A technical review on meta-analyses in the context of insect ecology

Meta-analyses first appeared in the 1970s (Gurevitch *et al.*, 2018) in medical and social sciences (Nakagawa and Santos, 2012). Since, ecological meta-analyses have grown in popularity and become increasingly more comprehensive (Cadotte *et al.*, 2012). The aim of any review is to describe the current knowledge on a specific topic (Nakagawa and Poulin, 2012); meta-analysis specifically is a quantitative, statistical method to summarise results across multiple studies (Arnqvist and Wooster, 1995; Gurevitch *et al.*, 2018), often as part of a systematic review (Vetter *et al.*, 2013; Gurevitch *et al.*, 2018). The process must be replicable and answer pre-defined questions (Lortie *et al.*, 2015).

Completing meta-analyses allows researchers to glean broad taxonomic and geographic generalisations, even when independent studies do not concur (Arnqvist and Wooster, 1995; Côté and Reynolds, 2012; Gurevitch *et al.*, 2018). Not only can we determine the presence of an effect, but also its magnitude and possible reasons for variation (Gurevitch and Hedges, 1999; Vetter *et al.*, 2013). Meta-analyses reduce the chance that scientists, media, and policy-makers place too much bearing on individual studies which may be methodologically flawed (Stewart, 2010; Koricheva and Gurevitch, 2014). In this way, a primary paper contributes evidence rather than a definite answer (Gurevitch *et al.*, 2018), which could lead to their devaluation despite their necessity.

Advantages of meta-analyses include their ability to: detect significant effects that individual studies fail to; draw conclusions in a way that statistically parallels that of primary studies; and establish future research questions (Nakagawa and Poulin, 2012). Narrative reviews cannot objectively test hypotheses due to informal non-statistical methods (Arnqvist and Wooster, 1995; Nakagawa and Poulin, 2012), and statistically flawed vote-counting (often confused with meta-analyses) can lead to false conclusions by ignoring the quality of primary studies (Nakagawa and Poulin, 2012). Instead, meta-analyses output a more informative and less subjective effect-size (Harrison, 2011).

Effect sizes are extracted from primary studies and fed into a statistical model to determine overall effect size and a confidence measure (Gurevitch and Hedges, 1999; Gurevitch *et al.*, 2018). Heterogeneity should be quantified using Cochran's Q or I2, the latter of which is becoming increasing popular (Nakagawa and Santos, 2012). Models can then be expanded to include moderator variables - equivalent to explanatory variables in linear models – in an attempt to explain this variation (Nakagawa *et al.*, 2017). R packages including metafor (Viechtbauer, 2010) and metagear (Lajeunesse, 2016) are available to facilitate the process.

Which effect size is used is based on the characteristics of the data, the most common being the standardised mean difference (Cohen's d or Hedges' g), log response ratio, odds ratio, risk ratio, or correlation coefficient (Arnqvist and Wooster, 1995; Viechtbauer, 2010). Of these, the standardised mean difference and response ratio are the most relevant to ecology, due to their nature of quantifying differences between groups (Koricheva and Gurevitch, 2014; Nakagawa *et al.*, 2017). Dirzo *et al.* (2014) used Hedges' g to conclude Lepidopteran species richness and abundance were greater in undisturbed than disturbed sites. Hedges *et al.* (1999) argues the response ratio is more useful because it quantifies proportional change, though it cannot be used if a large proportion of studies have a control mean of zero. Before running models, it is standard practise to weight effect sizes according to study size (Arnqvist and Wooster, 1995; Gurevitch *et al.*, 2018) and precision (Hedges *et al.*, 1999).

Fixed-effect and random-effects are the two baseline models used with the former assuming all studies share a common true effect and only one source of variation is present: the within-study (sampling) variance. The random-effects model also accounts for between-study variance, termed heterogeneity, resulting in wider confidence intervals (Gurevitch and Hedges, 1999; Borenstein *et al.*, 2010; Nakagawa and Santos, 2012; Gurevitch *et al.*, 2018). Additionally, the weights assigned to each study will be more similar owing to each one being composed of within-study variance plus a constant which decreases the relative differences between them (Borenstein *et al.*, 2010).

The model type must be considered at commencement of study (Borenstein *et al.*, 2010) with many authors agreeing random-effects models are superior for meta-analyses in ecology due to high heterogeneity (Gurevitch and Hedges, 1999; Noble *et al.*, 2017). This is especially true of insect population trends, which typically demonstrate high annual variation (Fox *et al.*, 2019; Didham *et al.*, 2020; Montgomery *et al.*, 2020). To identify causes of variation, it is common to subsequently run mixed-effects models (or meta-regression) involving moderators (Nakagawa and Santos, 2012; Gurevitch *et al.*, 2018). Van Klink *et al.* (2020) modelled temperature and precipitation as moderators but failed to explain significant variation in terrestrial and freshwater insect abundance trends (1925-2018). There was a weaker trend for protected compared to unprotected areas, though this was not formally tested. Further, Jähnig *et al.* (2021) highlight that abundance does not accurately describe population status considering sensitive species may be being replaced by tolerant ones.

A major limitation of meta-analyses is the commonplace of poor reporting in primary studies (Stewart, 2010). Researchers fail to report variances (Gurevitch *et al.*, 2018) and sample sizes (Côté and Reynolds, 2012), leading to exclusion of these papers or consequent use of unweighted meta-analyses, which over-estimate the influence of smaller studies (Koricheva and Gurevitch, 2014). Authors of meta-analyses also appear unaware of the high methodology standards. It is not uncommon for authors to withhold details on the inclusion criteria for studies (Nakagawa *et al.*, 2017), statistical model used (Koricheva and Gurevitch, 2014), or heterogeneity between studies (Vetter *et al.*, 2013). In Senior *et al.* (2016)'s review of 700 ecological studies, total heterogeneity was reported in fewer than 40%. Further, only one of the 133 meta-analyses reviewed by Vetter *et al.* (2013) satisfied the criteria identified as necessary for a high quality meta-analysis. To overcome this issue, journals need to enforce clear guidelines. Each report should follow a PRISMA-like framework (Page *et al.*, 2021) to aid the ability of successive researchers to replicate the process (Nakagawa *et al.*, 2017). Additionally, it is highly recommended that a forest plot is presented in the results to allow the reader to quickly gauge effect sizes and precision for each study, in addition to variation between them (Anzures‐Cabrera and Higgins, 2010; Vetter *et al.*, 2013).

Publication bias - due to significant results being more likely to be published - is frequently suggested as a flaw of meta-analyses (Nakagawa and Santos, 2012; Gurevitch *et al.*, 2018). Naturally, this is more likely to lead to over-estimation of effect size (Stewart, 2010). Nevertheless, we can detect publication bias via funnel plots and regression tests (Nakagawa and Santos, 2012). In a funnel plot, studies should be symmetrically distributed around the true effect size, with smaller and less precise studies demonstrating more variation from this value if no publication bias exists (Anzures‐Cabrera and Higgins, 2010; Harrison, 2011). Despite this, any apparent asymmetry could be due to experimental design (Gurevitch and Hedges, 1999) or chance (Nakagawa and Santos, 2012). Duval and Tweedie (2000)'s trim and fill method can be used to restore funnel symmetry by adjusting for missing studies. Additionally, over-coming publication bias may require aims and methods to be submitted before study initiation (Stewart, 2010).

Particularly relevant to insect ecology is the issue of non-independence, leading to increased chance of false positive results (Nakagawa and Santos, 2012; Nakagawa *et al.*, 2017). Although random-effects models are more suited to ecology than fixed-effect models, they were designed to take one effect size per study as their input. This is often not the case in ecology, for example Dirzo *et al.* (2014) calculated 52 effect sizes from 15 studies, which will overlap due to effect sizes being from the same study, or based on related species. Some researchers opt to use the log response ratio statistic as it is less susceptible to non-independence than the standardised mean difference (Noble *et al.*, 2017). Others only include one effect size per study, though this results in loss of data, and hence statistical power (Nakagawa and Santos, 2012). Therefore, the best approach is to apply multilevel models which account for the dependence structure of the data (Assink and Wibbelink, 2016).

As meta-analyses methods continue to evolve, fitting multilevel models should become more common, especially in insect ecology (Assink and Wibbelink, 2016). The use of machine learning to screen studies for inclusion, and improved methods for data extraction will ease the process (Lajeunesse, 2016; Gurevitch *et al.*, 2018). It is also likely that we will see enhanced methods for estimating statistical power, and assessing publication bias (Lajeunesse, 2016). Finally, as data become more available and accessible, and reporting standards increase, updating previously performed meta-analyses is achievable, enabling more reliable and up-to-date conclusions (Nakagawa *et al.*, 2017).

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