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Methods, Tools, and Technologies

Utilizing causal diagrams across quasi-experimental approaches

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Abstract

Recent developments in computer science have substantially advanced the use of observational causal inference under Pearl's structural causal model (SCM) framework. A key tool in the application of SCM is the use of casual diagrams, used to visualize the causal structure of a system or process under study. Here, we show how causal diagrams can be extended to ensure proper study design under quasi-experimental settings, including propensity score analysis, before-after-control-impact studies, regression discontinuity design, and instrumental variables. Causal diagrams represent a unified approach to variable selection across methodologies and should be routinely applied in ecology research with causal implications.

KEYWORDS

backdoor criterion, before-after-control-impact, causal diagrams, causal inference, directed acyclic graphs, instrumental variable, observational data, propensity score, regression discontinuity design, structural causal model

INTRODUCTION

The availability and importance of observation-based research has increased in recent years due to the proliferation of both digital data and global environmental threats that cannot be manipulated experimentally (Sagarin & Pauchard, 2010). While infrequently stated, most observational studies in ecology are aimed at answering causal questions, such as "What is the effect of protected areas on biodiversity?" (Gray et al., 2016). Yet, the prohibition of causal language for nonexperimental data promoted by Pearson and Fisher (Glymour, 2009) has constrained the use of observational data to answer fundamental causal questions in ecology. These opportunities and challenges highlight the importance of coherent methods to properly analyze observational data and attain accurate conclusions about ecological systems and processes.

Developments in observational causal inference have been spurred largely by the work of computer scientist Judea Pearl, whose structural causal model (SCM; Pearl, 2009) provides a comprehensive framework that utilizes causal diagrams to determine cause and effect relationships from purely observational data. Causal diagrams explicitly state the direction of causal associations between variables in a system and, in doing so, reveal noncausal (spurious) associations as well. By ensuring that researchers explicitly and transparently state their causal assumptions, causal diagrams invite critical reception and feedback that is typically difficult to frame.

What has gone unrecognized is that causal diagrams and principles from the SCM framework can also lead to effective study design across a range of quasi-experimental methods, including propensity score analysis, before-after-control impact (BACI) studies, regression discontinuity design (RDD), and instrumental variables (IV; Butsic et al., 2017). Quasi-experimental approaches are widely used among other disciplines, and in recent years, ecologists have argued for their increased use with ecological observational data (e.g., Butsic et al., 2017; Larsen et al., 2019; Wauchope et al., 2021). However, determining causal relationships from quasi-experimental methods requires proper study design that benefit from explicit communication about a researchers' causal assumptions (Adams et al., 2019; Ferraro et al., 2018). Here, we show how the principles of SCM and application of causal diagrams can extend across quasi-experimental approaches to lead to more robust causal inference from observational data. Using simulated data (with known causal effects), we show how the application of causal diagrams can return accurate causal estimates, as well as highlight how biases can arise when they are not considered.

STRUCTURAL CAUSAL MODEL

Pearl's SCM framework (Pearl, 2009) provides a comprehensive theory of causation by unifying structural equation models (Wright, 1921) with the potential outcome framework (Rubin, 2005) and other theories of causation. Although the mathematical underpinning is quite complex (Pearl, 2009), one of the advantages of applying SCM is that it reduces complicated equations and mathematical theory into a graphical application of rules using directed acyclic graphs (DAGs) to visualize and quantify causal relationships.

Directed acyclic graphs are causal diagrams that represent the causal structure of a system or process under study (Grace & Irvine, 2020). Specifically, nodes within a DAG represent variables, with directed arrows between nodes representing possible causal effects (e.g., $X \rightarrow Y$ shows that X affects Y). Directed acyclic graphs are also acyclic, meaning that they cannot contain bidirectional relationships or a feedback loop where a variable causes itself (Elwert, 2014). However, they can still represent ecological systems with bidirectional relationships by more finely articulating the temporal sequence of events (Greenland et al., 1999).

Directed acyclic graphs are created based on researchers' domain knowledge, which can be supported by expert opinion, scientific consensus, and relevant literature (e.g., Cronin & Schoolmaster, 2018; Grace & Irvine, 2020; Schoolmaster Jr et al., 2020). They must include all measured and unmeasured variables required to depict the system or process under study, as well as all common causes of any pair of variables included in the DAG (Spirtes et al., 2001). For example, to determine the effect of X on Y, our DAG must include X, Y, common causes of X and Y, and common causes of any pair of variables that are now included in the DAG.

As an ecological example, the DAG in Figure 1 represents the causal structure of how marine protected areas (MPAs) are expected to influence reef fish biomass for a hypothetical coral reef system, created based on past literature and expert knowledge of coral reef ecologists (Appendix S1: Table S1). We have created a simulated dataset based on the causal structure depicted in Figure 1, setting our *known* causal effect of MPA on reef fish biomass to **1.089** (Appendix S1: Section S1.2). We will use our DAG and associated simulated data to show how the application of SCM can lead to the accurate causal estimate of MPA on reef fish biomass.

Once a DAG has been created, it can be tested against the associated observational data to ensure DAG-data consistency. Simply put, a specified DAG will have (often many) independencies (e.g., X is independent of Y) and conditional independencies between variables (e.g., X is independent of Y, given Z) that should be consistent with the associated observational data, if both the DAG and observational data are representative of the data generating process (Pearl, 2009; Textor et al., 2016; Appendix S1: Section S1.3). If *all* implied independencies are compatible with the data, it provides support for a DAG. Given our DAG, there are 12 conditional independencies that are implied by our DAG (Appendix S1: Section S1.3). Using the package "dagitty," we can test our DAG against our simulated data, which shows that all implied independencies are consistent with our simulated dataset (Appendix S1: Section S1.3). We note that if a DAG does not pass DAG-data consistency, it must be tweaked until DAG-data consistency is ensured (Grace & Irvine, 2020; Textor et al., 2016). We also note that since several DAGs may pass DAG-data consistency, it is imperative that a finalized DAG is first and foremost justified through domain knowledge (e.g., through the literature, expert knowledge, and past experiments).

Once a DAG is finalized and ensures DAG-data consistency, we can apply a graphical procedure known as the *backdoor criterion* to determine the sufficient set for adjustment required to determine the effect of X on Y, or in this case MPA on reef fish biomass. The application of backdoor criterion is based on an algebraic procedure known as do-calculus, which equates observational distributions to what would be expected under a randomized control experiment (Pearl, 1993). While the application of do-calculus can make for challenging reading, based on its derived inferences rules, the backdoor criterion provides DAG-based graphical rules that can be applied to estimate causal effects from observational data.

Specifically, the backdoor criterion instructs us to block all *backdoor paths* between our predictor and

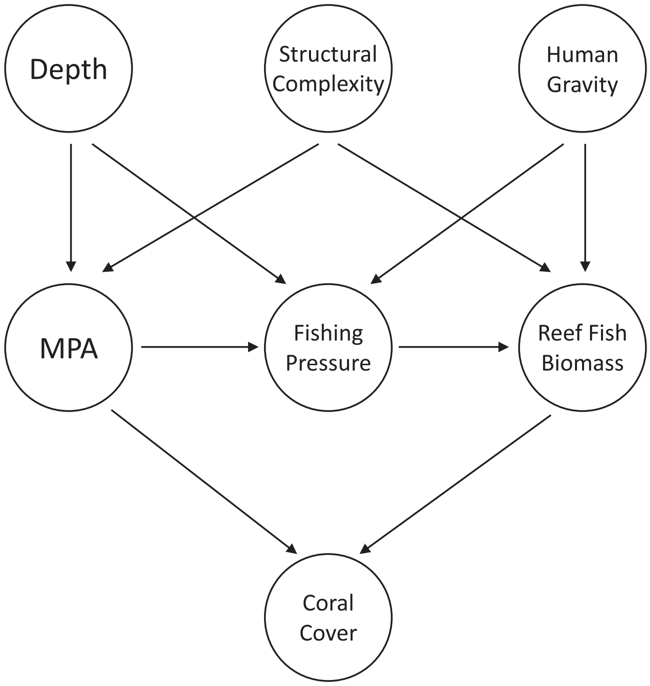

FIGURE 1 A directed acyclic graph (DAG) representing the causal structure between a marine protected area (MPA) and coral reef fish biomass

response variable, X and Y. A backdoor path is a sequence of arrows and nodes connecting X and Y variables with an arrow pointing into both X and Y. If left open, these backdoor paths create bias and induce spurious correlation by providing an indirect, noncausal path along which information can flow.

> *The backdoor criterion*:
> The backdoor criterion (Pearl, 1993, 2009) states that a set of variables, Z, is sufficient for estimating the causal effect of X on Y if variables in Z block all backdoor paths from X to Y. To block a backdoor path between X and Y, the path must be "d-separated" (Pearl, 1988). A path between X and Y can be d-separated if either:

1. the path contains at least one arrow-emitting variable that is in Z or
2. the path contains at least one collider variable (a variable with two incoming arrows, for example, B is a collider variable in $A \rightarrow B \leftarrow C$) that is outside Z and has no descendant in Z.

For our DAG, there are two backdoor paths between MPA and reef fish biomass that must be d-separated (i.e., blocked):

1. MPA ← Structural Complexity → Reef Fish Biomass
2. MPA ← Depth → Fishing Pressure → Reef Fish Biomass

The first backdoor path can be blocked by adjusting for structural complexity, and the second backdoor path can be blocked by adjusting for depth. Therefore, to block all backdoor baths, we must adjust for both structural complexity and depth.

We note that the application of the backdoor criterion can become complicated as we move on to larger and more complex DAGs (see Appendix S1: Section S1.3 for example). In some cases, more than one adjustment set may be available to determine the causal effect of X on Y. In these scenarios, it is best to choose the adjustment set with the lowest measurement error. Other times, the adjustment set(s) required may not be available due to the presence of unmeasured variables. To avoid this scenario, we recommend that researchers think critically and draw potential DAGs before collecting observational data. Given that the application of the backdoor criterion can become difficult to apply for increasingly complex DAGs, researchers can draw their DAG on www.daggity.net (instructions within site), which will apply the backdoor criterion and generate the adjustment set(s) required to determine causal effects, given a specified DAG and causal question.

Once the backdoor criterion is applied to determine the sufficient set for adjustment, we must choose an appropriate statistical model for analysis. Directed acyclic graphs are used to guide covariate selection (i.e., which variables to adjust for) and are not the estimation method; therefore, ecologists must select the statistical approach that best fits their data and study question. Directed acyclic graphs are nonparametric, meaning that they make no assumptions about the distribution of variables (e.g., normally distributed) or the functional form of causal effects (e.g., linear, nonlinear, and stepwise). As such, they are compatible with a wide range of statistical analysis (e.g., generalized linear model or hierarchical Bayesian model). Here, since our simulated data were created using linear relationships, we have applied a GLM. Our GLM specifies reef fish biomass as the response variable, MPA as the predictor, and includes depth and structural complexity as controls. We interpret the coefficient of MPA as our total causal estimate on reef fish biomass. Here, our causal estimate of MPA on reef fish biomass returned an accurate estimate of 1.17 [1.08, 1.26], with the 95% CI including the true causal effect of 1.089 (Appendix S1: Section S1.4).

It is important to note that covariate selection based on the backdoor criterion eliminates common statistical biases including confounding, overcontrol, and collider bias that often plague observational studies

(Elwert, 2014; Pearl, 2009). Confounding bias occurs when a common cause between predictor and response is not adjusted for. Given our DAG, if no adjustments are made, confounding bias would arise from depth and structural complexity, which effect both MPA and reef fish biomass. Indeed, a GLM with no adjustments gave an inaccurate estimate of 3.40 [3.32, 3.48] for the effect of MPA on reef fish biomass (Appendix S1: Section S1.5).

Lesser known, but equally important, are overcontrol and collider bias. Overcontrol bias occurs when an intermediate variable between predictor and response is adjusted for, blocking the causal association between predictor and response. Given our DAG, adjusting for fishing pressure would lead to overcontrol bias, giving an inaccurate estimate of −0.10 [−0.16, −0.04] for the MPA effect, even when depth and structural complexity are adjusted for (Appendix S1: Section S1.5). Collider bias occurs when a variable affected by both predictor and response is adjusted for, creating a spurious association between predictor and response. Given our DAG, adjusting for coral cover would lead to collider bias, giving an inaccurate MPA estimate of −0.13 [−0.15, −0.11] (Appendix S1: Section S1.5). Collectively, the application of the backdoor criterion eliminates all three statistical biases, allowing for accurate causal estimates.

The SCM framework can be employed across a range of observational ecological studies (e.g., Cronin & Schoolmaster, 2018; Grace & Irvine, 2020; Schoolmaster Jr et al., 2020). Importantly, causal diagrams (DAGs) and the principles of SCM (e.g., the backdoor criterion) can be applied to ensure proper study design across other quasi-experimental approaches that have gained traction within ecology (Butsic et al., 2017; Larsen et al., 2019), including propensity score analysis, BACI studies, RDD, and IV.

MATCHING METHODS

Matching methods are often employed to remove confounding bias within observational studies (Stuart, 2010). Matching methods aim to balance the distribution of covariates between treatment and control groups (Figure 2a). Covariates placed in a matching procedure should include confounding variables assumed to affect both treatment assignment (e.g., MPA placement) and the outcome (e.g., reef fish biomass), thereby minimizing confounding bias (Rubin and Thomas, 1996). Implementing matching methods first requires the selection of a distance measure, used to define how close two units are based on selected covariates. Several distance measures are available to researchers, including propensity scores and Mahalanobis distance (Stuart, 2010). Distance measures are subsequently used to match treatment and control units, which can be done through several matching methods including nearest neighbor matching, optimal matching, and exact matching (Stuart, 2010).

Matching methods have been employed across a range of ecological systems to determine the causal effect of treatments, including the effect of protected areas on natural forests (Andam et al., 2008; Herrera et al., 2019) and freshwater species (Chessman, 2013), the effect of agriculture on stream ecosystems (Pearson et al., 2016), and the impact of invasive species management on tree condition (Ramsey et al., 2019). However, although past studies state the confounding variables used in their matching procedure, it is unclear how these confounding factors interact with one another within the broader causal structure of a study system. Without this knowledge, it is unclear whether there are unobserved or unmentioned variables that need to be included in the matching analysis or whether the inclusion of all selected variables may lead to other forms of bias (e.g., overcontrol or collider bias).

To resolve these issues, matching methods can make use of causal diagrams (i.e., DAGs). As previously noted, after creating a DAG and ensuring DAG-data consistency, researchers can apply the backdoor criterion to determine which covariates need to be adjusted for to determine the causal effect of X on Y. Under matching procedures, the set of covariates that enter the matching procedure must satisfy the backdoor criterion (Pearl, 2009). For example, if we choose propensity score matching, then given our DAG (Figure 1), to determine the effect of MPA on reef fish biomass, depth and structural complexity must enter the propensity score for MPA placement (Appendix S1: Section S2). To ensure covariate balance is achieved, we can employ balancing tests, which are often applied across matching procedures (Appendix S1: Section S2). When this is done, and our propensity score is used as a covariate adjustment, we return an accurate causal estimate for MPA of 1.17 [1.07, 1.27] (Appendix S1: Section S2).

Employing the backdoor criterion also eliminates other forms of bias that can occur within matching methods, including overcontrol and collider bias (Mansournia et al., 2013; Pearl, 2009). For example, if all available variables entered our propensity score, this would ultimately lead to overcontrol bias (due to the inclusion of an intermediate variable, fishing pressure) and collider bias (due to the inclusion of a collider variable, coral cover), giving an inaccurate MPA causal estimate of −0.19 [−0.29, 0.11] (Appendix S1: Section S2). Additionally, DAGs include both measured and unmeasured variables needed to depict a complete causal structure, thereby explicitly stating any missing variables that must be considered. This is critical as

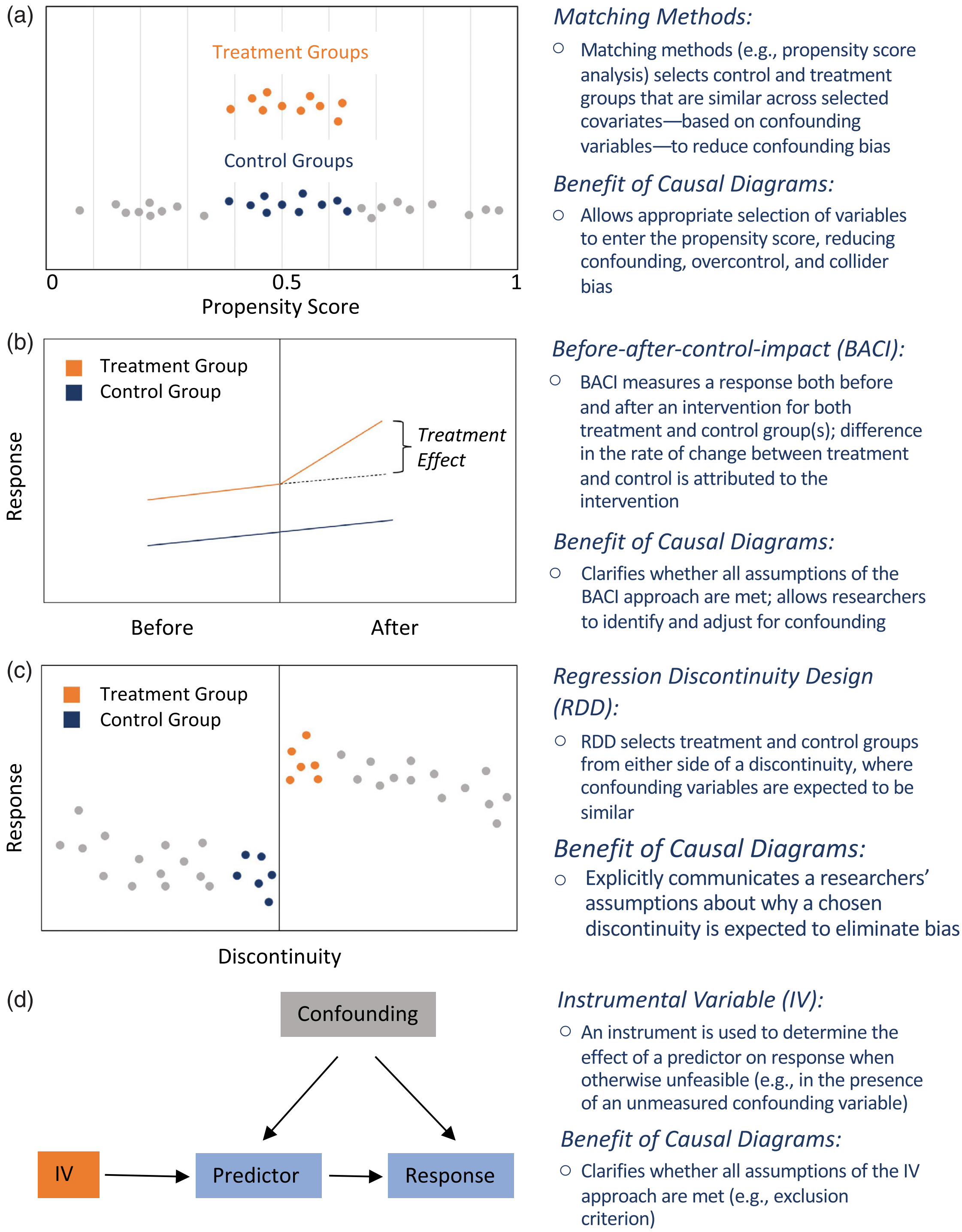

FIGURE 2 Quasi-experimental approaches: (a) propensity score analysis, (b) before-after-control-impact (BACI), (c) regression discontinuity design (RDD), (d) instrumental variable (IV)

the omission of unmeasured variables required in a backdoor adjustment set can lead to bias (Pearl, 2009). Collectively, the application of the backdoor criterion on DAGs helps determine covariates that must and must not enter a matching procedure, while also communicating the model's assumed causal structure.

BEFORE-AFTER-CONTROL-IMPACT

If observational data are available both before and after an event, BACI (Green, 1979) designs can be used to assess the effect of interventions, including anthropogenic disturbances or environmental management actions. Before-after-control impact works by measuring a response (e.g., reef fish biomass) both before and after an intervention (e.g., MPA placement) for both treatment and control site(s). Before-after-control impact rests on the assumption that trends in the treated and control groups would be identical if the intervention did not occur, meaning any difference in the rate of change between treatment and control is attributed to the intervention (Figure 2b).

Before-after-control impact and its extensions (e.g., BACIPS, Stewart-Oaten et al., 1986; Progressive-change BACIPS, Thiault et al., 2017) have been applied across various ecological studies, including to determine the effects of invasive species on invertebrates (Kadye & Booth, 2012), restoration programs on biota (Bousquin & Colee, 2014; Suren et al., 2011), and MPAs on coral reef fish communities (Thiault et al., 2019). Wauchope et al. (2021) further show how to analyze BACI data to determine *trend* and *immediate* change, in addition to average change, which may better capture ecological responses to interventions. However, although BACI study designs have the potential to provide valid causal inference, past reviews have noted the prevalence of improper study design, where the consideration of all relevant variables is often neglected, particularly joint consideration of both ecological and human factors (Adams et al., 2019; Ferraro et al., 2018). Here, DAGs can allow researchers to consider all relevant variables and clarify the assumptions required for appropriate BACI studies.

Let us consider a standard BACI design, which is also referred to as difference-in-difference in some fields (Wauchope et al., 2021). Given our asserted DAG and the application of the backdoor criterion, we know that depth and structural complexity are confounding variables that must be accounted for to determine the effect of MPA on reef fish biomass. A strength of a BACI design is that it already accounts for certain confounding variables. Confounding in BACI designs occurs only if a variable (1) effects the treatment group and (2) has an effect on the outcome *trends*, which can occur when a variable has a time-varying difference between treatment groups or a time-varying effect on the outcome (Zeldow & Hatfield, 2021). In our simulation, neither depth nor structural complexity have a time-varying difference between treatment groups or a time-varying effect on the outcome, so the application of a BACI analysis will return an accurate causal estimate for MPA of 1.07 [0.96, 1.20], without the need to adjust for these variables (Appendix S1: Section S3). Critically, when designing BACI studies, researchers must ensure that the variables in a backdoor adjustment set are accounted for, either by design or through appropriate statistical adjustments. For example, if a bleaching event occurred after initial MPA placement, and disproportionately reduced the structural complexity across MPA sites, structural complexity would now act as a confounding variable by having a time-varying difference between treatment groups. Under these circumstances, our BACI analysis returns an inaccurate causal MPA estimate of 0.19 [0.08, 0.30; Appendix S1: Section S3]. However, we can return an accurate estimate of 1.06 [0.97, 1.16] by making the appropriate adjustment for structural complexity (Appendix S1: Section S3). We refer readers to Zeldow and Hatfield (2021), who provide instructions on how to adjust for confounding variables, when they do arise in BACI studies.

Given the need for proper study design (Adams et al., 2019; Ferraro et al., 2018), using causal diagrams to guide BACI studies will ultimately lead to more impactful and accurate causal estimates due to the extra care taken to understand where and when causal assumptions can be met. In addition, researchers can also employ placebo tests used in BACI studies to further support their causal conclusions; for example, researchers can apply a BACI analysis using only pretreatment data, which should show a lack of causal effect (e.g., Schnabl, 2012). As such, the integration of causal diagrams with BACI can lead to additive methods for supporting causal claims, which in turn lead to more comprehensive causal conclusions.

REGRESSION DISCONTINUITY DESIGN

Regression discontinuity design aims to minimize the effect of confounding bias by exploiting a discontinuity in either space, time, or policy to separate observations into treatment and control groups (Imbens & Lemieux, 2008). The key assumption is that at or near this discontinuity, confounding variables are equal between treated and control groups. If the underlying confounding variables are similar before and after the change, then the treatment effect can be estimated by comparing the average difference between treated and control groups (Figure 2c). Although past review papers have highlighted the potential of RDD in ecology (Butsic et al., 2017), it remains underutilized. We could find only one example of its use for causal inference, a conference paper studying the effect of protected areas on deforestation, population settlements, and road infrastructure that used the border of protected areas as the discontinuity with treatment and control groups being comprised of study sites from each side (Perez et al., 2017).

Despite limited use to date, RDD provides a strong causal inference approach across ecological studies whenever there exists a sharp break between treatment groups across observational units, including protected area borders, fishing and land use zones, species ranges, and soil types (Butsic et al., 2017). Yet, here again causal

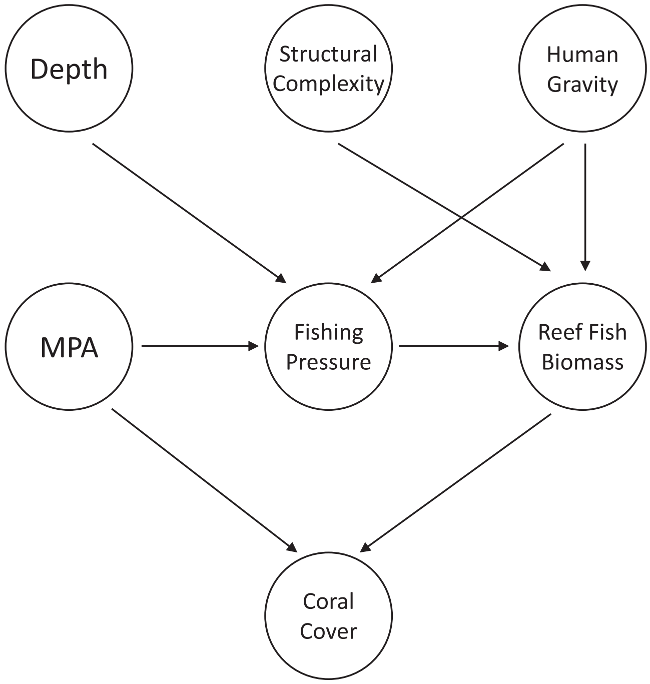

FIGURE 3 An alternative directed acyclic graph (DAG) representing the effect of a marine protected area (MPA) on reef fish biomass under a regression discontinuity design where only data near a discontinuity (MPA border) are considered

diagrams should be utilized to visualize how exploiting a discontinuity can break the backdoor (i.e., noncausal) paths between predictor and response. For example, to determine the effect of MPA on reef fish biomass, we can use the MPA border as our discontinuity if confounding variables (depth and structural complexity) are approximately the same on either side and fish do not readily move across the boundary. Figure 3 uses a DAG to represent the causal structure between MPA and reef fish biomass within this kind of RDD design. Here, our observational data come only from our discontinuity range, on either side of MPA border. As such, depth and structural complexity no longer act as confounding variables, meaning the effect of MPA on reef fish biomass can be estimated without needing to adjust for additional variables (i.e., there are no backdoor paths between MPA and reef fish biomass within this discontinuity). A simulated dataset using data only from this discontinuity (following the causal structure in Figure 3) returned an accurate causal estimate of MPA on reef fish biomass, 1.08 [0.66, 1.49] (Appendix S1: Section S4).

Visualizing RDD with causal diagrams is particularly important in ecology due to the complex nature of causal connections that may exist near a chosen discontinuity. Directed acyclic graphs allow researchers to visualize the causal structure near a discontinuity, to help ensure proper study design. Although underutilized, well thought out RDD communicated through causal diagrams can provide effective and transparent observational causal inference and should be more routinely used. Placebo tests used within RDD studies (e.g., using pretreatment variables as placebo outcomes) can further be employed to provide additional support for causal conclusions (Eggers et al., 2021).

INSTRUMENTAL VARIABLES

An IV approach (Kendall, 2015) can be used to determine the effect of X on Y in the presence of an unmeasured confounding variable, leading to confounding bias, or bidirectional relationships, which can generate simultaneity bias. For example, the DAG in Figure 2d shows that the effect of predictor on response cannot be determined from a simple regression analysis due to the presence of an unmeasured confounding variable. In such cases, an instrument, Z, can be used to determine the effect of X on Y if it meets three requirements (Hernan & Robins, 2006). First, Z must be correlated with the predictor variable; the stronger the correlation, the more effective the instrument Z will be. Second, Z must not have a *direct* causal effect on the response variable and must only be associated with outcome Y through X, known as the *exclusion criterion*. Third, there must be no confounding variables that affect both Z and Y. If these three requirements are met, Z can be used as an instrument to determine the effect of X on Y through a two-stage regression (Kendall, 2015).

Finding an instrument that satisfies all three criteria can be difficult practice, which may limit its use in ecological studies. However, when applicable, IV remains a powerful technique that can be used to prevent confounding and simultaneity bias across observational ecological studies. Already, several implementations of IV exist within the ecological literature: Busch and Cullen (2009) used site accessibility measures as instruments to determine the effectiveness of endangered species recovery treatments; Amin et al. (2015) used biodiversity as an instrument to determine the effect of protected areas on deforestation; and Butsic et al. (2015) used multiple instruments to determine the effect of warfare, mining, and protected areas on deforestation.

When implementing IV, causal diagrams should be drawn to depict the complete causal structure of a system under study and accurately assess whether a chosen instrument meets the necessary requirements. For example, Figure 4a depicts a DAG where X and Y are confounded by an unmeasured variable U. Here, our instrument, Z, does not initially satisfy the exclusion criterion because it effects Y through another intermediate variable, V: $Y \leftarrow V \leftarrow Z$. To satisfy the exclusion

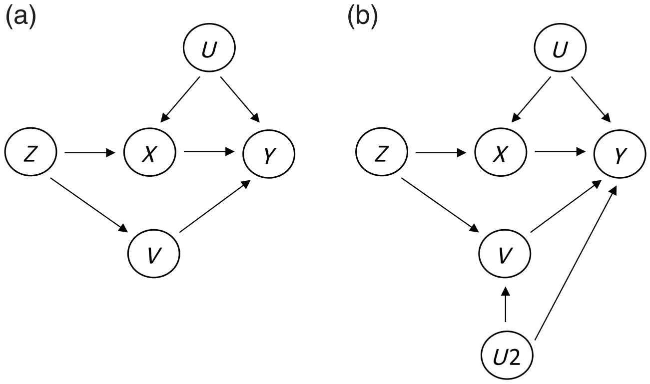

FIGURE 4 Directed acyclic graphs (DAGs) under two instrumental variable scenarios: (a) represents a scenario where an instrument, Z, can be used to determine the effect of X on Y, while (b) represents a scenario where an instrument, Z, cannot be used to determine the effect of X on Y because it cannot meet the exclusion criterion

criterion, we must block (d-separate) this pathway by adjusting for V. Once this is done, Z can be used as an instrument to determine the effect of X on Y. We can additionally test our causal assumption that our IV, Z, is sufficiently correlated with our predictor variable, X, by testing against weak instruments, which is commonly employed across IV studies (e.g., Staiger & Stock, 1997; Appendix S1: Section S5). Following this, an IV approach on our simulated data, using Z as our instrument and adjusting for V, returns an accurate causal estimate of 1.11 [1.07, 1.15] for X on Y (known causal estimate of 1.089; Appendix S1: Section S5).

As another example, the DAG in Figure 4b also requires adjustment for V to block the additional path from Z to Y: $Y \leftarrow V \leftarrow Z$. However, in doing so, we open another path between Z and Y: $Y \leftarrow U2 \rightarrow V \leftarrow Z$. V acts as a collider variable (variable with two incoming arrows) in this path, which we open when adjusting for it. To block this additional path, we must also adjust for $U2$. However, $U2$ is an unmeasured variable and therefore cannot be adjusted. In this scenario (Figure 4b), Z cannot act as an instrument to measure the effect of X on Y. A two-stage regression that did not adjust for $U2$ returned an inaccurate estimate of 0.73 [0.69, 0.78] for X on Y (known causal estimate of 1.089; Appendix S1: Section-S5). By utilizing causal diagrams, we explicitly communicate our assumptions about the causal structure between an instrument, predictor, and response variable, and accordingly, ensure that the assumptions required for an IV approach are satisfied. As such, the use of causal diagrams can lead to more accurate implementation of the IV approach.

CONCLUSION

Although causal diagrams are underutilized within ecology, they hold tremendous potential for guiding effective causal inference across a range of observational contexts. Here, we have highlighted their utility across four additional quasi-experimental approaches, showing how the use of causal diagrams clarifies and unifies variable selection in nonexperimental settings. Their use will also help to produce more transparent communication about causal assumptions, leading to more critical and accurate discussion about the conclusions that can be drawn from ecological research. Further, the integration of causal diagrams with quasi-experimental methods leads to additive methods for supporting causal claims (e.g., balancing tests for matching methods, placebo tests for BACI and RDD designs, and test for weak instruments for IV approaches), which can lead to more comprehensive causal analysis. Utilizing causal diagrams across quasi-experimental methods can lead to more accurate and comprehensive causal analysis. The consequences of such a change are profound—from management and policy decision making, to the development of ecological laws, ecology must embrace a causal understanding of our data-rich and radically changing natural world.

CONFLICT OF INTEREST

The authors declare no conflict of interest.

DATA AVAILABILITY STATEMENT

Code (Arif 2022) is available from Figshare: https://doi.org/10.6084/m9.figshare.18166682.

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