensurative study • Two factors assessed (roads and habitat) • Spatial replication	• Data pooled across study/survey periods • Invalid assumptions about predator activity • No control used	• 1 large study site • 1 year study period • 16 × 3 km transects beginning at a road and extending perpendicular away from the road, spaced at least 7 km apart Camera trapping • 5 camera trap sites (80 in total) established along each transect at set distances away from the road • 40 camera traps used at any one time, rotated amongst camera traps sites over 4 monitoring periods, for an average of 174 nights each • 13,950 camera trap-nights of data Spoor counts • Tracks and scats counted along entire transects on 3 occasions in January, March and July 2014 • 48 sand plot nights of data	Pseudo-experiment type V 13	N/A	5.99	10
28	Rees et al., 2019a Sand plots, cross fence study	Relationships between predators and prey	• Treatments and controls • Site and treatment independence • Spatial replication	• Non-random allocation of treatments • Unmeasured control • Unbalanced sampling per site, treatment, and survey • Results confounded by sampling effort and schedule, and seasonal effects	• 2 large study sites • 2 year study period • Each site divided into 'inside' and 'outside' treatment areas, inclusive of mixed land use histories in both treatments • Road-based sand plots spaced 1 km apart and sampled for 3 consecutive nights per survey • One site sampled once only in February 2013 (17 sand plots) • The other site sampled twice in November 2014 (118 sand plots) and February 2015 (100	Quasi-experiment type I 5	S	5.99	2

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
					sand plots) • Carrion sampling with 2 × 10–15 km roadkill transects in March 2013 at one site, and 4 × 35–76 km transects in September 2012 at the second site • Bird sampling occurred once only at 200 locations, comprised of a 500 m strip, over four surveys conducted in March 2014, July 2014, March 2015, and April 2015 • Species activity calculated as "the mean number of tracks per plot, per night" • 235 sand plot nights of data				
29	Rees et al., 2019b Spotlighting, cross fence study	Relationship between dingoes and barn owls	• Treatments and controls • Time series data • Spatial replication	• Non-random allocation of treatments • Non-independence of treatments (owls free to forage/ migrate across both sides of the fence over the study period) • Spatial and temporal mismatch between dingo sampling and owl diet sampling	• 7 small study sites • 4 year study period • Spotlighting transects 15–45 km long at each site • 2–12 repeated surveys per site • <315 km of vehicle track spotlighted	Quasi-experiment type I 5	S	3.55	2
30	Schroeder et al., 2015 GPS collars	Fine-scale interactions between dingoes, foxes and feral cats	• Multiple individuals monitored in detail • Examines both spatial and temporal activity of predators	• Small spatial scale • Low sample size of some animals, and short duration • Artificially constructed predator assemblage • Pre/post (BACI) data available for 4 cats, but unreported	• 1 small study site • 2 year study period • GPS tracking of 2 dingoes, 5 foxes, and 10 cats • GPS points continuously recorded every 2 h • 3–16 days of dingo interaction data for each fox, and 3–180 days of dingo interaction data for each cat • Number of GPS points analysed unreported	Pseudo-experiment type V 13	S	2.51	11
31	Stobo-Wilson et al., 2020a Camera traps	Ecological processes influencing mammal abundance	• Large spatial scale • Spatial replication • Multiple methods of small mammal sampling	• No treatments or controls • Results confounded by seasonal differences in predator activity	• 1 very large study area • 3 year study period • 312 camera trap sites, with a mean of 2.5 km between sites • 5 baited camera traps at each site • Each site surveyed once only for 26–80 days (mean 50 days) • Survey effort unreported, but probably ~70,000 camera trap nights of data	Pseudo-experiment type V 13	NS	5.99	34

Table 2 (*continued*)

#	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
32	Stobo-Wilson et al., 2020b Camera traps	Relationship between dingoes and feral cats	• Large spatial scale • Spatial replication	• No treatments or controls • Data potentially confounded by seasonal differences in predator activity	• 1 very large study area • 3 year study period • 376 camera trap sites, with a mean of 2.5 km between sites • 5 baited camera traps at each site • Each site surveyed once only for 26–80 days (mean 50 days) • 83,357 camera trap nights of data	Pseudo-experiment type V 13	NS	5.14	34
33	Wang and Fisher, 2012 Camera traps	Relationships between dingoes and feral cats	• Randomised, complete and representative sampling of the study site • Mensurative study	• No treatment or controls • No replication • Data from multiple camera trap types pooled together (though interspersed throughout the study site) • Results confounded by seasonal differences in predator activity	• 1 small study site • 1 year study period • Camera traps placed on roads nearest to 41 randomly generated locations across the site, and spaced at least 500 m apart • Each location was surveyed for 38–185 nights (mean = 96 nights) sometime between August 2009 and August 2010 • 4045 camera trap nights of data	Pseudo-experiment type VII 15	NS	2.51	1
34	Wooster et al., 2021 Camera traps, scat indices	Fox behaviour at resource points used by dingoes	• Time series data	• No treatments or controls, randomisation or replication • Sample sizes (number of scats collected) unreported • Data pooled across camera trap types, resource point types, and years	• 1 large study site • 3 year study period • Scats collected at a total of 50 resource points (within 20 m radius around 21 water points, 4 cattle carcasses, and 25 rabbit warrens) • Scats collected sometime in the winter of 2016, 2017 and/or 2018 • 10 resource points sampled in 2016, 37 in 2017, and 20 in 2018 (some points sampled once, and others up to three times) • 67 resource point counts of data • 1–3 camera traps deployed at each location during the winter survey periods • 1366 camera trap nights of data	Pseudo-experiment type VII 15	N/A	2.67	5
35	Wysong et al., 2020a GPS collars	Relationships between dingoes and cats	• Multiple individuals monitored in detail • Examines both spatial and temporal activity of predators	• Short duration, or exclusion of substantial amounts of data • Pre/post (BACI) data available, but unreported	• 1 large study site • 1 year study period • GPS tracking of 17 dingoes and 29 cats • GPS points continuously recorded every 2 h for dingoes and every 4 h for cats • Data analysis focussed on the 2 × 70 day periods prior to annual cat control programs in July 2013 and July 2014 • 16,458 GPS points analysed	Pseudo-experiment type V 13	NS	2.51	10

Table 2 (*continued*)

| #	Reference	Study topic	Methodological strengths	Methodological weaknesses	Study scale, sampling strategy and effort	Design classification, and H score	MRH support	Journal impact factor	University rank
36	Wysong et al., 2020b Camera traps, poison baiting	Relationship between sampling strategy and predator occupancy	● Before/after baiting data ● Complete and representative sampling of the study site	● No experimental control, randomisation, or replication ● Short duration	● 1 large study site ● 2 month study period ● 80 camera traps spaced at least 1.5 km apart ● 20 camera traps allocated to each of four treatments (on-road–lure, on-road–no lure, off-road–lure, off-road–no lure) ● 3683 camera trap-nights of data	Pseudo-experiment type VII 15	S	3.60	10
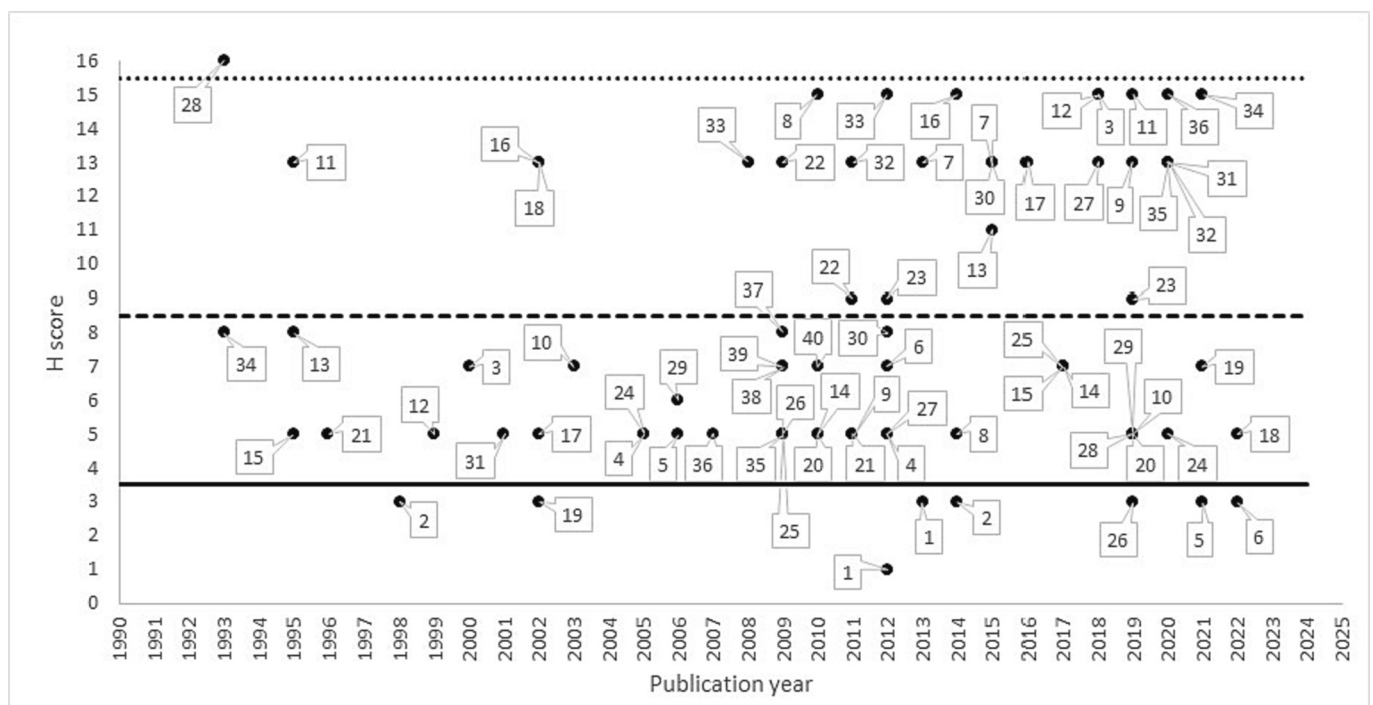
Fig. 1. Trends in the H scores (i.e. causal strength) of study designs investigating dingo-mesopredator relationships, 1993 to 2022. Identification numbers of studies (1–40) published between 1993 and 2012 correspond to those in Table 2 of Allen et al. (2013b), whereas identification numbers of studies (1–36) published between 2012 and 2022 correspond to those in Table 2 of the present study. Only studies with an H score ≤ 4 (bottom band) are capable of providing causal evidence for dingo-mesopredator relationships. Studies above this threshold (solid line) are correlative quasi-experiments (H scores 5–8; middle band) or pseudo-experiments (H scores 9–15; top band) that cannot provide causal evidence for dingo-mesopredator relationships. See Table 2 for further details.

of studies continue to be designed and implemented in such a way that they do not permit making causal inferences for these mechanisms (Table 2; Fig. 1). Only five studies had the ability to assess causal relationships between dingoes and mesopredators (H scores ≤4), and only two of these acquired relatively large sample sizes (Figs. 1 and 3). Not only are most studies done over small spatiotemporal scales with small sample sizes (Figs. 1 and 2), but the strength of their largely correlative and observational study designs (H scores ≥5) has also remained stagnant over a 30 year period (Fig. 1). To date only 11 % (8 of 76) of studies meet the threshold that permits reliable inferences about causal mechanisms (see below), and 57 % (43 of 76) of studies further suffer from some form of confounding associated with the methods implemented to sample predator populations (Table 2; Allen et al., 2013b). In other words, most studies did not collect enough data to reliably describe dingo-mesopredator relationships, nor did they employ all the essential

elements in their experimental design to make causal inferences about these relationships even if they did, regardless of any confounding factors arising from the implemented sampling methods. The prestige or rank of the publishing universities and journals were also unreliable guides to the quality of the science on this subject (Figs. 4 and 5).

The spatial scale of the studies we reviewed ranged from 1 km^2 (Forsyth et al., 2019) to ~45,000 km^2 (Allen et al., 2013a; Allen et al., 2014a) and the temporal scale or duration of studies ranged from 2 months (Wysong et al., 2020b) to 10 years (Colman et al., 2015; see also Fig. 2). With respect to sample sizes, sand plot studies ranged from 48 to 35,399 sand plot-nights of data, camera trap studies ranged from 1366 to 93,002 trap-nights of data, and spotlighting studies ranged from approximately 90–9000 km of tracks surveyed in total (Table 2). There are no magic numbers that signify ideal spatial scales, temporal scales, or sample sizes, but it should be obvious that a greater amount of data
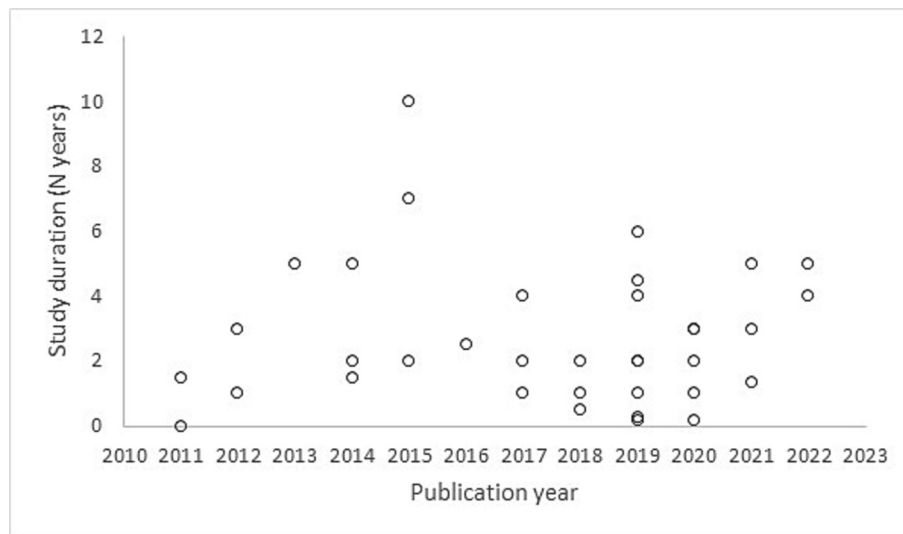
Fig. 2. Duration of the study periods reported in 36 studies investigating dingo-mesopredator relationships, 2012 to 2022. See Table 2 for further details.
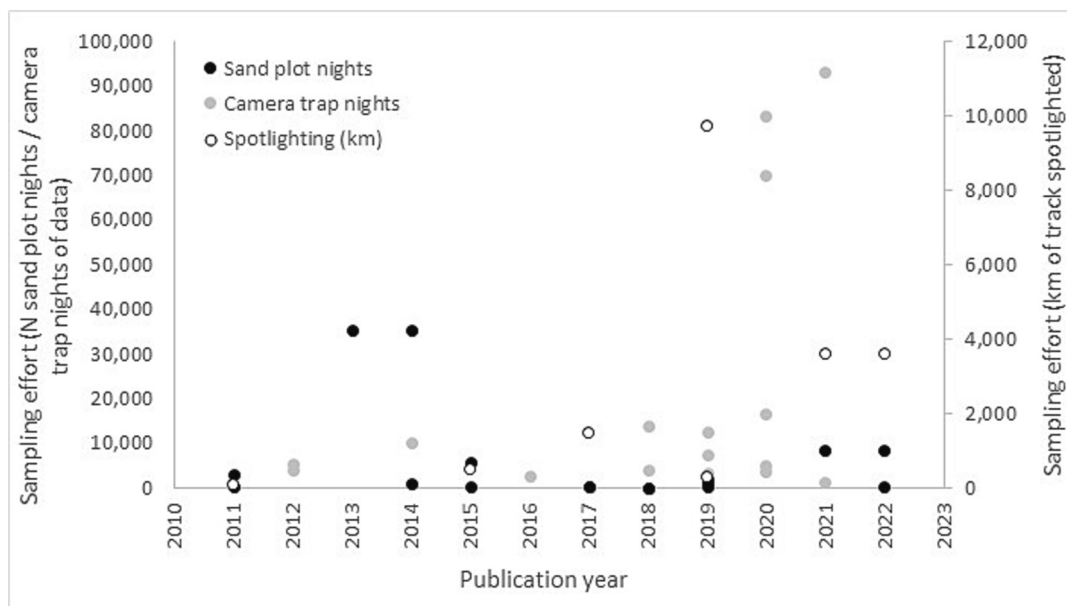

Fig. 3. Temporal trends in sampling effort, sample size or the amount of available data mentioned in 36 studies investigating dingo-mesopredator relationships, 2012 to 2022. See Table 2 for further details.

collected over larger spatiotemporal scales provides more information on population-level relationships between predators than a smaller amount of data collected over smaller spatiotemporal scales, especially when greater amounts of data are collected within an inferentially strong experimental design (Engeman et al., 2017). We concur with Moseby et al. (2012) that studies reliant on scales and sample sizes at the smaller end of these spectrums are unable to yield reliable inferences about dingo-mesopredator relationships at population-level scales relevant to predator managers and policy makers.

As valuable as large amounts of data collected over large spatio-temporal scales are (as did Arthur et al., 2013), even they cannot be reliably used to make causal inferences when they are derived from correlations. For example, Letnic et al. (2011b) and several other studies (Table 2; see also Allen et al., 2013b) frequently attribute observed differences between treatments (i.e. places with or without dingo control) to mesopredator release effects arising in response to dingo control, without adequately assessing whether or not these spatial differences

can be found within treatments (i.e. places without dingo control only). Such differences include greater threatened rodent abundance in places without dingo control, and most of these studies also do not undertake repeated sampling, instead collecting all their data during a single visit to the site over a few days (Table 2). But when repeated sampling is undertaken within large experimental treatments randomly allocated in places without dingo control, the resulting experimental data demonstrate that rodent abundance is driven by the availability of rodent food and habitat, dingoes and mesopredators have positive spatial relationships driven by rodent (prey) availability, and dingo control has little influence on rodent dynamics (Allen et al., 2018). In other words, the trophic cascades assumed to occur in the smaller and weaker correlative studies were not found in the larger and stronger experimental studies (e.g. Allen et al., 2013a; Allen et al., 2014a; Allen et al., 2018). Correlative study designs with non-randomised allocation of treatments essentially force researchers into undertaking analyses that cannot detect important dingo-mesopredator relationships and cannot provide
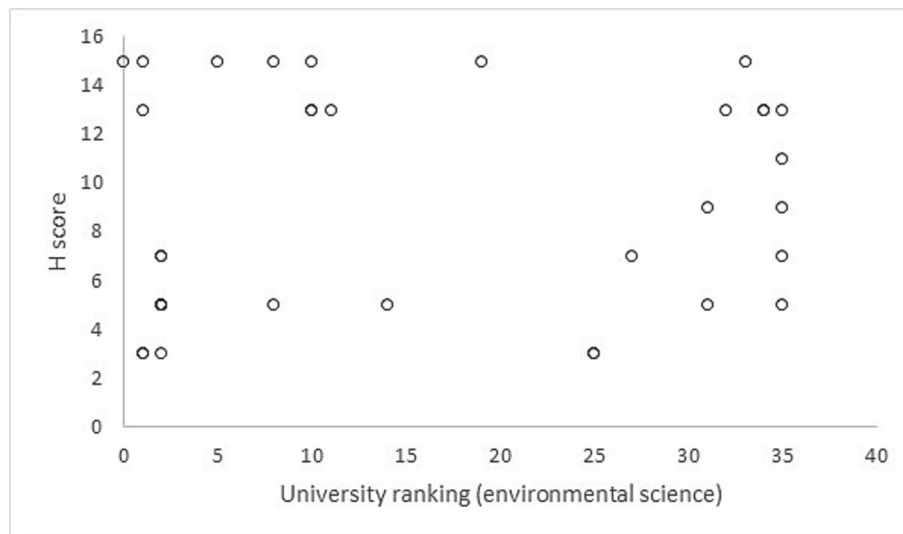
Fig. 4. Relationship between the H score (i.e. casual strength) and university ranking in environmental science, for 35 studies investigating dingo-mesopredator relationships, 2012 to 2022 (a university rank could not be generated for Forsyth et al. (2019)). See Table 2 for further details.
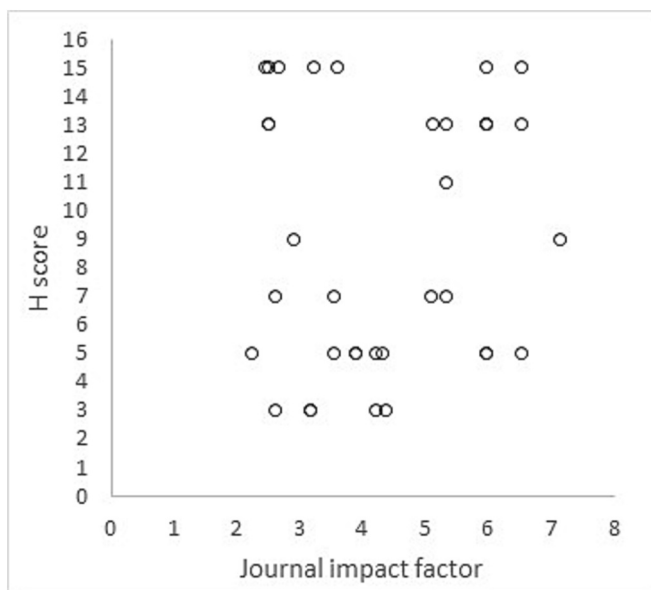

Fig. 5. Relationship between H score (i.e. causal strength) and journal impact factor for 36 studies investigating dingo-mesopredator relationships, 2012 to 2022. See Table 2 for further details.

causal insights (Platt, 1964; Caughley, 1980; Hone, 2007). This large variability in scale, sampling effort and design strength present in the available literature (Table 2) means that the 76 available dingo-mesopredator studies are not all equal or equally reliable, and a false sense of equivalency will arise when this variability is ignored or disregarded.

One might presume that the reputation, status, performance or prestige of the university that produced the work might be a useful guide for identifying reliable studies, but this was also not the case. Higher ranking universities were not associated with higher quality publications (Fig. 4). For example, the five experimental studies that reached the threshold for providing causal inference were produced by the University of Queensland (ranked 1st; $N = 2$; Allen et al., 2013a; Allen et al., 2014a), the University of New South Wales (ranked 2nd; $N = 1$; Moseby et al., 2019), and the University of Southern Queensland (ranked 25th; N = 2; Castle et al., 2021; Castle et al., 2022). One might

also presume that the reputation, status, performance or prestige of the publishing journal might be a useful guide for identifying reliable studies, but this was not the case either (Fig. 5). Studies published in *Global Ecology and Biogeography* (JIF = 7.1), *Journal of Applied Ecology* (JIF = 6.5), and *Ecography* (JIF = 6.0) represented the highest JIFs that empirical studies of dingo-mesopredator relationships have achieved (Table 2). However, the H scores of studies published in these journals ranged from 5 to 15 (Table 2, Fig. 5), indicating that none of the studies published in the best journals had the capacity to make causal inferences about dingoes' ecological roles; they were all correlative. Journal impact factors are a function of citation rates, but citation rates do not facilitate quality assessments for published studies (Kurmis, 2003; Grzybowski, 2015). The key lesson from these findings is that the status or prestige of the publishing university or journal does not reflect the strength, quality or reliability of the dingo-mesopredator studies they produce. Relying on such status or prestige as a surrogate measure of good science also represents an appeal to authority, which is one the world's 100 worst logical fallacies (Michaud, 2018) and the antithesis of 'science' (Allen et al., 2017).

The necessary experimental design features that enable causal inference about dingo-mesopredator relationships are: (1) the presence of matched or paired experimental treatments and controls (or nil-treatment areas), (2) treatments and controls must be randomly allocated, and (3) treatment effects must actually be measured and not just assumed (Caughley, 1980; Hone, 2007; Kimmel et al., 2021). In applied studies where the effects of dingoes on mesopredators are of primary interest, (4) repeated measurements must also occur over sufficient spatial and temporal scales to observe population-level effects that are not confounded by season- or habitat-related variables (Glen et al., 2007; Allen et al., 2011a; Engeman et al., 2017). In ideal circumstances, replication of treatments and controls is also highly desirable, though not necessary for enabling causal inferences (Hone, 2007). Implementing these design features can be made difficult by the time and budgetary constraints imposed on the graduate students and post-doctoral researchers that typically undertake this type of work, but implementation of these features is possible and has been done by such researchers on several occasions (Table 2). Assessment of the limiting design factors inherent in dingo-mesopredator studies indicate that treatment randomisation (or the lack of predator population manipulations) appears to be the biggest stumbling block constraining studies to correlations, along with failure to measure the control and the frequent omission of the repeated sampling needed to measure a treatment effect

(Table 2, Fig. 1). For the present purposes of our review, failing to randomise treatments and controls introduces multiple sources of confounding that undermine identification of causal factors. Or in other words, the number of potential causal factors for the observations grows much larger whenever treatments and controls are not randomly assigned, and there is little prospect of untangling them with subsequent analytical wizardry.

This is, perhaps, most relevant to the many cross-fence studies undertaken since 1993 or those that contrast predator populations in national parks with nearby livestock properties (Table 2, see also Allen et al., 2013b), where random allocation of treatments is not possible. Seven such cross-fence studies (19 % of the studies published since the last review) were conducted on either side of the national Dingo Barrier Fence which separates high-density dingo populations from low-density dingo populations in some places. In such cases, low-density populations of dingoes inside the fence were assumed to have been reduced or rendered 'functionally extinct' by lethal control targeting dingoes (e.g. Gordon and Letnic, 2016; Contos and Letnic, 2019). Yet none of these studies actually measured whether or not the dingoes in their study area were affected in any way by the control programs that targeted them, instead merely assuming that they had been significantly affected simply because they were targeted. Lethal dingo control programs are known to reduce dingo populations anywhere from 0 % to 100 %, and dingo populations often increase following lethal control (Newsome et al., 1972; Thomson, 1986; Fleming, 1996; Allen et al., 2013a; Allen, 2013, 2015; Kreplins et al., 2018; Ballard et al., 2020), so assumptions of 'dingo control' or 'functional extinction' are completely unsupported in the absence of actual measurements indicating this to be the case (Reddiex and Forsyth, 2006). This is especially true given these functionally extinct dingo populations still exert sufficient function to decimate sheep and goat grazing industries across the entire study region (Allen and West, 2013, 2015), requiring the erection of netting fences to save the remaining flocks from being eradicated by those same functionally extinct dingoes (Chudleigh et al., 2011; Clark et al., 2018). Feit et al. (2019) conceded that their study only yielded correlative evidence because they did not manipulate dingo abundance and instead relied upon an assumed difference in dingo density across the fence. Rees et al. (2019a) further acknowledged that the most parsimonious explanation for their observation of reduced ground cover and bird abundance inside the fence may be the 150 years of intensive sheep, goat and kangaroo grazing pressure that occurred there, and not dingo absence (see also Newsome et al., 2001). Statements of such caveats are necessary and welcome, but their presence cannot convert correlations into causations. Stretching to invoke a complex series of unmeasured and cascading ecological processes to explain differential fauna abundances on either side of the fence would have been unnecessary had the treatments been randomly allocated (e.g. Allen et al., 2018). Correlative studies are certainly useful for formulating hypotheses about potential causal processes, but they cannot and do not demonstrate those processes or invalidate the experimental studies that do (Kershaw, 1969; Eberhardt, 1976; Caughley, 1980; Underwood, 1990; Sutherland, 1996; Krebs, 1999; Hone, 2007), regardless of continued and demonstrably unsupported claims that these correlative studies represent 'strong evidence' for dingo suppression of mesopredators (Newsome et al., 2017b).

One of the limitations undermining our critical review was our inability to ascertain exactly what was done in some studies, making it difficult to confidently evaluate their causal strength or reliability. Some studies presented ambiguous descriptions of basic methodological details, such as how many sand plots were used, where they were deployed, and when they were monitored (e.g. Letnic et al., 2011b; Colman et al., 2015; Gordon et al., 2015; Gordon et al., 2017b), or how many GPS points were obtained and analysed (e.g. Schroeder et al., 2015). We evaluated and described the strength of experimental designs and assigned H scores as best we could given the information that could be extracted from the text of each study, but individual studies might have weaker or stronger experimental designs and H scores than what

we report depending on how the available data are organised for analysis. In other words, some studies may have been designed and executed perfectly well for their intended purposes (e.g. studies investigating the effects of fire; Table 2), but, for the purpose of investigating dingo-mesopredator relationships, all save five of them were correlative at best (Table 2). These findings reinforce the importance of clearly describing the methods and sample sizes used to investigate dingo-mesopredator relationships and further highlight the difficulty in repurposing or reinterpreting data collected for other purposes (Fleming et al., 2013; Allen et al., 2014b).

5. Status of evidence for dingo suppression of mesopredators

We have described systemic deficiencies within the literature, but have also shown that eight studies published since 1993 were designed in a way that does enable strong inference about dingoes' ability to suppress mesopredators (Fig. 1). So what do these studies reveal? Allen et al. (1996, 1998) released 16 dingoes onto an island as a biocontrol tool intended to eradicate ~3000 feral goats (which they rapidly achieved; see also Allen et al., 2012, 2021), but unexpected rainfall and goat culling operations on the nil-treatment island confounded any inferences about fauna responses to dingo introduction. Allen et al. (2013a; 2014a; which included unpublished data from Allen, 2005 and Eldridge et al., 2002; see also Edwards et al., 2021) exposed dingo populations to repeated lethal control events across large, open areas and showed that the extant fox, cat and goanna *Varanus* spp. populations failed to subsequently increase, presumably because contemporary dingo control practices did not remove a sufficient number of dingoes for a sufficient period of time to initiate a mesopredator release or subsequent trophic cascade. Improving on this experimental design, Castle et al. (2021) sustainably eradicated dingoes inside large fenced areas and showed that the extant fox, cat and goanna populations likewise failed to subsequently increase, not because dingoes were not sustainably supressed, but because negative relationships between predators were not apparent at large spatial and temporal scales. Castle et al. (2022) further showed that extant populations of multiple mammals, birds and reptiles fluctuated independently of dingo eradication at the same scale, though there was some indication that kangaroos increased where dingoes were removed. At a far smaller scale, Moseby et al. (2019) showed that dingo introduction into a 37 km^2 fenced enclosure produced mixed effects for extant fauna (some species increased, some decreased), but the potential causal factors for their modelled observations were obscured because they were driven largely by their incorporation of unrelated data from a separate correlative study into their models.

The data and results of these inferentially strong, experimental studies – i.e. the *only* studies capable of evaluating dingoes' potential role as a causal agent of mesopredator suppression or release – lead to the following demonstrable conclusions:

1. To date there is still not a single published study that reports a measured decline of foxes or cats in response to measured increase in dingoes, or a measured increase in foxes or cats in response to a measured decrease in dingoes;
2. The repeated lethal control of dingoes in large open areas has never been observed to produce mesopredator release effects for foxes, cats or goannas;
3. The sustained removal or eradication of dingoes from large fenced areas has never been observed to produce mesopredator release effects for foxes, cats or goannas;
4. Dingoes may kill or supress foxes and cats at small spatial scales (i.e. interference competition at an individual level), but these processes have never been observed to scale-up to produce population-level or regional-level mesopredator suppression effects; and
5. Dingo control-induced trophic cascades have never been observed to occur through mesopredator release effects.

Any assertions contrary to these conclusions are demonstrably founded on desktop, correlative or observational study designs or anecdotes with no power whatsoever to make inferences about causal effects, and which also typically exhibit relatively small scales and low sample sizes (Table 2; Allen et al., 2013b). Moreover, these demonstrable conclusions are contextually-independent and consistent between experiments conducted in different times and places, inclusive of above- and below-average seasonal conditions in arid, semi-arid, and tropical monsoonal ecosystems.

6. Conclusions and recommendations

We have shown that (1) the strength or quality of the literature on dingo-mesopredator relationships is typically weak and is not improving over time, that (2) the spatiotemporal scales of most studies and their resulting sample sizes are small, and probably too small to permit meaningful conclusions about these relationships at scales important to management, that (3) institutional prestige and journal impact factor are unreliable guides for identifying studies with the greatest causal strength; and (4) potentially confounding issues associated with predator sampling techniques continue to be commonly reported throughout the literature. These findings have important implications for managers and policymakers concerned about dingoes, and those more broadly interested in the ecological effects of apex predators or scientific integrity and literacy.

Claims that dingoes suppress mesopredators and initiate trophic cascades are unavoidably and demonstrably based entirely on a body of correlative and observational studies with no capacity to reliably describe such causal processes, and claiming otherwise is overt science denialism (Allen et al., 2017). In spite of repeated calls to improve the inferential ability of studies on the subject over many years (Glen et al., 2007; Allen et al., 2013b; Newsome et al., 2015; Engeman et al., 2017; Haswell et al., 2017), the persistent failure of 'the science' to improve over time represents a lot of money, time and effort expended for little to no advancement in knowledge. To reverse this trend we recommend that research funding organisations insist on the implementation of strong experimental designs capable of making causal inferences before they fund research that seeks to investigate causal processes. We also recommend that reviewers, and particularly editors, be more diligent in insisting on proper and complete description of the research methods (sampling methodologies, experimental design, and sampling effort or sample sizes), along with curtailment of any (often causal) interpretations to those that can be supported by the (often correlative) data. We further encourage early career researchers undertaking much of this work to better familiarise themselves with the abilities and inabilities of different experimental designs, and work towards improving the inferential capabilities of their experiments and analyses. The easiest improvements to make include randomly assigning treatments and controls and undertaking repeated predator sampling over sufficient spatial and temporal scales. In these ways, the casual strength of dingo-mesopredator studies may improve in the future.

We further discourage managers and policymakers from basing their decisions on the collective results of the many weak and correlative studies available, and instead recommend reliance on the consistent and contextually-independent results of the stronger studies, as described above. The 'zero level' studies characterised by those with H scores of ≥5 "should be interpreted as hypothesis generating for further study and should not be used in management policy and practice as evidence of causality"; whereas, those with H scores ≤4 should be used in "adaptive management and research… and… management policy and practice, with ongoing assessment in an adaptive management framework" (Hone and Krebs, In press). Dingo management decisions based on unreliable information inevitably lead to poor ecological outcomes (Allen et al., 2011b), so we continue to warn that failure to improve the causal strength of dingo-mesopredator studies will maintain the trend towards wasted effort for no gain. Until we collectively commit to improving the strength of our experimental designs and avoid the methodological pitfalls and limitations that continue to confound most studies on this subject, knowledge of dingo ecology and management will remain 'stuck in the mud'. We hope that this is not the case, and that our review goes some way towards remedying this pattern. More broadly, we hope that our results serve as a useful example to others working on apex predators that they also need to ensure the quality of information they are generating is improving over time. Researchers and managers will then have the best chance of conserving these important and valuable species into the future.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

All the data used to produce this ms is contained within the ms.

Ethics statement

No humans or animals were used in this report, and ethical approval for the study was not required.

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