



EDITORIAL

Circle the bandwagons – challenges mount against the theoretical foundations of applied functional trait and ecosystem service research

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After 8 years at the helm, we tip our hats to Simon Leather who is stepping down as Editor-in-Chief of *Insect Conservation and Diversity* after so successfully launching and nurturing the journal through its early years. The journal has gone from strength to strength under Simon's leadership, and we are fortunate that he will continue on in a new role as Senior Editor so that he can pass over the reins (reign?) to Raphael Didham, who will be the new Editor-in-Chief from 2016. May the next 8 years be just as successful! It is certainly our pleasure to announce that the recent 2014 Thomson-Reuters journal citation rankings show *Insect Conservation and Diversity* rising rapidly through the top quartile of Entomology journals (to 11th out of 92 journals), with an impact factor of 2.174. With continuing high-quality submissions from our readers, we know that this rising trend will only increase into the future.

The change in Editor-in-Chief role provides an interesting opportunity to reflect on where we have come from and where we are going with recent research trends in insect diversity and insect conservation studies. Out of the broader template of research needs that we identified in 2010 (Didham *et al.*, 2010), two emerging trends stand out to us. One is the meteoric rise of functional trait research to the point where even its key progenitors think it has become a 'bandwagon' (McGill, 2015) that is strong on vacuous generalities, but weak on mechanistic underpinnings. The other is the rapid weakening of socioecological support for the 'nature for people' platform in conservation management (Mace, 2014), as the realities start to bite on how to operationalise the measurement and monitoring of ecosystem services (Naeem *et al.*, 2015), and what the pitfalls would be of a full commodification of nature (McCauley, 2006; Redford & Adams, 2009; Mace, 2014). Naturally, the two trends are strongly interlinked because socioecological need drives increased demand for research, and that research can subsequently strengthen the theoretical basis for applied conservation outcomes if the science is strong (but it's a big 'if'). The problem is that the reverse can also be true because weak

scientific foundations behind decision-making can seriously undermine an ill-founded conservation agenda (Redford *et al.*, 2013). So, is the biodiversity–trait–service trifecta sufficiently well formulated, and well supported, that it can survive serious scrutiny? Many recent studies suggest not.

Where, then, have things gone awry? A superficial view of the trait literature might suggest that the measurement of diversity, traits, function and services are now all 'mainstream' within ecology and conservation biology, supported by a huge body of literature (McGill, 2015). And what could be wrong with that? We, among many others, had been calling for this type of development in the field for some 20 years, predicting that quantitative measurement of the functional effects of species on ecosystem processes should lead to greater mechanistic understanding of anthropogenic impacts on biodiversity (e.g. Didham *et al.*, 1996). We suspect that few people disagreed, but the implementation of functional trait approaches was sporadic and phenomenological until the influential synthesis and reorientation of the field 10 years ago by McGill *et al.* (2006) and others. Since then there has been an explosion in the number and types of trait studies, supported by increased availability of statistical approaches to the analysis of phylogenetic and trait diversity. It is fair to say that the majority of these studies have focused on plants, but there is growing representation of insect conservation and diversity studies in the trait literature as well, predominantly within freshwater systems (e.g. Poff *et al.*, 2006; Verberk *et al.*, 2013; Orlofske & Baird, 2014). Comparable trait studies on terrestrial invertebrates have, until very recently, lagged behind quite noticeably (e.g. Gibb & Parr, 2013; Barnes *et al.*, 2014; Yates *et al.*, 2014; Gibb *et al.*, 2015; Retana *et al.*, 2015).

Unfortunately, the vast majority of these studies fail to address either of the two central pillars of McGill's vision for the field: 'an emphasis on functional relationships between quantitative variables [linking traits, environment, performance and abundance] feeds rapidly into the identification of general patterns and, hence, [...]

predictive statements to help policy makers make informed decisions' (McGill *et al.*, 2006; p.183). Instead, there is rarely any serious discrimination of what constitutes a 'trait' on the spectrum from fine-scale physiological traits through to coarse-scale life history syndromes, and whether any or all of these have a direct functional interpretation. Most studies simply quantify a small suite of easy-to-measure morphological attributes of a few 'representative' individuals of each species within a defined taxonomic or trophic group, and report the results as if trait patterns are the end-game in their own right (usually distilling multidimensional trait space down into simple metrics of trait diversity or dispersion, or 1-D composite axes of trait covariance). These species-level trait approaches ignore overwhelming evidence for massive individual-level trait variability (Messier *et al.*, 2010; Webb *et al.*, 2010) and adaptive plasticity in relation to environment (Berg & Ellers, 2010), and are always likely to defy the emergent generalisation that McGill *et al.* (2006) had hoped for, precisely because they are so far removed from the stuff of natural selection (selective pressures acting on the performance and fitness of individuals with different suites of traits responding to varying environments and biotic interactions).

The antidote to this is not necessarily to abandon the trait bandwagon (McGill, 2015), but to raise the bar on expectations for (i) defining the mechanistic foundations behind the choice of functional traits, (ii) measuring individual-level trait variation in relation to biotic and abiotic environmental gradients, and (iii) quantifying the direct functional effects of trait variation on performance (particularly in relation to ecological processes), rather than just assuming invariant species-level trait correlates of ecosystem functions. Aspects of these approaches are starting to emerge in freshwater (Orlofske & Baird, 2014) and terrestrial (Barnes *et al.*, 2014) invertebrate studies, but we have barely scratched the surface on understanding the mechanistic links between traits, environmental variation and functional performance (which might well operate quite differently in animals than they do in plants).

Arguably, attempts to deliver the sort of predictive statements about functional trait responses that might be useful for insect conservation also face greater impediments than they do for plants or vertebrates because of the huge diversity of species and complexity of life history strategies. It is surprising, then, that Cardoso *et al.* (2011) did not even mention the word 'trait' in their seminal opinion piece on the seven impediments to effective invertebrate conservation: linking three societal dilemmas, (i) the public dilemma (invertebrates and their ecological services are mostly unknown to the general public), (ii) the political dilemma (policymakers are mostly unaware of invertebrate conservation problems) and (iii) the scientific dilemma (basic science on invertebrates is limited), to four contributing scientific shortfalls, (iv) the Linnean shortfall (most species are undescribed), (v) the Wallacean shortfall (species' geographic distributions are poorly known), (vi)

the Prestonian shortfall (spatio-temporal variation in population abundances are poorly known), and (vii) the Hutchinsonian shortfall (species life histories and sensitivities to habitat change are largely unknown) (Cardoso *et al.*, 2011). Clearly, an important centrepiece in several of their arguments was our lack of understanding of invertebrate-mediated ecosystem functions and the delivery of ecosystem services for humans (Cardoso *et al.*, 2011). Cardoso *et al.* (2011), however, never draw out the obvious shortfall in our understanding of the functional traits that underpin ecosystem services, and what this might mean for our ability to draw generalisable predictions for policy makers. Perhaps, this 'Eltonian shortfall' – the lack of knowledge of the functional attributes of interacting organisms and their subsequent influences on food web structure and function (see also Peterson *et al.*, 2011) – should be formalised as an 'eighth' impediment to invertebrate conservation (although we note that Hortal *et al.*, 2015 prefer to distinguish this as the Raunkiaeran shortfall in deference to the life form classification of Raunkiaer, 1934; although typology of forms based largely on growth point position in relation to abiotic stress simply reinforces most of the problems McGill, 2015 identifies in modern trait ecology today).

In some ways, the major societal dilemma for nature conservation might actually be the increasingly long bow that the ecosystem services paradigm is drawing despite a weakening scientific foundation. A pointed example is how Cord *et al.* (2015) believe that informed management decisions toward resolve conflicting objectives in the United Nations Sustainable Development Goals (SDGs) could be achieved by monitoring ecosystem services from space. Really? Even the causal relationship between remotely sensed vegetation structure/reflectance and natural capital at the stocks (versus flows) end of the ecosystem services spectrum is weak at best (e.g. carbon sequestration in above ground vs below ground reservoirs). It is even less conceivable that a causal link could ever be drawn between remotely sensed Earth Observation features and flows of ecosystem services such as invertebrate-mediated pollination, decomposition and so on. As Naeem *et al.* (2015) point out, a strong social sciences platform behind utilitarian ecosystem services schemes does not abrogate the requirement for strong natural sciences to understand (and predict) the delivery of functions by individual organisms with complex suites of traits embedded in diverse natural ecosystems. Yes, the particular 'functions' that are selected as beneficial 'services' from a human perspective are inherently subjective, but the mechanisms driving their delivery are no less objectively dependent on the complex interplay of trait-environment linkages than any other ecological function. Until the scientific foundations of trait ecology are strengthened, then the science case for predicting human impacts on ecosystem services will remain weak, in spite of the increasingly central role that it plays in the Aichi targets for the Convention on Biological Diversity, the United Nations SDGs, and the Intergovernmental Platform for Biodiver-

sity and Ecosystem Services. Insect ecologists and conservation biologists can play a major role in turning this around.

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