Seven myths about causal inference in ecology: clarifications and common ground

Authors: Laura E. Dee[1*], Paul J. Ferraro[2,3], Katherine J. Siegel[4], Steve J. Miller[5], Jarrett E.K. Byrnes[6], Hannah E. Correia[7], Tyler L. McIntosh[1,4], and Suchinta Arif[8]

Affiliations:

[1] Department of Ecology & Evolutionary Biology, University of Colorado, Boulder

[2] Department of Environmental Health and Engineering, Johns Hopkins University, 3400 N. Charles St., Baltimore, Maryland, USA 21218

[3] Carey Business School, Johns Hopkins University, 3400 N. Charles St., Baltimore, Maryland, USA 21218

[4] Cooperative Institute for Research in Environmental Sciences & Department of Geography, and Environmental Data Science Innovation & Impact Lab, University of Colorado, Boulder

[5] Department of Environmental Studies, University of Colorado, Boulder

[6] Department of Biology, University of Massachusetts Boston, 100 Morrissey Blvd., Boston, Massachusetts, USA 02125

[7] Department of Biology, University of North Dakota, 10 Cornell St., Grand Forks, North Dakota, USA 58202

[8] Department of Biological Sciences, University of New Brunswick Saint John, 100 Tucker Park Rd., Saint John, New Brunswick, Canada, E2L4L5

***Correspondence:** Laura E. Dee, laura.dee@colorado.edu

Abstract

Identifying and quantifying causal relationships from data is a central goal in ecology and conservation. To achieve this goal, ecologists are increasingly applying frameworks and methods from the broader, multidisciplinary causal inference literature. These applications have generated both enthusiasm and skepticism. Here, we address seven common sources of skepticism that we see as myths. Much of this skepticism treats causal inference as a competing research paradigm. However, it is better understood as a set of ideas and practices that can sharpen ecological reasoning and strengthen empirical evidence. The value of causal inference lies in clarifying the assumptions required to interpret statistical associations causally and in encouraging researchers to state these assumptions explicitly, justifying them using prior knowledge and biological reasoning. In this sense, causal inference does not replace what ecologists already do well. Rather, it helps reveal, refine, and communicate the logic already underlying empirical research with causal aims. Across the seven concerns that we examine, a common message emerges: debates about causal inference should focus less on labels and techniques and more on whether the assumptions behind a causal claim are credible, clearly stated, and fit for purpose when making causal claims in ecology.

Introduction

As ecologists and conservation scientists, we seek to understand causal relationships in complex socio-ecological systems, including the mechanisms that underlie cause and effect relationships. One way to advance causal understanding is through empirical approaches, including observational studies and experiments, which draw on and complement theory,

45 natural history, and process-based models and can be interwoven with other ways of knowing
46 (Gazing Wolf *et al.* 2024).

47 Ecologists increasingly draw on concepts and practices developed in other fields for
48 obtaining causal inferences from data. These frameworks clarify the conditions under which
49 correlations can be interpreted causally and provide a common language for performing,
50 communicating, and evaluating causal investigations. Their applications span diverse topics
51 across ecology, behavior, evolutionary biology, and conservation (e.g. Borger & Ramesh 2025;
52 Van Deynze et al. 2024; Dudney et al. 2025; Laubach et al. 2021; MacDonald et al. 2019;
53 Schrodt et al. 2025; Simler-Williamson & Germino 2022), and several causal inference
54 approaches have been summarized in recent publications (e.g. Addicott et al. 2022; Arif &
55 MacNeil 2023; Butsic et al. 2017a; Correia et al. 2025, 2026; Franks et al. 2025; Grace & Irvine
56 2020; Larsen et al. 2019a; Laubach et al. 2021; Siegel & Dee 2025; Wauchope et al. 2021). Our
57 premise is that these approaches can improve causal understanding of ecological systems when
58 used appropriately in conjunction with ecological reasoning and knowledge of study systems.

59 The application of causal inference approaches from outside of ecology, however, has
60 been met with some concern (e.g. Grace et al. 2025). Our own discussions with peers have
61 revealed fears that, while methods from other fields could be useful, those methods could be
62 applied without regard for ecological knowledge, leading to erroneous causal claims. Others
63 view any causal method with suspicion if it is not based on an experimental design, regardless
64 of the field from which the approach originates. Similarly, others feel that advocates for the use
65 of such approaches are dismissive of prior ecological research or alternative methods. Left

66 unresolved, such misunderstandings risk discouraging engagement with methodological
67 advances that can enhance ecological knowledge.
68 Here, we aim to address these concerns and explain why empirical causal research from
69 diverse fields offers opportunities to build on and complement – *rather than compete with* –
70 how ecologists already do empirical research. Causal inference approaches are not a statistical
71 panacea: they cannot yield credible conclusions without knowledge of the study system. Yet,
72 they are also not inherently incompatible with ecological complexity and questions.
73 Additionally, recognizing experiments as the gold standard for causal inference does not
74 preclude the careful, transparent, and biologically informed use of causal analyses with
75 observational data.

76 We organize our discussion around seven common concerns that have surfaced in
77 teaching, peer review, and a couple recent publications (Grace 2024; Grace *et al.* 2025). In Box
78 1, we summarize those concerns as a set of contrasting Myths and Clarifications, and then we
79 elaborate on these clarifications in the rest of our essay. Throughout, we emphasize that causal
80 inference is not a fundamental departure from, nor an attempt to reinvent, what ecologists
81 already do; it formalizes and makes explicit reasoning that ecological research has long relied
82 on. Our aim is not to advocate for a single method (*sensu* Wolkovich et al. 2026, page 6). In fact,
83 **causal inference is not a single method but rather a framework for structured thinking.** Using
84 a demonstrative example of wildfire and forest recovery (Figure 1), we illustrate some of the
85 many pathways to causal understanding that leverage approaches from the causal inference
86 literature and are used as part of an iterative, scientific process. We hope our clarification
87 contributes to a more productive and transparent conversation about causal claims in ecology,

88 while encouraging ecologists to engage more deeply with recent advances in empirical causal
89 research.

90

Box 1. Seven Common Myths and Clarifications about Causal Inference Approaches

*Myth 1***.** The causal inference literature is solely about a limited set of statistical methods.

Clarification: The causal research literature provides a unifying framework for posing and answering causal questions. Rather than just describing statistics or a particular method, it provides a way of thinking about the assumptions that allow one to make specific causal claims from data.

*Myth 2***.** Experiments automatically permit causal claims, but observational studies cannot.

Clarification: Causal claims from experimental data and observational data rely on variations of the same sets of untestable causal assumptions, and thus the validity of causal claims depends on the plausibility of these assumptions in a study context rather than whether the design is experimental or observational.

*Myth 3***.** Causal inference approaches are applied independent of mechanistic ecological knowledge.

Clarification: Causal claims in ecology, regardless of the method, require mechanistic knowledge of the study system, and the stronger that mechanistic knowledge, the more credible are the causal claims.

Myth 4. Causal inference approaches cannot contribute to new mechanistic knowledge.

Clarification: Causal inference approaches can create new mechanistic knowledge through the quantification of causal relationships, through mediation analyses that decompose causal relationships into their constituent pathways, and through moderator analyses that elucidate the heterogeneity of causal relationships across contexts.

Myth 5. Causal inference approaches require more demanding assumptions than other empirical approaches in ecology.

Clarification: The assumptions required to make causal claims from data are not unique to causal inference approaches; every empirical approach used in ecology relies on them. What differs is that causal inference states these assumptions explicitly, whereas mainstream approaches simply leave them implicit.

Myth 6. Causal inference approaches are limited in the types of causal relationships they can elucidate and the range of empirical settings in which they can be applied**.**

Clarification: Ecologists are applying these approaches to diverse causal questions, contexts, and data structures at an increasing rate.

Myth 7. Causal inference approaches require perfection.

Clarification: Causal inference requires clarity and transparency, not perfection.

91

1. CLARIFICATION 1: Causal inference is a framework for clarifying causal questions and assumptions not a set of statistical methods

One common concern is that researchers adopting causal inference are advocating for a specific set of statistical methods, suggesting that experiments are no longer a pathway towards causal knowledge, or attempting to replace the incorporation of *a priori* biological and mechanistic knowledge with statistical tools. This is not the case.

Causal inference is not a set of statistical techniques but rather a set of frameworks that share common principles: transparently stating assumptions, systematically eliminating rival explanations, and defining conditions under which causal claims are warranted. This overarching set of principles provides a structured approach for understanding how causal effects and their mechanisms can be defined, estimated, and interpreted – systematically, transparently, and in concert with existing knowledge (see Figure 2). Rather than aiming to replace ecological understanding with automated methods or statistical techniques alone, causal frameworks force us to think about the natural history of an ecological system and state our hypotheses about how the world works before deciding on a methodology to estimate causal effects. In this way, causal frameworks align with best practices in ecological research and provide processes for adhering to them.

To elaborate, a central premise in causal inference is that data never speak for themselves (Correia *et al.* 2026): data alone cannot identify causal relationships. Different underlying processes and causal structures can generate the same patterns in data (Anscombe

1973; D'Agostino McGowan *et al.* 2024; Matejka & Fitzmaurice 2017). We might never know the true underlying data-generating process, meaning the underlying mechanisms, system, or set of rules that create the data we observe. The data come from a combination of known and unknown variables, relationships, parameters, and inherent randomness. However, we can interpret these data by combining it with causal assumptions justified by natural history, domain-specific theory (including conceptual and mathematical models), and prior studies.

A core challenge with empirical causal research is that we face many rival explanations – alternative, often non-causal explanations – for relationships observed from data. Rival explanations can arise from sampling variability, unmeasured confounding variables (Addicott *et al.* 2022; Dee *et al.* 2023), interference among units (Rosenbaum 2012), model misspecification, measurement error, colliders (Arif & MacNeil 2023), and more. Rival explanations exist in experiments too (see Clarification #2 and Kimmel et al. 2021). *Any* of these rival explanations can obscure true causal relationships or result in spurious estimates of true relationships, leading to inaccurate conclusions about the sign and magnitude of causal relationships. For instance, if we observe a relationship between fire severity and forest regeneration, making a causal claim would involve ruling out that this observed relationship was due to other non-causal reasons, including measurement error of forest cover or greenness from satellites (Proctor *et al.* 2023) or confounding effects of topography or drought, which affect both fire severity and subsequent recovery potential (Littell *et al.* 2016; Stevens-Rumann *et al.* 2018).

Grappling with, and attempting to systematically rule out, rival explanations is foundational to causal inference and interpretation. Causal inference provides a useful, unified

vocabulary to discuss these rival explanations. This clarity of language can clarify concepts for students while enabling inter-researcher and inter-disciplinary transparency.

In summary, causal inference frameworks are bound by a set of key principles, which include leveraging existing ecological knowledge, transparently working with assumptions, and systematically ruling out rival explanations. These principles can be applied using various methodologies and research designs, giving ecologists the freedom to select approaches that best align with their causal question(s) and data, and they do not exclude the value of experiments (e.g., the Rubin-Neyman model is itself rooted in experimental design thinking, extended to observational settings; Rubin 1974, 2005). Said another way, adopting causal inference frameworks does not mean advocating for a single method or research design, specific statistical methods, or a single thought process.

In contrast, Grace (2024) and Grace et al. (2025) refer to this entire body of work as "*causal statistics*," a term coined to contrast it with their "*mechanistic investigations*" (i.e., studies that characterize the structures and processes through which causes produce effects). We find the label misleading: it suggests a set of statistical techniques that are incompatible with understanding processes (see Clarification 3), when what unifies these approaches is a way of reasoning about assumptions and alternative explanations (Rubin 1974, 2005). Rather than replacing best practices in ecological research, causal inference offers added opportunities and formalized processes for robust investigation of causal relationships, across a wide range of ecological contexts.

2. CLARIFICATION 2: Causal claims are not limited to experiments but depend on the same untestable assumptions in both experimental and observational designs.

A widespread misconception in ecological research is that experiments automatically yield causal estimates, whereas observational studies cannot. However, all research designs rely on causal assumptions, including randomized controlled experiments (reviewed in Kimmel et al. 2021 and more generally in Correia et al. 2026).

To illustrate, one consideration for experiments is whether the treatment itself affects an outcome of interest through pathways other than the cause of interest that is attempting to be manipulated. The key assumption, called "excludability," is that it does not (see Kimmel et al. 2021). As an example, consider an experiment using rainout shelters to simulate drought with the intent of studying the effects of drought on productivity. These rainout shelters could also alter light, temperature, or wind conditions, which means that the observed ecological responses may reflect more than just the effect of reduced precipitation. If so, the excludability assumption would be violated, and we cannot interpret the differences in productivity between sheltered and unsheltered plots as necessarily reflecting a causal relationship between drought and productivity. Researchers can use "procedural controls" to detect, quantify, and correct for the magnitude of the excludability assumption violation (Underwood 1986, 1996). Using this approach, for instance, Kreyling et al. (2017) did not find evidence that rainout shelters introduce microclimatic artifacts that affect plant physiology through pathways other than reduced rainfall. In practice, however, final publications of experimental studies in ecology rarely address the validity of causal assumptions explicitly or report on the implications for the

178 study's causal claims if the assumptions were violated, even though these issues were likely
179 considered by the researcher.

180 When a procedural control is not available, or when other untestable causal
181 assumptions besides excludability are in question, causal claims from experiments with
182 potential violations of the excludability assumption rely on the same techniques used in
183 observational studies. These include placebo tests (Ferraro & Hanauer 2014; Naidoo *et al.*
184 2025), estimators that condition on observed or unobserved confounding variables (Dee *et al.*
185 2023), redefining the target causal effect that we seek to estimate to one the design can
186 credibly identify (Kimmel *et al.* 2021a), or sensitivity analyses of the results to varying
187 magnitudes of the potential violation (Andraczek *et al.* 2024; Brumberg *et al.* 2026; Cinelli &
188 Hazlett 2020; Guizar-Coutiño *et al.* 2026). Leveraging these approaches in ecological
189 experimental contexts is rarely done but represents an opportunity to improve inferences from
190 imperfect experiments (reviewed in Kimmel et al. 2021a).

191 From the perspective of the causal inference literature, the line between an
192 "observational" and an "experimental" study is blurry. The key issue is not whether a study is
193 experimental or observational, but whether the assumptions required for causal interpretations
194 are plausible and clearly articulated. These assumptions could be more plausible in
195 experimental studies. Yet, acknowledging experiments as a gold standard for causal
196 identification does not preclude causal claims from observational data when paired with the
197 careful and transparent use of causal assumptions. In many cases, particularly when
198 experiments are infeasible or unethical, observational data offers us the best shot at answering

important causal questions (Figure 1). Well-designed observational studies with plausible, clearly articulated assumptions complement experiments and can also offer valuable a source of causal. Thus, valuing well-designed observational studies does not imply a movement away from valuing experiments.

3. CLARIFICATION 3: Causal inference approaches rely on mechanistic ecological knowledge and theory

A recent paper by Grace (2024) has mischaracterized the broader causal literature proposing that *"there is a key ingredient missing from standard causal statistical analysis, specifically a consideration of preexisting mechanistic knowledge"* (Grace 2024, page 1). Grace et al. (2025) then create a false dichotomy between established causal frameworks and studies that integrate mechanistic understanding. In truth, established causal frameworks rely, to varying degrees, on the integration of pre-existing mechanistic knowledge (Correia et al. 2026; Siegel & Dee 2025; Figure 2). As stated by Pearl (2010), "*such questions require some knowledge of the data-generating process, and cannot be computed from the data alone, nor from the distributions that govern the data."* This knowledge is essential for informing and satisfying the causal assumptions that allow researchers to interpret empirical relationships as causal (see Clarifications #1 and #2).

Carrying out causal analyses without knowledge of the underlying system structure can lead to erroneous causal claims, as the correct causal interpretation depends on understanding how variables in a system are causally connected, including through underlying ecological mechanisms (Cunningham 2021; D'Agostino McGowan *et al.* 2024; Elwert & Winship 2014;

Pearl 2011). Whether it comes from formal theory, prior empirical work, or natural history observations, pre-existing mechanistic knowledge shapes the research question, study aims, hypotheses, study design, interpretation of estimated relationships, and evaluation of a study's credibility (Figure 1).

A clear example of how pre-existing mechanistic knowledge is integrated in causal analysis is in the use of causal diagrams. Causal diagrams draw on pre-existing mechanistic knowledge from diverse sources to visualize the hypothesized underlying causal structure of a system, providing researchers with a formal process for identifying and mitigating confounding and other sources of bias (Bollen & Pearl 2013; Pearl 2009). These diagrams have become a foundational tool across disciplines and allow researchers to transparently communicate their assumptions about how variables may be causally connected within their study system (D'Agostino McGowan *et al.* 2024; Gutierrez *et al.* 2025; Pearl 2009, 2011). Researchers have developed causal diagrams that integrate mechanistic knowledge from various sources, including scientific literature (Ferguson *et al.* 2020), expert elicitation (Mascaro *et al.* 2023), and community voices (Marziali *et al.* 2026). Once generated, these diagrams help researchers assess whether causal assumptions can be plausibly satisfied and guide the development of causal analyses, for example by quantifying causal effects from observational data (for biological applications, see Arif & MacNeil 2023 and Laubach et al. 2021).

Mechanistic knowledge shapes not only how researchers construct causal diagrams, but also how they select and apply causal methods more broadly. Whether or not causal diagrams are used to codify prior knowledge, quasi-experimental designs make explicit use of existing mechanistic knowledge (reviewed in Butsic et al. 2017b; Byrnes & Dee 2025; Larsen et al. 2019;

243 Siegel & Dee 2025). For example, consider a study seeking to quantify the effect of bark beetle
244 outbreaks on forest structure. Methods based on propensity scores leverage mechanistic
245 knowledge about the determinants of bark beetle occurrence to develop a model of the
246 treatment assignment process, which allows the researcher to control for confounding
247 variables that affect both bark beetle occurrence and forest structure. In contrast, methods
248 based on regression discontinuity designs leverage knowledge that bark beetle occurrence
249 changes abruptly at a specific threshold value of another variable (e.g., at administrative
250 boundaries where different management practices create sharp differences in beetle densities
251 on either side), and whether other ecological factors also change at that same threshold in
252 ways that could independently influence forest structure (Imbens & Lemieux 2008). The
253 mechanistic knowledge that underlies these studies is not part of the statistical methods
254 themselves but are an important part of satisfying the method's causal assumptions (see
255 Clarification #1).

256 Proposed alternative paradigms using mechanistic knowledge to impart causal
257 interpretation to statistical relationships in Grace (2024) and Grace et al. (2025) are not actually
258 different from this integration of prior mechanistic knowledge into the assumptions and design
259 of causal analyses. In fact, the mechanistic-investigation example in Grace et al. (2025) and
260 (2026) of mangrove-freeze responses (based on Martinez et al. 2023) actually applies a quasi-
261 experimental, difference-in-differences design without referring to it that. Thus, the recent,
262 false dichotomy between "*causal statistics*" and "*causal mechanistic investigation*" coined by
263 Grace et al. creates an unnecessary division that can lead to more confusion than clarity. We

elaborate on why we view this distinction as a false dichotomy in *Supporting Information 1: Mechanistic investigations as causal inference exercises*.

When prior knowledge is more limited, causal discovery methods can be applied to generate plausible causal graphs from data (Figure 1; Figure 2; Correia et al. 2026). Causal discovery methods use patterns of statistical independence and dependence among variables to identify potential causal relationships from data (Glymour *et al.* 2019; Spirtes *et al.* 2001), including for dynamic, non-linear systems (Sugihara *et al.* 2012), or in the presence of latent confounding (Richardson & Spirtes 2002; Spirtes et al. 2001). Many causal discovery methods exist and more continue to emerge; like other causal methods, each operate under a set of causal assumptions and data requirements (Correia *et al.* 2026; Glymour *et al.* 2019; Runge *et al.* 2019; Spirtes *et al.* 1993). Causal discovery still relies on a researcher's pre-existing mechanistic knowledge to assess the credibility of causal assumptions (e.g., whether unmeasured confounders are assumed absent or allowed for, with corresponding implications for the strength of causal claims drawn); to constrain the possibilities of causal relationships; and to interpret and refine the resulting causal diagram (Correia *et al.* 2026). While causal discovery methods relax some assumptions required in causal inference (e.g. the causal graph is known), they provide weaker conclusions: they identify the existence of relationships rather than quantify them. In other words, causal discovery tackles a different causal task than causal inference: detecting the existence of plausible causal pathways, rather than estimating their magnitude (reviewed in Correia et al 2026). Because causal discovery primarily focuses on detecting the existence of plausible causal pathways rather than estimating their magnitude without bias, the mechanistic knowledge a researcher brings to the task is especially crucial, as

it determines which proposed relationships are ecologically plausible and which can be ruled out before the data are even examined (Correia et al. 2026). When prior knowledge of a system or mechanism is limited, initial descriptive studies (e.g. systematic observation, descriptive statistics) remain essential (Figure 2).

As D'Agostino McGowan et al. (2024) put it, causal research techniques have always depended on pre-existing mechanistic knowledge and are *"not just a statistics problem."* Thus, causal inference should not be misrepresented or reduced to "*causal statistics."* Instead, researchers should continue to transparently leverage pre-existing mechanistic knowledge, pairing it with varied causal inference approaches and datasets (Figure 2), to strengthen our causal understanding of the natural world.

4. CLARIFICATION 4: Causal inference approaches can generate new mechanistic knowledge

A related concern is that approaches from causal inference can only estimate the size of an effect and cannot generate new knowledge about the mechanisms that produce it. This undersells what these approaches can do.

In a recent paper, Grace et al. (2025) introduce the term "*causal statistics*" for the existing causal inference literature and contrast it with what they call "*causal mechanistic investigations*," presenting the two as "*Traditions With Different Requirements and Contributions Towards Causal Understanding"* in their title. They emphasize that "*statistical and mechanistic approaches do not necessarily yield similar kinds of information"* (page 4), advocate that "the tenets of causal statistics and the apparent conflict with the norms in the natural

sciences" (page 2), and that causal effects estimates *"by themselves tell us little about the underlying processes"* (page 5), implying that approaches from causal inference are inappropriate for advancing our knowledge of ecological mechanisms. This perspective creates a false dichotomy that may unnecessarily limit the approaches that ecologists consider, potentially leaving mechanistic knowledge on the table. Indeed, the study from which Grace (2024) and Grace et al. (2025) draw their mechanistic example (Martinez et al. 2023) itself pairs that mechanistic analysis with a quasi-experimental analysis quantifying the causal effect of the freeze event on mangrove cover (an analysis Grace presents as an instance of "causal statistics" rather than recognizing it as evidence that the two traditions are complementary), illustrating precisely the false dichotomy we argue against here (**see Supporting Information SI1**).

Approaches from causal inference can serve many different goals, one of which is to improve our understanding of ecological mechanisms (e.g., Correia et al. 2025; Morgan & Winship 2015; Wiens et al. 1993). Causal mediation analyses advance mechanistic knowledge by quantifying the processes through which causal effects arise and decomposing total causal effects into their direct and indirect pathways via mediating variables (Fairchild & MacKinnon 2009; Imai *et al.* 2010, 2012; Imai & Yamamoto 2013; Morgan & Winship 2015; Shpitser 2013; VanderWeele 2015, 2010). Mediation analysis offers a flexible suite of approaches that are appropriate for both ecological and management-focused questions (reviewed in (Correia *et al.* 2025). Here, too, causal assumptions are essential because a main goal is still to identify and eliminate rival explanations for observed correlations (Correia *et al.* 2025; Pearl 2011; Richiardi *et al.* 2013; Robins & Greenland 1992; Shpitser 2013).

Across all these approaches, the research questions and associated research designs determine whether a study can contribute mechanistic knowledge, not a specific method. A study asking '*how large is this effect overall?'* will not necessarily generate mechanistic knowledge, while one asking *'through what processes does the effect operate?'* is more likely to do so. This is essentially the distinction made in (Tilman 1987), who challenged whether his peers' experiments could test for the presence of interspecific competition. Tilman certainly did not discard experiments wholesale; instead, he challenged specific research designs (the choices of treatment and outcome variables). The same logic applies here: ecologists using tools from causal inference *can* generate mechanistic knowledge, but it is the specific research question and design that will determine whether a study does so.

Still, we acknowledge that a better understanding of the effects of individual causes and their mediators (i.e., local causal dependencies) might not be sufficient to build a complete mechanistic theory of how an effect arises (i.e., a "mechanism" in the sense of (Craver & Darden 2013), but we believe this understanding is a necessary condition for progress towards that goal.

5. CLARIFICATION 5: Causal inference requires assumptions shared with all empirical approaches for causal questions in ecology

An emerging concern about causal inference approaches is that they demand satisfaction of *"an extensive list of restrictions"* for verifying assumptions, characterized as a "*perfection standard*" (Grace et al. 2025, pages 2 and 4), rather than allowing expert judgment about bias

based on mechanistic knowledge. This characterization mischaracterizes the goal of causal inference. The aim is not perfection in verifying assumptions, but transparency in stating them so that scientists can systematically assess their plausibility based on domain knowledge. Every research design makes causal assumptions, even carefully designed experiments (Kimmel *et al.* 2021b). Newer-to-ecology tools from causal inference do not introduce new assumptions where none existed before but rather make explicit the causal assumptions implicit in other commonly used techniques and encourage researchers to transparently present, probe, and justify them based on existing knowledge.

To illustrate, Structural equation models (SEM), widely used in ecology to evaluate hypothesized causal pathways (Grace *et al.* 2010, 2012; Shipley 2004, 2009), are a are a clear example of a familiar ecological tool that already relies on demanding causal assumptions (reviewed in (Bollen & Pearl 2013; Grace *et al.* 2012; Grace & Irvine 2020; Shipley 2004), whether or not researchers articulate them. Assumptions to guarantee unbiased estimates in SEMs include, but are not limited to, (1) all relevant causal relationships ("paths") among included variables are correctly specified; (2) no important causal pathways are omitted; (3) no causal relationships are specified between variables that are not causally connected; (4) all paths through which *X* influences *Y,* whether direct or through mediating variables, are correctly and completely specified; (5) unobserved confounders are accounted for via specified correlations or other appropriate techniques (e.g. latent variables); and more (assumptions reviewed in Bollen & Pearl 2012; Pearl 2014). When these assumptions are violated, the estimated relationships could differ from true causal relationships in both magnitude and sign (Bollen & Pearl 2012, 2013; Grace *et al.* 2012; Grace & Irvine 2020; Shipley 2004). Studies using

SEM in ecology, however, rarely articulate these assumptions (*but see* Lefcheck & Duffy 2015: Appendix B) or assess how plausible violations could alter the study conclusions.

SEM is not unique in this regard: all approaches to causal analyses make assumptions. The broader causal literature has clarified the assumptions required for interpreting a relationship estimated from data as causal across a wide range of contexts and data structures. Expert knowledge, broadly defined, is then needed to assess the plausibility and validity of those assumptions for a particular study or context. This literature provides a principled approach and set of tools that can help ecologists more explicitly align research questions, assumptions, and analytical strategies when designing studies for observational data and experiments (e.g., Gerber & Green 2012; Kimmel et al. 2021). The suitability and comparative advantages of each design depend on the research question, aim, data availability, plausibility of meeting the approach's assumptions, and the extent of prior domain knowledge. In this light, we do not view any single statistical approach as universally superior for causal inference (e.g. two-way fixed effects vs. SEM vs. instrumental variable designs).

In summary, rather than imposing new burdens on ecological research, the causal inference literature provides tools for making visible the assumptions that were always there, so that ecologists can assess, justify, and communicate their causal claims transparently.

6. CLARIFICATION 6: Causal inference approaches apply across diverse questions, contexts, and data structures

Causal inference methods are being applied to diverse questions and applications, yet some ecologists claim that causal inference approaches are '*too restrictive'* for characterizing ecological relationships, arguing that studying continuous treatments or pooling data across sites places ecological research "*outside the conditions required for strict counterfactual comparisons*" (Grace et al. 2025, page 10). These concerns are unfounded. (Grace *et al.* 2025)

The suite of causal inference approaches can collectively, and flexibility, meet the diverse needs of many ecological analyses. Already, these approaches are being used to address a wide variety of questions across many different systems, scales, data structures, and aims, including attribution of biodiversity loss (Van Deynze *et al.* 2024; Schrodt *et al.* 2025), climate change (Dudney *et al.* 2021, 2025; Suskiewicz *et al.* 2024), restoration and conservation (Andam *et al.* 2008; Fick *et al.* 2021; Joppa & Pfaff 2009; Pfaff & Robalino 2012; Simler-Williamson & Germino 2022); disease dynamics (e.g. Macdonald & Mordecai 2020), animal behavior (Borger & Ramesh 2025; Laubach *et al.* 2021), fire ecology (Siegel *et al.* 2022), and biodiversity-ecosystem functioning (Andraczek *et al.* 2024; Dee *et al.* 2023). Exciting applications to new questions and data structures are emerging each month (e.g., Boyd et al. 2026; Kaiser et al. 2025; Tabell et al. 2026).

Consider first the objection of Grace *et al.* (2025) that causal inference tools cannot be applied to data pooled across study sites (Grace et al 2025, pages 9-11). In fact, many of the research designs underpinning these tools are specifically designed to address the confounding that arises when data are pooled across sites with different background conditions. Three quasi-experimental techniques illustrate this, using fire as an example. Propensity score

approaches directly model how fire exposure relates to other site characteristics so that confounding effects of site differences can be removed through matching, reweighting, or regression adjustment (Butsic et al. 2017; Stuart 2010). Site effects in difference-in-differences designs adjust for static site characteristics, so that estimation relates changes in outcomes to changes in fire characteristics (e.g. Wauchope et al. 2021). Spatial regression discontinuity approaches leverage spatial discontinuities in fire exposure at fire boundaries, comparing outcomes at adjacent locations within sites so that multi-site datasets can be combined without cross-site differences confounding the comparison (Imbens & Lemieux 2008).

The second objection, that causal inference tools are restricted to binary treatments (Grace et al 2025, page 10) is equally unfounded. Indeed, a large body of literature specifically addresses the use of such methods with continuous and categorical treatments (e.g. Chen et al. 2025; Kazemi & Olsen 2024; Lee et al. 2025). Figure 1 illustrates the range of questions these tools can address, using forest recovery following fires of different severities as a motivating topic. Causal inference tools can be used to study the effects of binary (fire or not), continuous (fire severity), or multi-valued (low, medium or high severity) treatments. Depending on the question, estimation can focus on far more than a single average treatment effect. Ecologists can study heterogeneity using conditional average treatment effects (e.g., allowing effects of fire on recovery to depend on pre-fire drought) or quantile treatment effects (e.g., what is the effect of fire on the 10th percentile of biomass in forests?). Other non-linearities can also be accommodated, increasingly flexibly (Arif 2025; Athey & Imbens 2016). As discussed in Clarification #4, effects can also be decomposed to unpack the effects of mechanisms using mediation analyses.

Moreover, in the presence of uncertainty, ecologists can also estimate bounds on effects using partial identification approaches, which relax the assumptions needed for point estimation of a causal effect in exchange for a less precise range of effect values (Kline & Tamer 2023; Manski 1989). For example, when studying the effects of plantations on pine invasions, (McConnachie *et al.* 2015) assume plantations were placed in areas with greater counterfactual pine cover rather than randomly, replacing an equality assumption (exogeneity) with an inequality (e.g., the conditional mean of the error term is nonnegative). Bounds may also be more practically useful; for example, we may want to know if a pulse perturbation is strong enough to drive a population to local extinction, but beyond a critical threshold the exact effect strength is less important.

As these examples illustrate, causal inference tools can be applied to data spanning many different empirical conditions (e.g., from different sites in ecological networks) and to estimate many different types of relationships (e.g. bounds, heterogeneous treatment effects), which is a far wider range than restrictive characterizations of causal inference suggest (Grace 2024; Grace et al 2025).

7. CLARIFICATION 7: Causal inference requires clarity and transparency, not perfection

Grace et al. (2025) claim that causal inference requires "*a perfection standard*" (page 4), i.e., assumptions that are impossible to meet in practice, and acceptance of this perfection standard "*places many scientific questions of importance for understanding ecological systems off limits to causal investigation"* (page 12). We disagree with this characterization, and with the alternative "*useful approximation standard"* proposed in place of causal assumptions (Box 2).

We agree that perfection is unattainable. We lack infinite resources, face feasibility constraints, and collect noisy measurements, even when conducting randomized controlled experiments (Kimmel et al. 2021). If we required perfection, we would likely have to discard almost all ecological studies, including causal studies using observational data, experimental studies in messy field settings where implementation is not perfect, models that use approximations to reality, and descriptive studies by observers with incomplete system knowledge.

Perfection is not, however, what causal inference requires. The causal literature uses hypothetical idealized study designs to clarify what perfection would look like, then asks us to be *transparent* about how our work departs from that ideal and about whether those departures would change our conclusions. In this way, a binary distinction between 'causal' and 'non-causal' analyses and claims is inappropriate. Instead, there is a spectrum from 'more plausibly causal' to 'less plausibly causal' that the causal inference paradigm establishes, with domain knowledge placing a study on that spectrum.

Transparency – not perfection – is the goal. We all know no single study is perfect. It is the authors' transparency about what exactly they did that allows science to continue advancing. Transparency allows researchers to interpret and report their own results cautiously, enables others to assess whether they agree the causal assumptions hold, and invites future work to reduce the uncertainties that lead to different results (Dee *et al.* 2026). Transparency is important because empirical causal claims rarely come from direct observation

of the true, underlying data generating process; they rest on a combination of data, pre-existing knowledge, and causal assumptions.

Assessing and being transparent about how much weight those assumptions can bear proceeds in three steps. First, we envision the "ideal experiment" for a causal question of interest. This ideal experiment is often unattainable; for example, ethically we should not cause species extinctions, nor can we feasibly manipulate all possible combinations of species at relevant scales. Second, we seek to approximate the ideal experiment as best as possible with an alternate research design that can identify causal relationships under certain assumptions (Hernán & Robins 2016; Hernan & Robins 2024; Rubin 1974). Finally, because causal assumptions cannot be verified directly from data, the third step is to be our own critic: we assess the plausibility that our causal assumptions hold in our empirical setting, seek evidence consistent with violations of those assumptions, and ask how those violations would change our conclusions be (Hernán & Robins 2016; Hernan & Robins 2024). Collectively, these steps provide us with some insights into how far away from "ideal" we may so that we and others can assess what and how much we can learn from our study.

Expecting imperfection, the third step in the pursuit of transparency asks about the consequences of imperfection. This third step warrants elaboration, because it is where our approach and that of Grace (2024) and Grace et al. (2025) diverge on what counts as adequate scrutiny. Probing the consequences of imperfection is a core part of the overall causal research process (see Clarification #1). To help understand the extent to which model specification decisions or potential violations of causal assumptions change inferences, we conduct

robustness checks and formal sensitivity analyses (for useful examples of robustness checks, see Dee et al. 2023 and Dudney et al. 2025). In contrast, Grace et al. (2025) propose a "*useful approximation*" for defending causal claims, which requires assessing whether bias is likely smaller than the estimated causal effect. This asks the right question but offers no means of answering it: the criterion is fundamentally untestable, because assessing whether it holds requires quantifying the very unmeasured confounding we cannot observe (Box 2). Such an approach may be useful in cases of extraordinarily strong immediate effects and simple systems -- for example in the question of whether airplane crashes cause deaths (Grace 2024). However, ecological systems are inherently complex, with ecological analyses often targeting effects of unknown size, and at various time and distance from a cause.

Sensitivity analyses address the same question with concrete tools to ask, "*how strong would bias from an unmeasured confounder, measurement error, or other causal assumption violation have to be to overturn the result*?" Such tools include quantifying how strong an unmeasured confounder would need to be to nullify an estimated effect (Cinelli & Hazlett 2020; Oster 2017); calculating bounds on how much estimates could shift under worst-case bias scenarios with partial identification (Manski 2020); and injecting simulated biases into datasets to observe how much conclusions change (Cinquini *et al.* 2025). By reporting these sensitivity analyses, researchers can show that any plausible bias is unlikely to exceed the estimated effect, even in the absence of perfect mechanistic knowledge or uncertainty (e.g. see ecological examples in Andraczek et al. 2024; Brumberg et al. 2026; Dee et al. 2023; Guizar-Coutiño et al. 2026; Naidoo et al. 2025).

Taken together, we argue that transparently grappling with and presenting causal assumptions helps subject matter experts assess their plausibility given the data, analysis, context, and research question (also see Dee et al. 2026) . The plausibility of meeting assumptions, and thus the validity of results, can then be debated. Such disagreements can identify the uncertainties that matter most, informing future analyses (e.g. see Dee et al. 2026). Causal understanding thus accumulates not by certifying individual studies as causal or not, but by progressively narrowing what remains in doubt over time.

Outlook and Conclusions

In our essay, we have argued that engagement with the modern literature on empirical causal research is not a break from the way ecologists do science. In addressing seven myths that could falsely position causal inference at odds with the norms and practices of ecology, we have shown that this literature extends modes of causal, mechanistic reasoning that biologists and ecologists have long used, dating back to Darwin and Mendel. What the causal literature adds is a way of making the assumptions underlying that reasoning explicit and assessable. Therefore, encourage broader engagement of ecologists with this rapidly evolving, multi-disciplinary literature.

This literature offers a flexible and growing set of approaches suited to many different research questions and inferential goals. We therefore envision an inclusive and collaborative future where theory, experiments, causal inference workflows for observational data, and causal discovery inform one another (e.g. (Correia *et al.* 2026; Runge *et al.* 2019; Schrodt *et al.* 2025). Several ecological studies already exemplify such integration (e.g., Arif & MacNeil 2022;

539 Arif & Massey 2023; Suskiewicz et al. 2024). This integration is also reflected in high-stakes
540 policy assessments. For example, the U.S. framework for assessing causal relationships
541 between air pollution and effects on health and ecological welfare, for example, combines
542 weight-of-evidence reasoning with counterfactual and quasi-experimental approaches
543 (National Academies of Sciences 2022; i.e., see Appendix C).

544 The approaches in this literature, however, are not a panacea, nor does they provide
545 easy, off-the-shelf solutions to causal questions in every context. The concepts and methods
546 offered in the causal literature can sharpen causal claims and contribute mechanistic
547 understanding, but they are unlikely, by themselves, to provide a complete mechanistic account
548 of ecological systems in the sense of Craver & Darden (2013). Moreover, applying techniques
549 from this literature without explicitly stating and critically evaluating their causal assumptions
550 can lead to incorrect inferences while creating the appearance of progress in causal
551 understanding.

552 Practices that promote transparency and interrogate causal assumptions are therefore
553 essential to the credibility and robustness of causal claims (Dee *et al.* 2026; National Academies
554 of Sciences 2022). These practices include stating and justifying causal assumptions, performing
555 sensitivity tests (Cinelli & Hazlett 2020; Dee *et al.* 2023; Guizar-Coutiño *et al.* 2026), and
556 assessing whether findings are consistent with process-based models or natural history
557 knowledge. Indeed, causal claims are strengthened when multiple, divergent lines of evidence
558 converge, an aim that frameworks for evidence triangulation can facilitate (Dee *et al.* 2026;
559 Gutierrez *et al.* 2025; Lawlor *et al.* 2016; Matthay *et al.* 2020). The causal literature also
560 provides guidance for specifying and interpreting what causal relationships can be estimated

561 from our data (the “estimand”) and for assessing the populations and settings to which our
562 empirical understandings can be generalized or transported (see Bareinboim & Pearl 2016;
563 Spake et al. 2025; Tabell et al. 2026).

564 Crucially, engagement with the broader causal literature need not be a one-way transfer
565 of methods into ecology. It can also enable ecologists to develop new causal frameworks and
566 shape causal research in other disciplines. For example, convergent cross mapping, which was
567 developed in ecology for causal discovery in dynamic and non-linear systems (Sugihara *et al.*
568 2012), has already been adopted across multiple disciplines (e.g., (Runge *et al.* 2019; Schiecke
569 *et al.* 2015; Tsonis *et al.* 2018). Ecology’s rich history of creative experimental approaches can
570 likewise offer lessons to other fields. Experimental economics, for example, may benefit from
571 how ecologists have approached generalizability (i.e., “external validity”) of experimental
572 evidence. Relevant practices include reporting standards that facilitate evidence synthesis and
573 meta-analyses (e.g., Nakagawa et al. 2023; O’Dea et al. 2021), as well as coordinated research
574 networks that apply comparable protocols across contexts to build cumulative knowledge and
575 characterize context-dependence (Borer *et al.* 2014, 2023). By engaging with the broader
576 literature on empirical causal research, ecologists can deepen causal understanding within our
577 field while contributing distinctively ecological insights to a richer and more pluralistic
578 understanding of causality.

579

580 **Acknowledgements**

We thank Jon Chase, John Drake, and the Ecology Letters editorial board for providing feedback and a forum for this discussion. LD acknowledges support from NSF DEB #2340606. JEKB acknowledges support from the Stone Living Lab and NASA #80NSSC21M0358.

Display Items

FIGURE 1. The causal inference literature offers multiple pathways for empirical causal understanding for a variety of research questions, including about mechanisms. These frameworks draw on and complement theory, natural history observations, and process-based models and can be braided with other ways of knowing (Gazing Wolf *et al.* 2024). Approaches can be tailored to diverse research questions and goals and include methods for learning potential causal structures in data and for estimating average effects, heterogeneous effects (Athey & Wager 2019; Jawadekar *et al.* 2023), mechanisms (Correia *et al.* 2025; Morgan & Winship 2015; VanderWeele 2015, 2010), and more (e.g. Spake et al. 2025). Approaches can be used together and *iteratively* as part of the scientific process to enrich understanding of dynamic, complex ecosystems and to provide robust evidence for conservation and other interventions. For instance, a researcher could start with causal discovery to generate a causal graph and associated hypotheses (e.g. what is a plausible causal graph relating fire to forest recovery?), validate or reject those plausible graphs with domain knowledge, then progress to mediation analyses (e.g. to quantify how much fire severity mediates the effect of fires on post-fire recovery trajectories or ecosystem transformations).

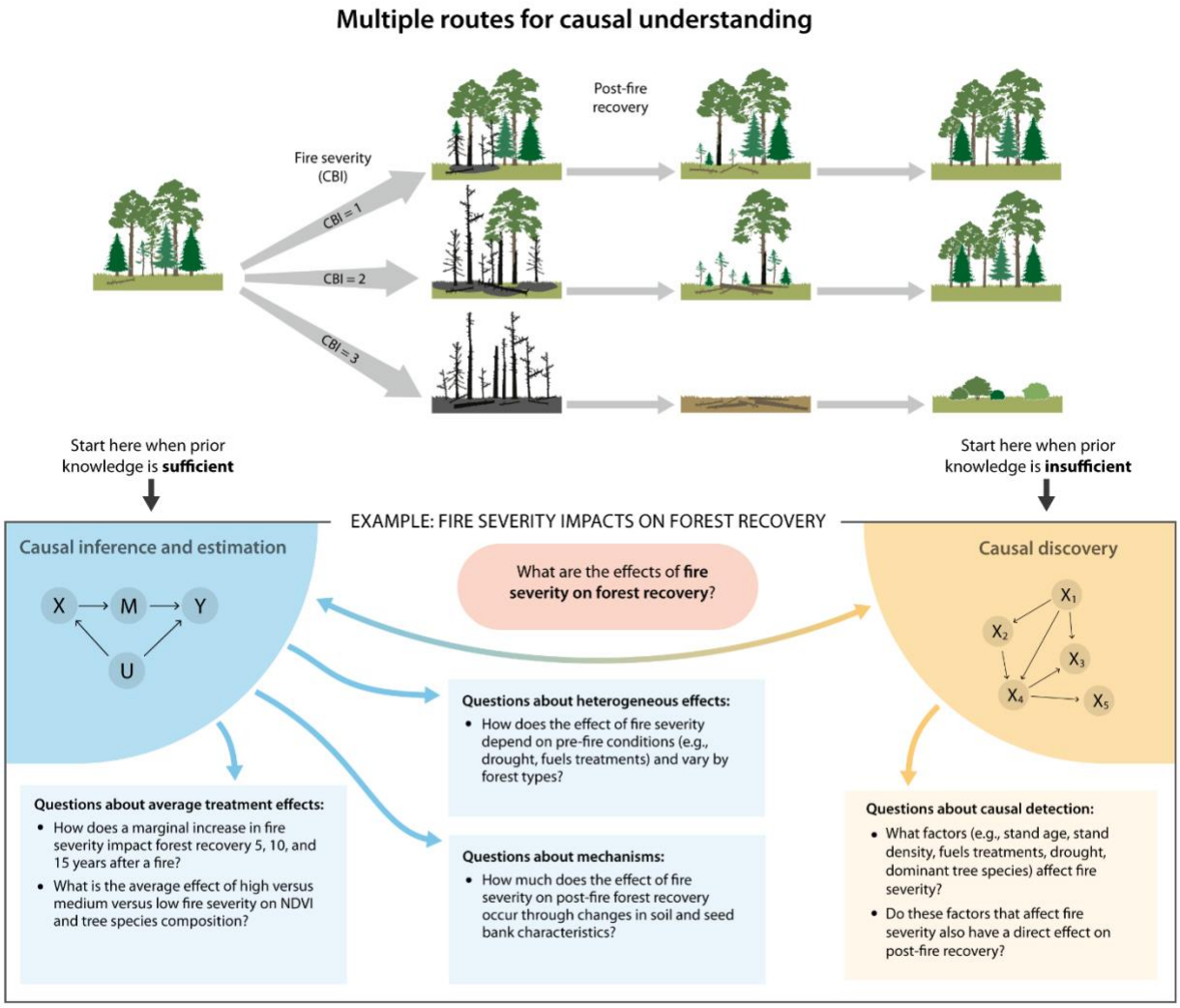
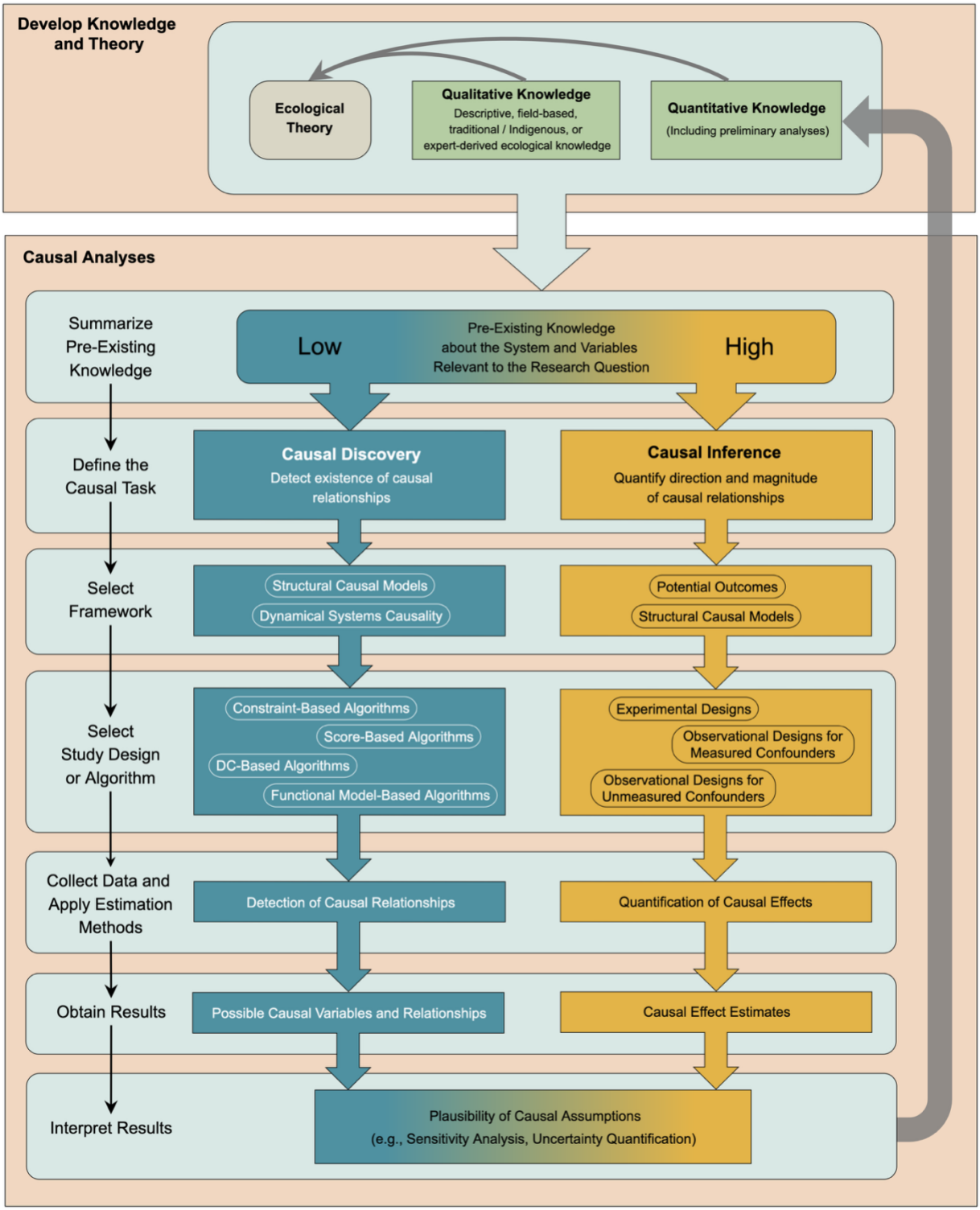

Multiple routes for causal understanding
Post-fire recovery
Fire severity (CBI)
CBI = 1
CBI = 2
CBI = 3
Start here when prior knowledge is sufficient
Start here when prior knowledge is insufficient
EXAMPLE: FIRE SEVERITY IMPACTS ON FOREST RECOVERY
Causal inference and estimation
X
M
Y
U
What are the effects of fire severity on forest recovery?
Causal discovery
X1
X2
X3
X4
X5
Questions about heterogeneous effects:
How does the effect of fire severity depend on pre-fire conditions (e.g., drought, fuels treatments) and vary by forest types?
Questions about average treatment effects:
How does a marginal increase in fire severity impact forest recovery 5, 10, and 15 years after a fire?
What is the average effect of high versus medium versus low fire severity on NDVI and tree species composition?
Questions about mechanisms:
How much does the effect of fire severity on post-fire forest recovery occur through changes in soil and seed bank characteristics?
Questions about causal detection:
What factors (e.g., stand age, stand density, fuels treatments, drought, dominant tree species) affect fire severity?
Do these factors that affect fire severity also have a direct effect on post-fire recovery?

Figure 2. The causal research literature offers more than methods – it offers a workflow for thinking about, doing, interpreting, and reporting science in ways that are compatible with ecological research. This figure shows one potential workflow for causal analyses, illustrating that causal analysis is a structured but flexible process, inclusive of a variety of methods and frameworks, modified from Correia et al. (2026). Developing knowledge and theory prior to causal analyses, including about mechanisms, can draw on many sources (including but not limited to those included in the SI of Correia et al. 2026).



617

618

619 **Box 2. Transparency or the Useful Approximation Standard?**

Our emphasis on the transparent articulation of causal assumptions, and on methods that help satisfy them (for example, by reducing unmeasured confounding), is characterized by Grace (2024) and Grace et al. (2025) as an unrealistic “perfection standard.” Instead, they advocate for what they call a “useful approximation standard." They argue that an estimate may be treated as causal so long as the causal component of an estimate is believed to exceed its bias component: $|\beta| > |\psi|$, where β is the unobserved true effect and ψ the unobserved bias, and only their sum is identified by the data.

We see four problems with this standard.

First, the inequality is untestable, even with infinite data. Assessing whether it holds requires knowledge of the strength of unmeasured confounders or of other uncontrolled sources of bias, such as colliders or non-classical measurement error. But these quantities are unobserved. In the case of confounding, we may not even be able to name all relevant confounders. The standard therefore rests on an untestable and, in our view, very strong assumption about the magnitude of hidden biases.

Second, in science and policy, the magnitudes of causal effects matter as much as their sign. Even if the useful approximation inequality held, a researcher could at best infer the correct sign of an effect. But in ecology, effect sizes are often as important as, or more important than, effect signs. For example, we may care less about whether temperature has a non-zero effect on vegetation productivity than about how the magnitude of that effect varies across different temperatures. Likewise, in coastal systems, the key question may not be whether coastal

642 eutrophication has any effect on fisheries, but how much eutrophication can occur before
643 oxygen depletion becomes critical. Errors in effect size estimation can have serious
644 consequences for science and policy, especially when resources are limited.
645
646 Third, ecologists do not need to choose between perfection and inaction. We have tools for
647 reducing the influence of unmeasured confounding variables and for relaxing unrealistic
648 assumptions. No analysis can be perfect, but that is precisely why we should use the best
649 available tools to obtain the most credible estimates possible.
650
651 Fourth, the useful approximation standard offers no procedure for translating prior knowledge
652 into analytical decisions. Both Grace (2024, page 15) and Grace et al. (2025, page 9) state that
653 the short-term goal of causal knowledge analysis is "*to inform decisions related to the statistical*
654 *procedures appropriate for analyzing new data,*" but neither specifies which decisions prior
655 knowledge should govern, or how. The broader causal literature does provide such guidance
656 (see Correia et al. 2026; Siegel & Dee 2025).
657
658 No causal analysis is perfect. For that reason, we emphasize transparency about potential
659 threats to causal interpretations and about the implications of possible violations of causal
660 assumptions. Grace et al. (2025, page 2) describe their own paradigm as one "*not defined by*
661 *rigid adherence to specific practices,*" in explicit contrast to the paradigm they attribute to
662 causal statistics. But they mistake which practice is at issue. We do not urge rigid adherence to
663 any particular method, study design, or threshold for acceptable bias. We instead urge a

commitment to transparency about the plausibility of the causal assumptions that underlie causal claims, and about the degree to which these claims change under plausible violations of those assumptions. We believe this practice is essential for robust, credible empirical ecological science.

References

Addicott, E.T., Fenichel, E.P., Bradford, M.A., Pinsky, M.L. & Wood, S.A. (2022). Toward an improved understanding of causation in the ecological sciences. *Front. Ecol. Environ.*, 20, 474–480.

Andam, K.S., Ferraro, P.J., Pfaff, A., Sanchez-Azofeifa, G.A. & Robalino, J.A. (2008). Measuring the effectiveness of protected area networks in reducing deforestation. *Proceedings of the National Academy of Sciences*, 105, 16089–16094.

Andraczek, K., Dee, L.E., Weigelt, A., Hinderling, J., Prati, D., Le Provost, G., *et al.* (2024). Weak reciprocal relationships between productivity and plant biodiversity in managed grasslands. *Journal of Ecology*, 112, 2359–2373.

Anscombe, F.J. (1973). *Graphs in Statistical Analysis*. *Am. Stat.*

Arif, S. (2025). Estimating causal effects with machine learning: A guide for ecologists. *Methods Ecol. Evol.*, 16, 2771–2783.

Arif, S. & MacNeil, M.A. (2022). Utilizing causal diagrams across quasi-experimental approaches. *Ecosphere*, 13.

Arif, S. & MacNeil, M.A. (2023). Applying the structural causal model framework for observational causal inference in ecology. *Ecol. Monogr.*, 93.

Arif, S. & Massey, M.D.B. (2023). Reducing bias in experimental ecology through directed acyclic graphs. *Ecol. Evol.*, 13.

Athey, S. & Imbens, G. (2016). Recursive partitioning for heterogeneous causal effects. *Proc. Natl. Acad. Sci. U. S. A.*, 113, 7353–7360.

Athey, S. & Wager, S. (2019). Estimating treatment effects with causal forests: An application. *Obs. Stud.*, 5, 37–51.

Bareinboim, E. & Pearl, J. (2016). Causal inference and the data-fusion problem. *Proceedings of the National Academy of Sciences*, 113, 7345–7352.

Bollen, K. & Pearl, J. (2012). Eight Myths About Causality and Structural Models. In: *Handbook of Causal Analysis for Social Research* (ed. S.L. Morgan). Springer, pp. 301–328.

Bollen, K.A. & Pearl, J. (2013). *Handbook of Causal Analysis for Social Research*. *Handbook of Causal Analysis for Social Research*, Handbooks of Sociology and Social Research. Springer Netherlands, Dordrecht.

Borer, E.T., Harpole, W.S., Adler, P.B., Lind, E.M., Orrock, J.L., Seabloom, E.W., *et al.* (2014). Finding generality in ecology: A model for globally distributed experiments. *Methods Ecol. Evol.*, 5, 65–73.

Borer, E.T., MacDougall, A.S., Stevens, C.J., Sullivan, L.L., Wilfahrt, P.A. & Seabloom, E.W. (2023). Writing a massively multi-authored paper: Overcoming barriers to meaningful authorship for all. *Methods Ecol. Evol.*, 14, 1432–1442.

Borger, M.J. & Ramesh, A. (2025). Let's DAG in: how directed acyclic graphs can help behavioural ecology be more transparent. *Proceedings of the Royal Society B: Biological Sciences*, 292.

Boyd, R., Cooke, R., Powney, G. & Pescott, O. (2026). Why your causal diagram should probably include sampling and measurement processes.

Brumberg, H.D., Dee, L.E., Murayama, H., Alvarado Barrientos, J.J., Bessesen, B., Bouffard, M.G., *et al.* (2026). Riparian vegetation reduces coastal turbidity. *Communications Sustainability*, 1, 29.

Butsic, V., Lewis, D.J., Radeloff, V.C., Baumann, M. & Kuemmerle, T. (2017a). Quasi-experimental methods enable stronger inferences from observational data in ecology. *Basic Appl. Ecol.*, 19, 1–10.

Butsic, V., Lewis, D.J., Radeloff, V.C., Baumann, M. & Kuemmerle, T. (2017b). Quasi-experimental methods enable stronger inferences from observational data in ecology. *Basic Appl. Ecol.*, 19, 1–10.

Byrnes, J.E.K. & Dee, L.E. (2025). Causal Inference With Observational Data and Unobserved Confounding Variables. *Ecol. Lett.*, 28.

Chen, S., Wu, H. & Zhao, H. (2025). A comparison of causal inference methods for evaluating multiple treatment groups. *J. Nonparametr. Stat.*, 37, 1317–1340.

Cinelli, C. & Hazlett, C. (2020). Making Sense of Sensitivity: Extending Omitted Variable Bias. *J. R. Stat. Soc. Series B Stat. Methodol.*, 82, 39–67.

Cinquini, M., Makhlouf, K., Zhioua, S., Palamidessi, C. & Guidotti, R. (2025). A Bias Injection Technique to Assess the Resilience of Causal Discovery Methods. *IEEE Access*, 13, 97376–97391.

Correia, H.E., Dee, L.E., Byrnes, J.E.K., Fieberg, J.R., Fortin, M.-J., Glymour, C., *et al.* (2026). Best practices for moving from correlation to causation in ecological research. *Nat. Commun.*, 17, 1981.

Correia, H.E., Dee, L.E. & Ferraro, P.J. (2025). Designing causal mediation analyses to quantify intermediary processes in ecology. *Biological Reviews*, 100, 1512–1533.

Craver, C.F.. & Darden, Lindley. (2013). *In search of mechanisms : discoveries across the life sciences*. The University of Chicago Press.

Cunningham, Scott. (2021). *Causal inference : the mixtape*. Yale University Press.

D'Agostino McGowan, L., Gerke, T. & Barrett, M. (2024). Causal Inference Is Not Just a Statistics Problem. *Journal of Statistics and Data Science Education*, 32, 150–155.

Dee, L., Kimmel, K., Hayden, M., Kelleher, S., Ohlert, T., Merchant, T., *et al.* (2026). From open science to credible science: practical guidance for more transparent and robust causal claims in ecology and conservation. https://doi.org/10.32942/X2SD5S

Dee, L.E., Ferraro, P.J., Severen, C.N., Kimmel, K.A., Borer, E.T., Byrnes, J.E.K., *et al.* (2023). Clarifying the effect of biodiversity on productivity in natural ecosystems with longitudinal data and methods for causal inference. *Nat. Commun.*, 14.

743 Van Deynze, B., Swinton, S.M., Hennessy, D.A., Haddad, N.M. & Ries, L. (2024). Insecticides,
744 more than herbicides, land use, and climate, are associated with declines in butterfly
745 species richness and abundance in the American Midwest. *PLoS One*, 19, e0304319.
746 Dudney, J., Dee, L.E., Heilmayr, R., Byrnes, J. & Siegel, K. (2025). A Causal Inference
747 Framework for Climate Change Attribution in Ecology. *Ecol. Lett.*, 28.
748 Dudney, J., Willing, C.E., Das, A.J., Latimer, A.M., Nesmith, J.C.B. & Battles, J.J. (2021).
749 Nonlinear shifts in infectious rust disease due to climate change. *Nat. Commun.*, 12.
750 Elwert, F. & Winship, C. (2014). Endogenous Selection Bias: The Problem of Conditioning on a
751 Collider Variable. *Annu. Rev. Sociol.*, 40, 31–53.
752 Fairchild, A.J. & MacKinnon, D.P. (2009). A General Model for Testing Mediation and
753 Moderation Effects. *Prevention Science*, 10, 87–99.
754 Ferguson, K.D., McCann, M., Katikireddi, S.V., Thomson, H., Green, M.J., Smith, D.J., *et al.*
755 (2020). Evidence synthesis for constructing directed acyclic graphs (ESC-DAGs): A novel
756 and systematic method for building directed acyclic graphs. *Int. J. Epidemiol.*, 49, 322–329.
757 Ferraro, P.J. & Hanauer, M.M. (2014). Advances in Measuring the Environmental and Social
758 Impacts of Environmental Programs. *Annu. Rev. Environ. Resour.*, 39, 495–517.
759 Fick, S.E., Nauman, T.W., Brungard, C.C. & Duniway, M.C. (2021). Evaluating natural
760 experiments in ecology: using synthetic controls in assessments of remotely sensed land
761 treatments. *Ecological Applications*, 31.
762 Franks, D.W., Ruxton, G.D. & Sherratt, T. (2025). Ecology needs a causal overhaul. *Biological*
763 *Reviews*, 100, 1950–1969.
764 Gazing Wolf, J., Ignace, D.D., David-Chavez, D.M., Jennings, L.L., Smiles, D., Blanchard, P., *et*
765 *al.* (2024). Centering Indigenous Knowledges in ecology and beyond. *Front. Ecol. Environ.*,
766 22.
767 Gerber, A.S.. & Green, D.P.. (2012). *Field experiments : design, analysis, and interpretation*.
768 W.W. Norton.
769 Glymour, C., Zhang, K. & Spirtes, P. (2019). Review of causal discovery methods based on
770 graphical models. *Front. Genet.*, 10.
771 Grace, J. (2026). Call for a paradigm shift from statistical causal inference to multi-evidence
772 causal investigation.
773 Grace, J.B. (2024). An integrative paradigm for building causal knowledge. *Ecol. Monogr.*, 94.
774 Grace, J.B., Anderson, T.M., Olff, H. & Scheiner, S.M. (2010). On the specification of structural
775 equation models for ecological systems. *Ecol. Monogr.*, 80, 67–87.
776 Grace, J.B., Huntington-Klein, N., Schweiger, E.W., Martinez, M., Osland, M.J., Feher, L.C., *et*
777 *al.* (2025). Causal Effects Versus Causal Mechanisms: Two Traditions With Different
778 Requirements and Contributions Towards Causal Understanding. *Ecol. Lett.*, 28.
779 Grace, J.B. & Irvine, K.M. (2020). Scientist's guide to developing explanatory statistical models
780 using causal analysis principles. *Ecology*, 101, 1–14.
781 Grace, J.B., Schoolmaster, D.R., Guntenspergen, G.R., Little, A.M., Mitchell, B.R., Miller, K.M.,
782 *et al.* (2012). Guidelines for a graph-theoretic implementation of structural equation
783 modeling. *Ecosphere*, 3, 1–44.
784 Guizar-Coutiño, A., Nicholson, G., Coomes, D., Ferraro, P.J., Swinfield, T. & Jones, J.P.G.
785 (2026). Unobserved confounders cannot explain over-crediting in avoided deforestation
786 carbon projects. *Nat. Ecol. Evol.*

787 Gutierrez, S., Glymour, M.M. & Smith, G.D. (2025). Evidence triangulation in health research.
788 *Eur. J. Epidemiol.*, 40, 743–757.
789 Hernán, M.A. & Robins, J.M. (2016). Using Big Data to Emulate a Target Trial When a
790 Randomized Trial Is Not Available. *Am. J. Epidemiol.*, 183, 758–764.
791 Hernan, M.A.. & Robins, J.M. (2024). *Causal inference : what if*. Taylor and Francis.
792 Imai, K., Keele, L. & Tingley, D. (2010). A General Approach to Causal Mediation Analysis.
793 *Psychol. Methods*, 15, 309–334.
794 Imai, K., Tingley, D. & Yamamoto, T. (2012). *Experimental designs for identifying causal*
795 *mechanisms*. *J. R. Statist. Soc. A*.
796 Imai, K. & Yamamoto, T. (2013). Identification and sensitivity analysis for multiple causal
797 mechanisms: Revisiting evidence from framing experiments. *Political Analysis*, 21, 141–
798 171.
799 Imbens, G.W. & Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *J.*
800 *Econom.*, 142, 615–635.
801 Jawadekar, N., Kezios, K., Odden, M.C., Stingone, J.A., Calonico, S., Rudolph, K., *et al.* (2023).
802 Practical Guide to Honest Causal Forests for Identifying Heterogeneous Treatment Effects.
803 *Am. J. Epidemiol.*, kwad043.
804 Joppa, L.N. & Pfaff, A. (2009). High and far: Biases in the location of protected areas. *PLoS*
805 *One*, 4, 1–6.
806 Kaiser, A., Resasco, J. & Dee, L. (2025). Synthetic control methods enable stronger causal
807 inference using participatory science data in cities.
808 Kazemi, A.A. & Olsen, I.C. (2024). Instrumental variable analysis with categorical treatment.
809 *Stat. Methods Med. Res.*, 33, 2043–2061.
810 Kimmel, K., Dee, L.E., Avolio, M.L. & Ferraro, P.J. (2021a). Causal assumptions and causal
811 inference in ecological experiments. *Trends Ecol. Evol.*, 36, 1141–1152.
812 Kimmel, K., Dee, L.E., Avolio, M.L. & Ferraro, P.J. (2021b). Causal assumptions and causal
813 inference in ecological experiments. *Trends Ecol. Evol.*, 36, 1141–1152.
814 Kline, B. & Tamer, E. (2023). Recent Developments in Partial Identification. *Annu. Rev.*
815 *Econom.*, 15, 125–150.
816 Kreyling, J., Arfin Khan, M.A.S., Sultana, F., Babel, W., Beierkuhnlein, C., Foken, T., *et al.*
817 (2017). Drought Effects in Climate Change Manipulation Experiments: Quantifying the
818 Influence of Ambient Weather Conditions and Rain-out Shelter Artifacts. *Ecosystems*, 20,
819 301–315.
820 Larsen, A.E., Meng, K. & Kendall, B.E. (2019). Causal analysis in control–impact ecological
821 studies with observational data. *Methods Ecol. Evol.*, 0.
822 Laubach, Z.M., Murray, E.J., Hoke, K.L., Safran, R.J. & Perng, W. (2021). A biologist's guide to
823 model selection and causal inference. *Proceedings of the Royal Society B: Biological*
824 *Sciences*, 288, 20202815.
825 Lawlor, D.A., Tilling, K. & Smith, G.D. (2016). Triangulation in aetiological epidemiology. *Int. J.*
826 *Epidemiol.*, 45, 1866–1886.
827 Lee, S., Ma, Y. & de Luna, X. (2025). Covariate balancing for causal inference on categorical
828 and continuous treatments. *Econom. Stat.*, 33, 304–329.
829 Lefcheck, J.S. & Duffy, J.E. (2015). Multitrophic functional diversity predicts ecosystem
830 functioning in experimental assemblages of estuarine consumers. *Ecology*, 96, 2973–2983.

Littell, J.S., Peterson, D.L., Riley, K.L., Liu, Y. & Luce, C.H. (2016). A review of the relationships between drought and forest fire in the United States. *Glob. Chang. Biol.*, 22, 2353–2369.

MacDonald, A.J., Larsen, A.E. & Plantinga, A.J. (2019). Missing the people for the trees: Identifying coupled natural–human system feedbacks driving the ecology of Lyme disease. *Journal of Applied Ecology*, 56, 354–364.

Manski, C.F. (1989). *Anatomy of the Selection Problem*. *Source: The Journal of Human Resources*.

Manski, C.F. (2020). The lure of incredible certitude. *Econ. Philos.*, 36, 216–245.

Marziali, M.E., Inglis, K., Frank, P., Campbell, W., Lambert, S., Magagula, P., *et al.* (2026). Who is the expert? A process guide to incorporate lived/living experience into directed acyclic graphs: an applied example from HIV research. *J. Epidemiol. Community Health (1978).*, jech-2025-224787.

Mascaro, S., Wu, Y., Woodberry, O., Nyberg, E.P., Pearson, R., Ramsay, J.A., *et al.* (2023). Modeling COVID-19 disease processes by remote elicitation of causal Bayesian networks from medical experts. *BMC Med. Res. Methodol.*, 23.

Matejka, J. & Fitzmaurice, G. (2017). Same stats, different graphs: Generating datasets with varied appearance and identical statistics through simulated annealing. In: *Conference on Human Factors in Computing Systems - Proceedings*. Association for Computing Machinery, pp. 1290–1294.

Matthay, E.C., Hagan, E., Gottlieb, L.M., Tan, M.L., Vlahov, D., Adler, N.E., *et al.* (2020). Alternative causal inference methods in population health research: Evaluating tradeoffs and triangulating evidence. *SSM Popul. Health*, 10.

McConnachie, M.M., van Wilgen, B.W., Richardson, D.M., Ferraro, P.J. & Forsyth, A.T. (2015). Estimating the effect of plantations on pine invasions in protected areas: a case study from South Africa. *Journal of Applied Ecology*, 52, 110–118.

Morgan, S.L. & Winship, C. (2015). *Counterfactuals and causal inference*. Cambridge University Press.

Naidoo, R., Gulati, S., Vercammen, J. & Burton, C. (2025). Estimating Causal Impacts of Human Recreation on Wildlife in the Absence of Experimental Controls. *Conserv. Lett.*, 18.

Nakagawa, S., Yang, Y., Macartney, E.L., Spake, R. & Lagisz, M. (2023). Quantitative evidence synthesis: a practical guide on meta-analysis, meta-regression, and publication bias tests for environmental sciences. *Environ. Evid.*, 12, 8.

National Academies of Sciences, E. and M. (2022). *Advancing the Framework for Assessing Causality of Health and Welfare Effects to Inform National Ambient Air Quality Standard Reviews*. National Academies Press, Washington, D.C.

O'Dea, R.E., Lagisz, M., Jennions, M.D., Koricheva, J., Noble, D.W.A., Parker, T.H., *et al.* (2021). Preferred reporting items for systematic reviews and meta-analyses in ecology and evolutionary biology: a <scp>PRISMA</scp> extension. *Biological Reviews*, 96, 1695–1722.

Oster, E. (2017). Unobservable Selection and Coefficient Stability: Theory and Evidence. *Journal of Business and Economic Statistics*, 0, 1–18.

Pearl, J. (2009). Causal inference in statistics: An overview. *Stat. Surv.*, 3, 96–146.

Pearl, J. (2010). An Introduction to Causal Inference. *Int. J. Biostat.*, 6.

Pearl, J. (2011). *Causality: Models, reasoning, and inference, second edition*. *Causality: Models, Reasoning, and Inference, Second Edition*.

Pearl, J. (2014). The Causal Foundations of Structural Equation Modeling. *Handbook of Structural Equation Modeling*.

Pfaff, A. & Robalino, J. (2012). Protecting forests, biodiversity, and the climate: Predicting policy impact to improve policy choice. *Oxf. Rev. Econ. Policy*, 28, 164–179.

Proctor, J., Carleton, T. & Sum, S. (2023). *Parameter Recovery Using Remotely Sensed Variables*. Cambridge, MA.

Richardson, T. & Spirtes, P. (2002). Ancestral graph Markov models. *The Annals of Statistics*, 30, 962–1030.

Richiardi, L., Bellocco, R. & Zugna, D. (2013). Mediation analysis in epidemiology: Methods, interpretation and bias. *Int. J. Epidemiol.*, 42, 1511–1519.

Robins, J.M. & Greenland, S. (1992). Identifiability and Exchangeability for Direct and Indirect Effects. *Epidemiology*, 3.

Rosenbaum, P.R. (2012). Interference Between Units in Randomized Experiments. *J. Am. Stat. Assoc.*, 102, 191–200.

Rubin, D.B. (1974). Estimating causal effects of treatments in randomized and nonrandomized studies. *J. Educ. Psychol.*, 66, 688–701.

Rubin, D.B. (2005). Causal inference using potential outcomes: Design, Modeling, Decisions. *J. Am. Stat. Assoc.*, 100, 322–331.

Runge, J., Bathiany, S., Bollt, E., Camps-Valls, G., Coumou, D., Deyle, E., *et al.* (2019). Inferring causation from time series in Earth system sciences. *Nat. Commun.*, 10.

Schiecke, K., Pester, B., Feucht, M., Leistritz, L. & Witte, H. (2015). Convergent Cross Mapping: Basic concept, influence of estimation parameters and practical application. In: *2015 37th Annual International Conference of the IEEE Engineering in Medicine and Biology Society (EMBC)*. IEEE, pp. 7418–7421.

Schrodt, F., Beck, M., Estopinan, J., Bowler, D.E., Fontaine, C., Gaüzère, P., *et al.* (2025). Advancing causal inference in ecology: Pathways for biodiversity change detection and attribution. *Methods Ecol. Evol.*, 16, 2276–2304.

Shipley, B. (2004). *Cause and Correlation in Biology A User ' s Guide to Path Analysis , Structural Equations and Causal Inference*. *Cambridge University Press*.

Shipley, B. (2009). Confirmatory path analysis in a generalized multilevel context. *Ecology*, 90, 363–368.

Shpitser, I. (2013). Counterfactual graphical models for longitudinal mediation analysis with unobserved confounding. *Cogn. Sci.*, 37, 1011–1035.

Siegel, K. & Dee, L.E. (2025). Foundations and Future Directions for Causal Inference in Ecological Research. *Ecol. Lett.*, 28.

Siegel, K.J., Macaulay, L., Shapero, M., Becchetti, T., Larson, S., Mashiri, F.E., *et al.* (2022). Impacts of livestock grazing on the probability of burning in wildfires vary by region and vegetation type in California. *J. Environ. Manage.*, 322, 116092.

Simler-Williamson, A.B. & Germino, M.J. (2022). Statistical considerations of nonrandom treatment applications reveal region-wide benefits of widespread post-fire restoration action. *Nat. Commun.*, 13, 3472.

Spake, R., Jackson, E.E., Bullock, J.M., Gardner, E., Tipton, E., Grainger, M.J., *et al.* (2025). Precision ecology for targeted conservation action. *Nat. Ecol. Evol.*, 9, 1102–1111.

Spirtes, P., Glymour, C. & Scheines, R. (1993). *Causation, Prediction, and Search*. Springer New York, New York, NY.

Spirtes, P., Glymour, C. & Scheines, R. (2001). *Causation, Prediction, and Search*. The MIT Press.

Stevens-Rumann, C.S., Kemp, K.B., Higuera, P.E., Harvey, B.J., Rother, M.T., Donato, D.C., *et al.* (2018). Evidence for declining forest resilience to wildfires under climate change. *Ecol. Lett.*, 21, 243–252.

Stuart, E.A. (2010). Matching Methods for Causal Inference: A Review and a Look Forward. *Statistical Science*, 25.

Sugihara, G., May, R., Ye, H., Hsieh, C., Deyle, E., Fogarty, M., *et al.* (2012). Detecting Causality in Complex Ecosystems. *Science (1979).*, 338, 496–500.

Suskiewicz, T.S., Byrnes, J.E.K., Steneck, R.S., Russell, R., Wilson, C.J. & Rasher, D.B. (2024). Ocean warming undermines the recovery resilience of <scp>N</scp> ew <scp>E</scp> ngland kelp forests following a fishery-induced trophic cascade. *Ecology*, 105.

Tabell, O., Moser, N., Ovaskainen, O. & Karvanen, J. (2026). Transporting Causal Effects in Ecology: Concepts, Models and Software.

Tilman, D. (1987). The Importance of the Mechanisms of Interspecific Competition. *Am. Nat.*, 129, 769–774.

Tsonis, A.A., Deyle, E.R., Ye, H. & Sugihara, G. (2018). Convergent Cross Mapping: Theory and an Example. In: *Advances in Nonlinear Geosciences*. Springer International Publishing, Cham, pp. 587–600.

Underwood, A.J. (1986). The analysis of competition by field experiments. In: *Community Ecology: Patterns and Process. ISBN: 9780867932720* (eds. Kikkawa, J. & D.J. Anderson, D.J.). Blackwell Scientific, London, pp. 240–269.

Underwood, A.J. (1996). *Experiments in Ecology*. Cambridge University Press.

VanderWeele, T. (2015). *Explanation in Causal Inference*. 1st edn. Oxford University Press, New York, NY.

VanderWeele, T.J. (2010). Bias formulas for sensitivity analysis for direct and indirect effects. *Epidemiology*, 21, 540.

Wauchope, H.S., Amano, T., Geldmann, J., Johnston, A., Simmons, B.I., Sutherland, W.J., *et al.* (2021). Evaluating Impact Using Time-Series Data. *Trends Ecol. Evol.*

Wiens, J.A., Stenseth, N.Chr., Horne, B. Van & Ims, R.A. (1993). Ecological Mechanisms and Landscape Ecology. *Oikos*, 66, 369.

Wolkovich, E.M., Davies, T.J., Pearse, W.D. & Betancourt, M. (2026). A four-step simulation-based workflow for ecological analysis and science. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*, 384.