What Drives Declining Support for Long-Term Ecological Research?

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Several recent papers have reinvigorated a chronic concern about the need for ecological science to focus more on long-term research. For a few decades, significant voices among ecologists have been assembling elements of a case in favor of long-term ecological research. In this article and for the first time, we synthesize the elements of this case and present it in succinct form. We also argue that this case is unlikely to result in more long-term research. Finally, we present ideas that, if implemented, are more likely to result in appropriate levels of investment in long-term research in ecological science. The article comes at an important time, because the US National Science Foundation is currently undertaking a 40-year review of its Long-Term Ecological Research Network.

Keywords: epistemology, science funding

Deep insights about how baboons think (Cheney and Seyfarth 2008), fundamental clues about resilience emerging from networks of interacting species of small mammals (Brown et al. 2001), essential nuance about life histories mediating succession in plant communities (Rees et al. 2001), the critical influence of grazing on restoration of species diversity in grassland ecosystems (Collins et al. 1998)—the insights alluded to in each of those examples were revealed through long-term research of ecological systems (hereafter, *long-term research*). Those examples and many others represent insights that would have been obscured, if not invisible, through the lens of short-term research. These four papers also happen to have been cited more than 2500 times, collectively.

Long-term research is as varied as other forms of ecological research—varied with respect to the systems and concepts to which it attends. Long-term research includes research in lakes (Magnuson et al. 2005), forests (Foster and Aber 2006), grasslands (Knapp et al. 1998), deserts (Havstad et al. 2006), marine environments (Ducklow et al. 2013), urban environments (Grimm et al. 2000), and more. Long-term research includes work on ecosystem processes (Kominoski et al. 2018), population ecology (Peterson et al. 2014), and life histories (Clutton-Brock and Pemberton 2004). Excellence in long-term research is distinguished from excellence in other forms of ecological research, quite straightforwardly, with respect to the duration of the study period. The difference is simple, although long-term research is difficult to implement and is apparently able to produce results that are otherwise unattainable.

Juxtaposed against that positive impression is a concern that funding for long-term research of ecological systems by the National Science Foundation (NSF) declined in absolute and relative terms throughout a dozen-year period during the early part of the twenty-first century (Hughes et al. 2017). As funding for short-term ecological research increased by approximately 70%, funding for long-term research declined by around 60%. The decline occurs in spite of champions for long-term research making a persistent case to increase funding for long-term research. In the present article, we review and critique the case for increasing support for long-term research. The review is timely, in part, because the NSF is currently undertaking a 40-year review of its Long-Term Ecological Research Network of 28 research sites. Since 2010, 11 of the network's sites were put on probation, and 4 others were terminated, raising concerns about the future of the network and long-term research in general. That circumstance has caused some within the Long-Term Ecological Research community to wonder if NSF staff are less compelled to support existing long-term ecological research programs given their widely noted value (S. L. Collins, Personal Communication). Although elements of the present article refer to a particular funding institution (the NSF), the principles are applicable to long-term ecological research far beyond that institution.

One point in the case for increasing support to long-term research is that it offers distinctive contributions to the growth of ecological knowledge (box 1). This point has been made on multiple occasions over the past four decades (e.g., Callahan 1984, Franklin et al. 1990, Magnuson 1990, Hobbie 2003, Lindenmayer et al. 2012). The distinctive contribution

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Box 1. A summary of the traditional case for increasing support for long-term ecological research.

1. Contributes distinctive knowledge—slow processes, complexity, variability, ecological surprises
2. Disproportionate contribution to growth of scientific knowledge
3. Disproportionate contribution to development of policy
4. Appreciated across the community of ecologists—not merely among those who most participate in long-term ecological research

of long-term research to ecological knowledge is well represented by three key phrases: slow processes, variability, and complexity. That is, long-term research is distinctive for its ability to explain processes that unfold slowly over the course of ecologically relevant scale of time (i.e., decades to centuries), such as the population dynamics of long-lived organisms (e.g., Douglas fir trees, sea turtles); the "normal" range of variability of an ecological process; and complex processes, especially those resulting from multiple and interacting causal factors. More recently, long-term research has also been acknowledged for its distinctive capacity to document and explain ecological "surprises" (Doak et al. 2008, Lindenmayer et al. 2010, Anderson et al. 2017), which can arise from tipping points that result in regime shifts between alternate stable states and the legacies of unexpected historically contingent events.

A second point in the case for increasing support of long-term research is its disproportionate contributions to the growth of ecological knowledge. This value has also long been proffered and was recently quantified. Specifically, scientific journals with the greatest impact factors tend to publish a greater portion of papers whose subject is long-term research (Hughes et al. 2017).

A third point in the case for increasing support of long-term ecological research is its disproportionate contribution to the development of policy. This value of long-term research, like the second, has also long been proffered and was recently subjected to empirical evaluation: Reports from the US Natural Research Council (NRC), which tend to be especially influential for policy, rely more on the findings of long-term research than other kinds of articles published in peer-reviewed journals (Hughes et al. 2017). Furthermore, support for this claim comes from a survey of authors of NRC reports, which confirm that greater reliance on long-term research was a conscious decision, reflecting the authors' beliefs that long-term research provides especially valuable insight for policy (Hughes et al. 2017).

A fourth point in the case for long-term research is that its value is appreciated across the community of ecologists—not merely among those who most participate in long-term ecological research. This point stands in contrast to a (misplaced) concern that calls for increasing support of long-term research are made primarily by those who would most benefit from the increased support. Evidence for the misplaced nature of this concern includes Kuebbing and colleagues (2018), who surveyed ecologists—primarily those who do not receive funding for long-term research and found, for example, "almost 80% ($N = 936$) of respondents said that long-term experiments had contributed 'a great deal' to ecological understanding" (Kuebbing et al. 2018). They also asked an open-ended question: *What are the primary incentives for conducting long-term research?* And they analyzed responses using content analysis, and identified these themes as being most frequent: the personal satisfaction of learning more about the studied system, the positive professional reputation that accompanies conducting long-term research, and increased funding (this last theme can be interpreted as lack of funding being an obstacle and increased funding being an incentive). A third of respondents mentioned at least one of these themes.

Although some of these four points have been recently quantified, other points have been advanced since the mid-1980s (e.g., Callahan 1984). The decline in funding for long-term research—despite the case for its value—warrants conjecture about why support for long-term research continues to decline. To that end, we offer the following observations.

Long-term ecological research is the result of a recursive network of social processes

Insight might come from studying long-term research as a behavior that researchers engage in (or not) to varying degrees; the decisions of funders are only part of the influence on that behavior. Figure 1 depicts a conceptual model of factors that might influence researchers' behavior. The central spine of the model indicates that implementation of long-term research is a behavior exhibited by researchers (1 in figure 1) and that behavior is strongly influenced by funding processes that include both funding priorities and review processes (2 in figure 1). The model indicates that researchers' behavior is also likely influenced by priorities set by agencies charged with managing human–environment relationships (3 in figure 1), the priorities of university administrators and promotion and tenure committees (typically composed of fellow scientists; 4 in figure 1), norms in scientific publishing that are characterized by increasing pressure to publish at ever greater rates (5 in figure 1), and researchers' perceptions of the costs and benefits of long-term research (6 in figure 1). The model indicates that researcher's engagement of long-term research (1 in figure 1) is influenced by a feedback loop that involves the *perceived* benefits of long-term research to the general public (7 in figure 1), resultant political support (of a lack thereof; 8 in figure 1), and the effect of that political influence on

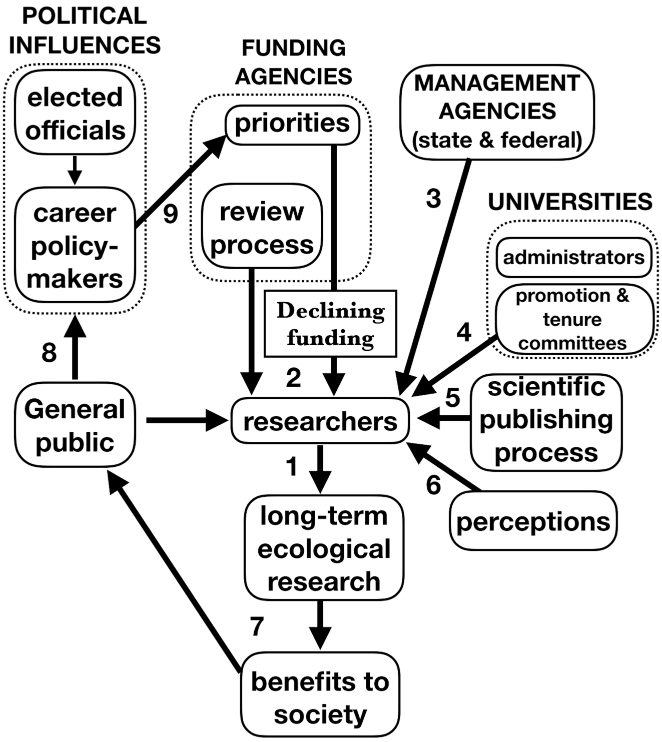

Figure 1. The traditional argument for increased support for long-term ecological research (box 1) does not take adequate account of the factors that influence whether an individual researcher decides to become more involved in long-term ecological research. Some of these additional influences are depicted in the present article.

priorities set by funding agencies (9 in figure 1). The model is only intended to serve as a heuristic to generate discussion concerning what the influences might be and to imply how one might study those influences.

Provisional insight may rise from merely comparing figure 1 with the existing case for long-term research (box 1). For example, if point 3 of box 1 were broadly and simply accurate, then policy-makers and those who determine funding priorities would—on the whole—be powerfully positive forces in favor of increased support for long-term research (figure 1). In truth, their support is not sufficiently positive to result in increased (or even stable) funding for long-term research. If point 4 of box 1 were broadly and simply true, then promotion and tenure committees—who steer and determine the professional fates of younger researchers—would seem to be powerful forces for increased support (in the form of participation, not funding) for long-term research. In truth, these committees are likely not such positive influences on long-term research. We hasten to add that these claims represent our intuitions after reflecting on box 1 and figure 1. In truth, these are empirical claims that can be subjected to evaluation using methods commonly employed by social scientists to study judgments, decisions, and behaviors. Without systematic empirical inquiry, we risk misapprehending the types of interventions capable of resulting in long-term research that better serves society and is better supported by researchers and funding agencies.

Figure 1 also suggests the value of expanding the focus of concern from funding for long-term research to include support for long-term research that includes funding, as well as the portion of ecologists who are deeply engaged with long-term research. To illustrate the concern, 60% of 1179 surveyed ecologists self-reported participation in long-term research (Kuebbing et al. 2018). If so, then the depth of engagement may be modest, given that, for example, a much smaller portion of authors in peer-reviewed journals are writing papers in which they report on long-term research.

Finally, the political influences depicted in figure 1 include bureaucratic games that dissuade researchers from continuing long-term research. For example, Charles Keeling experienced repeated attempts by government agencies to relieve him of research on the measurement of atmospheric carbon dioxide, which would have resulted in a decline in the excellence of that research (Keeling 1998). The unkind politics of long-term research may be a far more important threat to long-term research than is commonly appreciated (see also Nelson et al. 2011, Packer 2015, Smith 2016).

Long-term ecological research may not serve society well enough to inspire increasing support

An answer to the title's question may be found by reflecting on the nascent transformation of restoration ecology, which has largely treated the past as a canonical conservation goal—that is, past states, processes, and ranges of variability of an ecosystem. That goal has been well served by knowledge traditionally produced by long-term research.

The Anthropocene is, however, forcing restoration ecology to transform its vision and long-term ecological research may benefit from doing the same. The restoration ecology of the future will likely be more interested not so much in past states, processes, and variability than in the *history* of a place (Higgs et al. 2014). History (as opposed to the past) emphasizes the importance of making explicit the powerful influence of normative interpretations that always accompany descriptions of the past. This history is critical for creating, maintaining, and restoring ecological justice—that is, just relationships between among humans and with the nonhuman natural world (Vucetich et al. 2018).

If long-term research and its practitioners are to enjoy broad and growing support in the Anthropocene, they might have to contribute to such a vision more explicitly and directly. Doing so would require long-term research to considerably broaden the scope of sought knowledge. The most valuable kinds of knowledge to emerge from future long-term research is likely to be more deeply transdisciplinary (spanning biophysical science, social and behavioral science, and the arts and humanities), to more effectively handle normative considerations associated with the management and policy implications of long-term ecological research (e.g., Vucetich et al. 2019, Batavia and Nelson 2019), and to be more effectively conveyed to much broader audiences (e.g., policy-makers, the public). In addition, and insomuch as long-term research is especially valuable for understanding ecological surprises, the ultimate societal value of long-term research may include evoking the virtue of humility. Lack of humility in our relationship to and understanding of nature is arguably a root cause of the most pressing environmental problems (e.g., Holling and Meffe 1996).

The prospect that long-term research might be (or should be) valued for its potential to inspire humility in our relationship with nature raises questions. For example, how might one evaluate the potential for long-term research to generate humility? The question resembles concern about the NSF's ongoing developmental pains associated with its broader impacts criterion (BIC) for proposals, mandated by the US Congress as a means of promoting taxpayer-funded science that is of tangibly perceptible benefit to tax payers. Although concern about the BIC is sometimes expressed as difficulty in evaluating the BIC, the root challenge may lie elsewhere. A significant and sincere view among scientists is that the BIC is simply an inappropriate basis for evaluating basic research (Holbrook and Frodeman 2007). Some are appreciative of the BIC but lack the skill for understanding and implementing the BIC, and others may underattend the BIC because they believe that best matches the disposition of peer reviewers (Nadkarni and Stasch 2013, Watts et al. 2015).

If one of the richer values of long-term research is its potential to generate humility, then the challenge will be less how to evaluate it and more how to foster the interest and capacity to do so. The difficulty of fostering that capacity may be that virtues such as humility are not especially valued in our society (Brooks 2015). If so, then examples of channeling the results of long-term research toward a humble human–nature relationship might be valuable. Such examples might include Carson (1965), Kimmerer (2003) and Vucetich (2010).

Returning to the heart of the matter, the traditionally recognized products of long-term research (point 1, box 1) currently appear insufficient to inspire increasing support for long-term research. And if nascent transformation in restoration ecology is any indication, the traditional products of long-term research may not even be what society needs most from ecologists.

We may not understand well enough how long-term ecological research really works

Elite artists and athletes may perform their skills without knowing precisely how or why their efforts are so productive. In the same way, the existence of excellent long-term research does not necessarily imply that we, as a community of ecologists on the whole, adequately understand certain critical elements of *how* long-term research works. For example, some researchers may be dissuaded from developing long-term research because they believe their professional success depends on publishing papers at a frequency that is precluded by the presumably slower pace of publication for long-term research (Kuebbing et al. 2018). That belief presumes that short-term research sits in opposition to long-term research. Our understanding and experience indicate that short-term research is nested within long-term research and does not hinder publication rates.

At a more fundamental level, we may not understand what might be called the *epistemology* of long-term research—that is, the distinctive elements of long-term research that result in its distinctive contributions to the growth of ecological knowledge. Consider, an example: From six decades of research on the wolves and moose of Isle Royale, two of the more significant findings were the result of chance observations and not a result of testing *a priori* hypotheses. More specifically, *ad hoc* analysis of a chance observation of an immigrant wolf led to significant insight about genetic rescue (Adams et al. 2011). Also, *ad hoc* analysis of data gathered for a couple decades before and after the inadvertent introduction of a wolf disease (canine parvovirus) led to significant insight on temporal fluctuations in the strength of top-down forces in a food chain (Wilmers et al. 2006). Compare those contributions with the obsessive attention given (especially in funding proposals) to motivating research from *a priori* hypotheses.

A priori hypotheses are obviously essential, on the whole, to the growth of knowledge. But knowledge may not grow best from every funding proposal being so intimately tied to *a priori* hypotheses. In the same breath, excellence in long-term research is not *merely* detailed, systematic, and enduring observation.

Consider Keeling's observations that atmospheric levels of carbon dioxide have been increasing for the past six decades (Keeling 1998). Consider also long-term research on animal populations in the Serengeti, which has taught much about the regulation of mammalian communities with special attention to the influence of predation and competition among herbivores (Sinclair et al. 2009). These are examples of excellence in long-term research. That assessment follows easily from the results of those projects. The far more difficult assessment is what *general* features allowed those programs to be excellent. We, the coauthors of the present article, could resolve to monitor birds in our neighborhood forests and resolve to do so in a detailed, systematic, state-of-the-art, and enduring manner. Essential as those properties are, they are inadequate if we aspire to even approximate the excellence found in, for example, research on Darwin's finches in the Galapagos (Grant and Grant 2014). What are the *general* features of excellent long-term research, especially when long-term research is not driven by *a priori* hypotheses or formal experiments—the other vehicle of growth in knowledge with a history of overemphasis in ecology (Likens 1983, Taylor 1989)?

We caution against presupposing the answer, in part, because of explanations epistemologists have offered for why and to what extent experiments and various means of testing *a priori* hypothesis have contributed to the growth of (ecological) knowledge (e.g., Taper and Lele 2004, Rosenberg 2011). In particular, these explanations are sophisticated and still debated by epistemologists. Inquiries about the epistemology of long-term ecological research deserve the same kind of rigor. Although pursuing that rigor is beyond the scope of this article, the worthwhile point is that knowing the general principles that favor excellence in long-term research may be extremely valuable to funders, peer reviewers, and grant seekers. Not knowing the general principles is likely to prevent younger scholars from ascending to leadership positions in long-term research, except for those who can figure it out intuitively.

Finally, there may be value in drawing an analogy between long-term research and elements of healthy living (diet, exercise, meditation). In both cases, we know the behavior is objectively good and socially desirable. However, we find it difficult to engage in either behavior. Competing interests and behavioral defaults are often to blame. With respect to long-term research, the competing behaviors may include an undue obsession with securing the most research funding in the fastest possible way and the most publications in the shortest time. Making those outcomes desirable for their own sake may out compete the interest to pursue something newer and less familiar, such as long-term research.

Continuing to make the case summarized in box 1 without some new element or perspective is unlikely to result in increased support for long-term research. We do not know what that new effort or perspective is, except what the thoughts raised in the present article might point to.

Life is replete with cases of individuals and organizations making decisions that do not align with their values. Long-term ecological research may be another case. Decision science indicates that such cases may be aided by at least two policy-relevant ideas:

First, agencies allocate research funds to broad categories (such as long-term research) according to formal and transparent application of decision-aiding processes designed to help align one's decisions with their expressed values (e.g., structured decision-making; Gregory et al. 2012). Second, universities establish incentives for researchers who incorporate long-term research into their annual and 5-year academic plans and then substantively engage those plans with the nurturing of their supervisors and promotion and tenure committees.

Although they are important, these policies may be insufficient, because long-term research is also likely limited by a culture of science that is too often, too attentive to erudite novelty and insufficiently able to relate to the rest of society. This culture of science includes funders of science, administrators of science, practitioners of science, intended beneficiaries of science and relationships among those parties (figure 1). The present article is not assigning blame for failure to grow (or even maintain) long-term research but is, rather, a call to more creatively understand actions that might foster its growth. That problem is not limited to long-term ecological research, although it may be a kind of canary in the coalmine that is more sensitive to those obstacles than are other kinds of science.

Over the past several decades, a number of important papers have been written by well-regarded scientists making strong arguments for long-term research. We summarize the basic orientation of their arguments (box 1). The straightforward premise of the present article is that these arguments appear to not be working. A contribution of this article is to point out that repeating those arguments may not be enough. Our aspiration in the present article is to inspire the science community to think with strategic creativity about more elusive obstacles that limit the quantity of excellent long-term research—whatever those obstacles may be.

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