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EDITORIAL

Solving environmental problems in the Anthropocene: the need to bring novel theoretical advances into the applied ecology fold

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Introduction

The romanticized view of untouched, pristine nature is fast disappearing. From pollution and deforestation, to the introduction of non-native species, we now live in a world where almost every major ecosystem has been impacted by human activities. Geologists have now recognized this wholesale alteration of the Earth's environment as sufficient to demark our current era as a new geological epoch – the Anthropocene (Zalasiewicz *et al*. 2008).

The identification of the forces that underpin the physical and biological changes in the Anthropocene have also led to increased global awareness about the consequences of our actions and motivated numerous international, national and regional policies that focus energy and resources to remedying the more disastrous effects of our actions and prevent future impacts. These are, for example, encapsulated by the recent Conference of the Parties meetings on climate (COP 22 of the United Nations Framework Convention on Climate Change – UNFCC in Marrakech, November 2016) and biological diversity (COP13 of the Convention on Biological Diversity – CBD in Cancun, December 2016).

Translating global awareness and concern into effective policies requires sound science to inform management decisions. As a result, applied ecology has increased in prominence and relevance. Ecology is obviously not the first science whose relevance to human wellbeing has become a core component in driving research and funding decisions. The study of human physiology, for example, has indispensable relevance to medical science – that is, the value of this discipline is measured by its ability to help sick people, and not necessarily by its contribution to understanding better how healthy people function. In a similar way, ecology needs to be relevant for our 'sick people': human-dominated landscapes and their vulnerable species and functions. Ecologists have spent much effort studying intact and semi-natural systems to understand the basic operations of nature. But now, we are required to develop this understanding further to minimize loss, and to improve ecological integrity and human wellbeing.

Applied ecology aims to use ecological knowledge to improve the state of biodiversity and the services ecosystems deliver. Potential interventions range from designing and prioritizing landscape protection (Oliver *et al*. 2012), ensuring the delivery of food production and other services (Carvalheiro *et al*. 2012), local scale remediation of chemical contamination and restoration (Rohr *et al*. 2016) to global scale rewilding (Svenning *et al*. 2016). Applied ecology indeed provides evidence and tools that can inform management and policy across spatial scales, can lead to new developments in our fundamental understanding of the natural world, and is at the forefront of using ecological knowledge to develop and implement strategies. Yet, despite the multiple advances we see in every issue of *Journal of Applied Ecology*, there is differential success in the transition of some ecological tools and concepts into applied practice. Our goal here was to examine how and why some theories, concepts and methods successfully transition to the applied realm and to ask if some other areas of research have more to offer applied ecology than has yet been realized.

Defining applied ecology

Applied ecology is often seen as a subfield of ecology – but we argue that it is more properly seen as the endpoint of all ecological concepts or theories (Fig. 1), as well as

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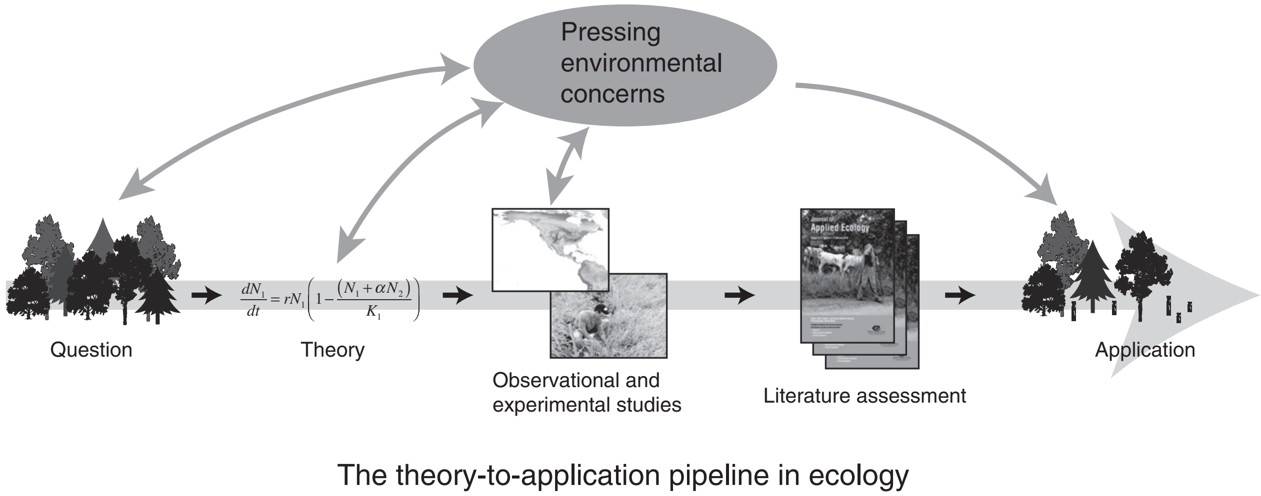

Fig. 1. The idealized theory to application pipeline in ecology. The transition from fundamental questions to application requires several critical transitions, from theory to experimentation, and the accumulation of studies into robust and generalized understanding, before designing applied actions. However, pressing environmental concerns might provide sufficient incentive to develop new theory and experiments, or justify circumventing the pipeline to develop applied actions based on incomplete information or theory development.

the 'space' where interactions between ecological science and society can be explored and advanced (Toomey, Knight & Barlow 2016). Just like ecology, applied ecology spans all spatial and temporal scales, levels of biological organization and interactions among these. Given this view of applied ecology, all subfields of ecology provide concepts, tools and data that can potentially be used in designing applied management action and conservation policy. Certain applied research traditions, such as invasion biology and conservation biology, have contributed greatly to generating and/or implementing ecological concepts, and to combining those with the socio-economic underpinnings that are critical for the development of sound policy. In this context, we recognize applied ecology as a much broader enterprise than these subfields alone. Here, we ask how that much wider range of ecological subfields have interacted with applied research.

By bridging the space between ecological sciences and the management of biological resources or ecological systems, applied ecology inhabits the space between fundamental science and hypothesis testing and the development and application of novel solutions and technology. Of course, the development and/or application of ecological concepts and technology to environmental management requires that solutions to problems be placed into broader socio-economic realities. Applied ecology is more likely to progress through interdisciplinary tools and collaborations, especially with economics and other social sciences. Although we require articles to have a strong ecological basis, we welcome submissions that successfully integrate concepts, analysis and information from other disciplines, as these can provide step changes in our understanding of key issues (e.g. Gallardo & Aldridge 2013; Prowse *et al*. 2015).

Given the inherent breadth of applied ecology, we can ask if various subfields of ecology equally interact with applied management, or whether various intellectual or structural hurdles limit transition to application. Here, we use our collective experience as Editors of *Journal of Applied Ecology* to review the several barriers that might impinge how ecological research transitions to application and impact (Milner-Gulland *et al*. 2012), and we end with a call to all ecological subfields to contemplate opportunities to generate applied outcomes.

Bridging the theory–application gap

A classic example of the successful interaction between theory and application is exemplified by metapopulation biology. Levins (1969) modelled the persistence of single species populations in a patchy landscape, based on just two parameters – colonization and extinction rate. He referred to this model and the general phenomenon of population dynamics across patches as 'metapopulations'. Levins' first papers on metapopulations (Levins 1969; Levins & Culver 1971) were completely theoretical and generated new hypotheses about spatial population dynamics. However, it was not until the 1980s and 1990s that this concept began to gain traction in the literature and empirical tests of metapopulation dynamics began to appear (e.g. Hanski 1983, 1991). Researchers quickly recognized that the metapopulation concept and theory were instrumental to understanding population increases and declines in fragmented landscapes, and as the theory was tested and expanded to include factors other than just colonization and extinction rates, it became a central tool to understand both the spread of undesirable species and the decline of sensitive species. Today, the metapopulation concept appears frequently in *Journal of Applied Ecology* to understand and predict both species invasions and the persistence of threatened species. For example, metapopulation theory is used to understand and develop management for invasive amphibians (Chandler *et al.* 2015; Letnic *et al.* 2015) and for crown of thorns outbreaks across coral reef networks (Hock *et al.* 2014). On the other end of the spectrum, it is used to support the conservation of vulnerable species including black-footed ferrets in plague-dominated landscapes (Shoemaker *et al.* 2014) and the persistence of plants in fragmented landscapes (Teller, Miller & Shea 2015).

It is clear that some subfields of ecology have had a more difficult transition from the theoretical and empirical stages of investigation to application. This could be simply because some subfields are inherently too theoretical or complex, and so lack obvious application; however, we do not think that this is generally the case. This 'theory–application gap' is certainly not unique to ecology, and there is a growing emphasis on assessing impact across all areas of research. For example, evolution has important relevance for applied management, from the evolution of pesticide resistance (Jansen *et al.* 2015) to adaptation to environmental change (Purcell *et al.* 2008), and here too many evolutionary concepts and theories struggle to become germane to applied problems. In essence, the value of fundamental research to applied practice ranges from heuristic value in early stages to practical pragmatic solutions to on-the-ground problems (Barlow *et al.* 2016). We see three main hurdles that slow the transition from basic to applied ecological science.

1. THE THEORY–APPLICATION TIME-LAG

Ecological subfields or research traditions start with a series of observations and questions, and typically rely on a theoretical phase to generate hypotheses (Fig. 1). Observational and experimental studies are then designed to evaluate hypotheses, and once independent assessments accumulate in the literature, critical reviews and meta-analyses then evaluate the validity of a set of explanations for certain processes and patterns observed in nature (Fig. 1). This process means that in order for research to be germane to applied management, the accumulation of evidence requires a substantial amount of time. Consequently, some younger subfields have yet to transition to the applied phase. Of course, this is an idealized view of how science works, and the accumulation of evidence for or against a particular theory, and the subsequent belief of scientists is undoubtedly a very complex process (e.g. Lakatos 1976). Further, the transition to the application phase can circumvent evidence accumulation because either researchers are primarily interested in applied management, or pressing environmental concerns influence research and funding decisions for those research projects that prioritize applied management. The opposite problem – that individual researchers are more motivated by research on theoretical problems and do not see personal value or fulfilment in pursuing applied research – may be even more common.

A field that has yet to have a major impact on applied practice is metacommunity ecology, first formulated in 2004 as the study of the outcomes of multispecies interactions in spatially structured (patchy) landscapes (Leibold *et al.* 2004). The conceptual links to metapopulation ecology are obvious and explicit. However, metacommunity concepts, while proving incredibly important for understanding community structure and dynamics, have not been widely used to model or evaluate applied actions. For example, metapopulation models are used to understand the role of roads on population persistence, but metacommunity models have not been used to gauge the effect of those roads on competitive dynamics, how diversity responds to disturbance or predator–prey dynamics for complex food webs.

2. THE DATA AVAILABILITY GAP

Sound applied ecology planning requires a data-driven evidence base. For many systems, management and conservation decisions are being made with insufficient data (Diniz-Filho *et al.* 2013; Stephens *et al.* 2015; Geijzendorffer *et al.* 2016; Honrado, Pereira & Guisan 2016). As a result, basic information to underpin management decisions is badly needed. As Hans Kruuk pointed out (in the context of carnivore management), '...perhaps the most important message ... is the dire need for more basic data on the natural history of carnivores... This we need not just for the almost extinct ones, but for virtually all of them. Not Nobel-prize winning studies, but basics, and lots of it' (Kruuk 2009). As this quote makes clear, this type of research might not be highly valued for individual careers, but it needs to be prioritized nonetheless. Even when many papers have been produced on a topic, this does not mean that the data necessary for the application of underlying theories are available. For example, systematic conservation planning requires detailed biodiversity and cost data at the planning stage, as well as at later stages to evaluate the success of ongoing conservation activities (Margules & Pressey 2000). Unfortunately, the accurate and high resolution data necessary for this planning and evaluation are seldom available, especially in the most biodiverse nations with some of the most urgent environmental issues. Thus, even if an ecological concept is well tested and understood, actual applied implementation might be limited by the lack of relevant data.

3. SCALE MISMATCHES

Applied management and conservation planning are often explicitly linked to specific spatial scales. These scales might be determined by the ecological phenomena (e.g. species population or range size, an ecosystem process) or by geopolitical delimitation. Successful applied management or conservation actions require that ecological understanding, the targets of management and governmental agency authority or property size, all occur at commensurate spatial scales (Guerrero *et al.* 2013). Scale mismatches are likely one of most important limitations to successfully transitioning from pure ecological science to applied solutions. Although policy is often set at national or international levels (e.g. CBD, EU Birds and Habitats Directives), our detailed understanding of mechanisms and processes is obtained at relatively small spatial scales. However, threats range in spatial scale from global climate change to local road construction, and ecological understanding and management priorities may not adequately reflect the scale-dependent nature of different threats. Thus, while the ecological concept development might be sufficiently mature to allow for applied implementation, the data necessary to plan and evaluate management and conservation actions might be lacking or simply at an incomparable scale. For example, changes in species' range sizes with climate change might be well understood at large spatial scales (e.g. Maiorano *et al.* 2013), but how nature reserves should plan for and manage the local repercussions (e.g. species migration) is often unclear to managers.

Overcoming the theory–application gaps

In many ways, the threats to biodiversity across the world are remarkably consistent – in many regions, they remain principally Aldo Leopold's 'axe, plow, cow, fire and gun' (Leopold 1933), but we can now safely add climate change, roads, diseases and invasive species to that list. Yet, the management responses to these generic threats may be very different, depending on the societal context, interactions between drivers and the main environmental targets. So, perhaps one of the greatest challenges for applied ecology is how to move beyond case studies, and develop science whereby management-relevant inferences developed in one context can be applied elsewhere.

It is easy to argue that scientifically informed applied management is more important than ever before. Solving or ameliorating many of the world's most pressing environmental problems requires advanced ecological understanding and demonstrably successful management solutions. However, given the fact that not all ecological theories or subfields have advanced to the application phase, the question is, how do we move forward applied conservation and management agendas? Moreover, the more pressing the environmental concern, the greater the need for ecological science to leap over the theory–application gap (Fig. 1).

Leaping over the theory–application gap requires that researchers are willing to take a chance that the limited understanding, data or experience available to inform management actions is enough to provide a basis for action. People may fear making a mistake or being scrutinized for their decisions. The natural inclination when the stakes are so high environmentally, economically, and for one's reputation, is to be risk-averse and instead wait for greater certainty in ecological and applied understanding. Here, we provide five strategies for quickly bridging the theory–application gap.

In the first instance, we can say that in the absence of scientific understanding of a system or potential impact of human activity, the way forward should assume that ecosystem injury will result. In essence, we repeat Wilson's (1992) recommendation that the default position should be to assume that natural systems have unmeasured value and that they deserve protection lest we risk losing these benefits. If the theory–application gap is caused by the lag in testing theory, or a lack of data availability, then we should adopt the precautionary principle and act now to minimize environmental harm while the science catches up with the applied concern. However, this option is challenging in most circumstances and relies on a societal ethic that is willing to limit economic growth for uncertain benefits.

In contrast to this first approach, the next four approaches find ways to accelerate the rate of interactions between theory and application. First is that researchers use existing information to test theories quickly, and this approach is best enhanced if data are permanently stored and openly accessible to facilitate sharing. We live in an era where data can accumulate much quicker than our ability to analyse and understand them. For example, the Long-term Ecological Research network continuously records data from monitoring schemes in more than 25 reserves around the USA, producing a copious amount of data that may be of use for testing a number of hypotheses. Although there will always be an important place for new studies and carefully thought out field experiments, we should not overlook the huge potential of analyses or meta-analyses of existing data to advance theory quickly.

The second way to facilitate better integration between theory and application is to develop a relatively low threshold for the number of positive tests of theory needed before designing applied ecology studies that assess management options. This option would argue that when the environmental concern is high, we need only a few confirmatory cases before applications can be designed and tested. However, an important caveat is that studies testing a particular theory need to be well designed and extremely robust, and that conclusions are clear and can be used to inform management decisions. Management actions can then be implemented as robust experiments, so that we can continue to learn and develop strategies after implementation (Salafsky, Margoluis & Redford 2001).

Thirdly, we would strongly advocate that ecological studies are designed with applied ramifications in mind (Barlow *et al.* 2016). Rather than bemoaning the research-implementation gap, we should be seeing it as a productive space where we can explore shared interests (Toomey, Knight & Barlow 2016), develop new insights into theory and application and build long-term relationships that would allow us to test those insights more effectively. For example, perhaps some of our robust inferences about environmental management require data and input from the private sector (Armsworth *et al.* 2010; França *et al.* 2016). Before designing a research project, practitioners could be consulted to see if mutual interests can determine study design and more easily provide a pathway from theory to practice. Even theoretical papers can be more closely aligned with applied problems by thinking about aspects of theory or simulations that can be used directly to inform management practice.

Finally, ecological research can be promoted within interdisciplinary collaborations so that social and economic considerations influence study design and interpretation. Just like with the previous point, by including diverse points of view in initial discussions about research projects, the applied relevance is more likely to be a natural consequence of the research. Perhaps more importantly though, is that the pressing environmental issues are caused by human behaviour and economics, and we are unlikely to provide generally applicable solutions without considering these interdisciplinary aspects.

Conclusions

There is a desperate need to develop management and conservation applications from emerging areas of ecological research. The pressing environmental concerns facing the modern world require that we circumvent the 'natural' trajectory of ecological research and that applied ecology research quickly adopts new concepts and tools. This requires more collaboration among ecologists, applied practitioners, industry, economists and social scientists.

Of course, even if ecological research transitions to applied relevance, and interdisciplinary collaborations are maximized, this does not mean that this research will naturally transition to organizational or governmental policy. Independent of our theory to application gap, is a science to policy gap, which has been frequently commented upon (Bradshaw & Borchers 2000).

Moving forward, we urge authors to develop novel management recommendations based on basic ecology subfields that have yet to fully develop applied protocols. Researchers at all levels, from graduate trainee to senior scientists, should consider how their research could be used to inform applied management.

Finally, we wish to reinforce that *Journal of Applied Ecology* is the home for applied work stemming from any subfield of ecology, working at any spatial scale. We realize that there is a continuum in the relevance of applied research, from immediately available to practitioners to the other end of the spectrum where research results challenge prevailing dogma and call for more work (Barlow *et al.* 2016). It may be difficult for researchers working in emerging areas to think of immediate practical applications, but we challenge researchers to consider the broad policy and management shortcomings that can be informed by their work.

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