EDITOR'S COMMENTS

Causality Meets Diversity in Information Systems Research

By: Sunil Mithas, Senior Editor
Ling Xue, Associate Editor
Ni Huang, Associate Editor
Andrew Burton-Jones, Editor-in-Chief

As outlined in our March 2022 editorial, we are using this year's editorials to help authors and reviewers understand how editors at MISQ think about key issues in manuscripts they handle. We have identified several issues that we believe would benefit from guidance.[1] This one is dedicated to the topic of *causality in quantitative IS research*. There are many views on this topic, and we hope that the editorial spurs further dialogue.

We need to start with what we do and do not cover. Our focus in this editorial is on how knowledge of causality can help quantitative researchers explain an outcome of interest and, especially, *how different views of causality enable different explanations* (Salmon, 1998). This focus requires us to exclude many other important aspects of causality. In particular, we do not examine the causal relationships between constructs and indicators (i.e., questions of construct validity) (see Borsboom et al., 2004) but instead focus on causal relationships among constructs (i.e., questions of internal validity) (Shadish et al. 2002). We also do not discuss the roles of causality in qualitative research (e.g., Avgerou, 2013) or in experiments or machine learning research, which are covered in other MISQ editorials/articles (Gupta et al., 2018; Karahanna et al., 2018; Padmanabhan et al., 2022). Many studies also use traditional econometric approaches for assessing relationships (Greene, 2000; Heckman & Pinto, 2022; Wooldridge, 2002), sometimes, but not always, attributing causal claims to them. Because causality is not always the main focus of such approaches, we do not discuss them either. All these topics are relevant to discuss, but we can only cover a limited scope. With this scope in mind, let us explain what motivated us to write the editorial.

First, there is *an increased need* to address causal questions in IS research. The phenomena that we study are among the most important phenomena in business and society, and their importance is increasing all the time (Brynjolfsson et al., 2021). Executives and policy makers should be able to turn to IS research for answers to causal questions, and we should have appropriate ways of discharging this responsibility. While issues of causality will not be central to every paper, they will be central to some papers, and so we need authors and reviewers to be aware of the discourse related to causality in IS research and elsewhere.

Second, while the explosion of attention to causality across the social sciences over recent decades has been important, we have also seen several *unwelcome consequences*. One unwelcome consequence is the seemingly endless checks that reviewers sometimes ask authors to conduct or that authors think they must include. While some papers require an intense focus on causality, others do not. For instance, we often see authors providing (or reviewers requesting) "yet another" robustness check without a valid reason. This can easily lead to an "arm's race" in which authors and journals are judged by how rigorously authors check for causality. Relatedly, we have the uneasy feeling that far too many researchers prefer to work with data needed to pass the latest tests for causality rather than starting with important research questions where causality may be harder to establish. In all these cases, we must remember that while knowledge of causality *serves* our interests, it is not the *main* interest. After all, this is *MIS Quarterly*, not *Causality Quarterly*. Another unwelcome consequence is that too many researchers overspecialize in their understanding of causality, focusing on one view without appreciating the value of others (or even the value of noncausal research). Whether it is because of their training or just the sheer difficulty of keeping up with the literature on different views of causality, overspecialization can create two further problems: scholars may review a paper that adopts one view of causality through the lens of their preferred-but-different view, leading to inappropriate review comments, or scholars may simply fail to appreciate the work done by scholars who adopt other views on causality, impeding cumulative progress.

[1] For each editorial, we are following a three-stage process. The editor-in-chief (Andrew) first selects editors with expertise on the topic to run a masterclass for our editorial board. These editors include scholars with varying levels of editorial experience to allow for differences in views. In this case, Sunil Mithas is a second-term SE, Ling Xue is a second-term AE, and Ni Huang is a first-term AE. The same group then runs an online seminar for authors. We then incorporate the learning from both sessions into this editorial. These editorials are not offered to provide the "one true view" of the topic; they simply reflect the views of a subset of editors (the authors) at the time of the writing.

This editorial is a call to appreciate diverse views of causality. Our remarks are necessarily brief. We simply cover three views for thinking about causality that we consider particularly relevant to IS research. We make no claim that they are the only views, and recognize that there are continuing debates about their relative merits. There are also diverse perspectives within each view. We sidestep these complexities to focus on our main theme, but we provide references that cover these details. Overall, we hope the editorial will help you to appreciate the diversity of causal thinking relevant to our field and the possibilities that such diversity offers for continued growth in understanding.

History and Background on Causality

"I would rather discover one causal relation than be King of Persia." – Attributed to Democritus (430-380 BC)

Causality has received the attention of philosophers and scientists for centuries (Barringer et al., 2013; Hume, 1740/1977). Most doctoral students learn early on that causality needs at least three types of evidence: covariation, temporal precedence, and elimination of rival explanations (Shadish et al., 2002). They also learn of the importance of having a theory or mechanism to explain a relationship (Cornfield, 1959; Pearl & Mackenzie, 2018; Singleton & Straits, 2018). Given the complexity of the topic, however, it is almost impossible for any student to know the causality literature fully (Losee, 2011) or to offer an overview (or even a definition) that everyone will consider fair or complete.

IS researchers have attended to causality since the field's early days (See Mithas et al., 2014 for a review). Lucas (1978) was the first to mention the word "causality" in *MISQ* in his study of the drivers of successful IS implementations. In related work, he stressed the importance of research design when addressing causal questions (e.g., with longitudinal and experimental designs) (Lucas, 1975, 1980). Other references to causality in IS research include discussions of the importance (and difficulty) of conducting experiments with firms (Banker et al., 1990), the limitations of approaches for testing causality (Lee et al., 1997), and the links between causality, theory, and research questions (Gregor, 2006; Grover et al., 2020; Markus & Rowe, 2018).

For a long time, philosophers examined causality at a very abstract level, quite distant from the world of empirical research. This changed in the late 1800s, when much attention turned to analytical, econometric, and statistical approaches relevant to frontline research (Bunge, 2009). Fast-forwarding to today, so much progress has been made on these analytical and statistical approaches that it is hard to keep up with the literature. While excellent reviews of the literature exist (Angrist & Krueger, 1999; Heckman & Pinto, 2022; Imbens & Rubin, 2015; Imbens & Wooldridge, 2009; Morgan & Winship, 2015; Ragin, 2014; Rosenbaum, 2002), it is important to recognize that these reviews typically cover causality from just one perspective.[2] As a result, unless readers are careful, they can easily miss other important perspectives on causality.

Considering the diversity of the IS field, this editorial covers three perspectives on causality that we believe are particularly relevant to the IS field: *path analytic*, *potential outcomes*, and *configurational.* While some researchers may use different labels for them, we use these labels merely as "short-hands" to characterize these views, in a similar spirit to Cronbach (1957) who distinguished between "experimental" and "correlational" psychology. The three views have different applicability for different types of causal questions and IS researchers will benefit from a pluralistic conception of causality that appreciates the role of each one. We recognize that some believe that these views are not on the same footing, and some may have strong tastes and preferences for one or the other; we leave such debates and discussions for appropriate outlets and fora.

The *path analytic* view of causality drew on analytical work from the 1920s and grew rapidly in the 1970s with software like LISREL (Bollen, 1989; Bollen & Pearl, 2013). Early examples of this view in IS research include studies of staffing (e.g., Moore, 1979). The *potential outcomes* view of causality uses the logic of experimentation to analyze causal effects in observational studies (Rubin, 1974, 1978). It attracted significant attention in statistics (Holland, 1986), epidemiology (Little & Rubin, 2000), sociology (Sobel, 1996; Xie, 2012), and economics (Heckman et al., 1997), and it has found application in IS research as well (e.g., Chen et al., 2018; Mithas et al., 2006). Quasi-experimental methods using this view led to the Nobel prize in economics in 2021 (Hannon & Harrison, 2021).[3] Finally, the *configurational* perspective stems from political science and sociology along with earlier work in philosophy (Mackie, 1965; Ragin, 2000). Of the three views, it is the most recent to appear in IS research (El Sawy et al., 2010; Levallet et al., 2021; Park et al., 2020; Park & Mithas, 2020).

[2] Even in other business disciplines, reviews of causality tend to focus mostly on only one view, and typically the potential outcomes view and/or traditional econometric view (e.g., Gow et al., 2016; Ho et al., 2017; Mithas et al., 2022; Roberts & Whited, 2013).

[3] The rapid evolution of the literature can be seen in how it is treated in econometric textbooks. For instance, although the 2000 edition of a widely used econometrics text (Greene, 2000) does not have index entry for "potential outcomes" or "counterfactual" and does not refer to the work of Rubin, Card, Angrist, or Imbens, newer editions make significant references to this body of work.

Three Different Views of Causality

Having introduced the three views, we now take a closer look at each one. Consider Figure 1, which shows an illustrative causal model of the relationship between D and Y.

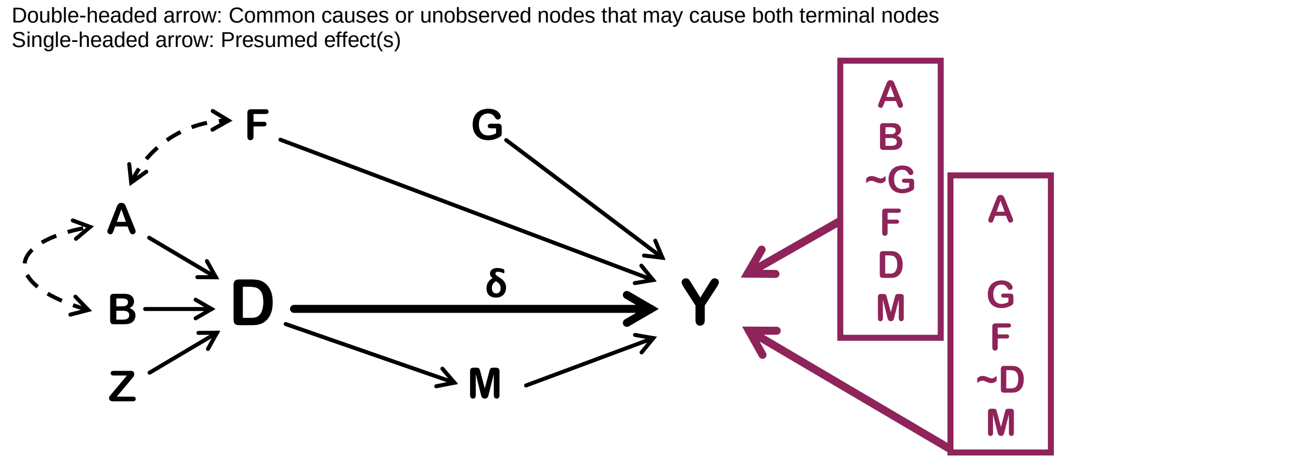

Figure 1. Model to Illustrate Different Views of Causality (Adapted from Morgan & Winship, 2015)

The parts to the left of Y in Figure 1 show two alternative views of causality (the path analytic view, and the potential outcomes view). In many ways, this part of Figure 1 resembles the kind of path analytic model seen in many published papers in IS and other fields. The path analytic view specifies a network of relationships linking the intervention such as D (which need not be binary and can include multiple variables) with the outcome via various mediating pathways such as M. The potential outcomes view mostly focuses on the overall or total causal effect of D (typically binary[4]) on the outcome Y, while accounting for other observed variables (e.g., A, B, G, F), avoiding bad controls, and ruling out alternative explanations by considering variables that were unobserved but may account for the causal relationship of interest. In contrast to the potential outcomes view, which focuses on estimating just one causal effect, the path analytic framework tends to characterize many (sometimes even all) paths as causal.

Finally, the right side of Figure 1 shows the configurational view, where causal conditions (e.g., A, B, G, F, D, M) configure in different ways to result in Y. Later, we use Figure 3 with a subset of variables to further illustrate the configurational view, which uses set theory and necessity/sufficiency conditions to conceptualize causal relationships. The core idea is that an outcome of interest is presumed to stem not from just one causal condition (e.g., D, the new IS or whatever intervention one has in mind in a potential outcomes view) but instead from a constellation of conditions. The researcher in the configurational view seeks to learn the roles and often conjunctural and asymmetric effects of causal conditions in different configurations using set-theory and Boolean algebra, instead of covariances that undergird both the path analytic and potential outcomes approaches.

Table 1 provides more details on each view of causality. In the following sections, we further elaborate these distinctions.

Path Analytic View of Causality

The path analytic view of causality refers to a diverse family of methods for causal inference that broadly conceive of causality in terms of graphical models of variables and paths (typically reflecting presumed theoretical structures). This framework has a strong tradition in sociology, psychology, and economics (Duncan, 1966; Goldberger, 1972; Heckman & Pinto, 2022; Wright, 1921). Examples of path analytic models include structural equation modeling (SEM), mediation and moderation models, and directed acyclic graphs (DAGs), among others (Bollen, 1989; Bollen & Pearl, 2013), although DAGs can also be accommodated in the potential outcomes view (Imbens, 2020; Morgan & Winship, 2015).

---

[4] Imbens and Wooldridge (2009, p.8) note that most of the literature "has largely focused on binary treatment case … some extensions of these methods to multivalued, and even continuous treatments … is ongoing, and much remains to be done there."

Table 1. Interplay between Different Views of Causation and Types of Research Questions		
Causal view	**Main focus, and typical research questions**	**Key concepts and references**
Path analytic	• What are the causal paths that explain the effect of a treatment on an outcome, and is it mediated, moderated, or both? If so, how? • Example: ○ *How does* IT capability affect firm performance via other organizational capabilities?	Concomitant variation, structural equation modeling (SEM), LISREL / PLS, directed acyclic graphs, measurement and structural models, recursive and nonrecursive models, Granger causality[5] (Bollen, 1989; Duncan, 1966; Goldberger, 1972; Jöreskog, 1978; Pearl, 1998; Wright, 1921)
Potential outcomes	• How much does the outcome change when the treatment changes? • Example: ○ *Does* IT capability increase firm profits or shareholder value compared to if the firm did not possess IT capability?	Rubin's causal model (RCM), matching, Instrumental Variables, Regression Discontinuity Design (RDD), fixed effects, difference-in-differences (DID), mechanism-based causal inference, sensitivity analysis (Angrist & Pischke, 2009; Fisher, 1935; Imbens, 2020; Imbens & Rubin, 2015; Splawa-Neyman, 1923/1990; Pearl, 2000; Rubin, 1974)
Configurational	• What sets of conditions lead to an outcome of interest and in what configurations does a particular factor of interest lead to the outcome? • Example: ○ *What configurations* of IT capabilities and other organizational capabilities create high (vs. low) performance?	Conjunctural causation, regularity, constant conjunction, complex causality (necessary/ sufficient conditions, INUS), sets and Boolean Algebra (Mackie, 1965; Mill, 1843/1882; Ragin, 1987)

Path analytic models enable the estimation of direct, indirect, and moderated effects, along with complex combinations of these effects, including longitudinal and multilevel designs for exploring underlying mechanisms or boundary conditions of theories (Raudenbush & Bryk, 2002; Zhao et al., 2010).[6] This ability to understand the mechanisms underlying relationships is important as it offers evidence for causal explanations. Path models and mediation analyses have also been combined with experimental studies to help explain and test theoretical explanations or underlying mechanisms of observed relationships (Huang et al., 2021). There have also been attempts to reconcile path models with the potential outcomes framework in mechanism-based causal inference (Morgan & Winship, 2015). In particular, the application of mediation analyses to uncover underlying mechanisms for observed relationships has led to methodological developments that extend traditional linear structural models with the language of potential outcomes (Imai et al., 2010; Imai, Keele, & Yamamoto, 2010; Peng, in press).

In terms of advantages, path analytic models are particularly helpful for answering "why" and "how" questions not just "whether" or "what" questions. In other words, path models focus on the causal explanations and the "molecular" causation of relationships, rather than the identification of "molar" causal effects (Shadish et al., 2002). Additionally, path models emphasize model-data consistency to test whether the patterns in the data match the causal assumptions of the theory (Bollen & Pearl, 2013).

In terms of disadvantages, path analytic models can sometimes appear unprincipled or ad hoc if there are inadequate examinations of the inherent causal assumptions in the models some of which may not be "directly testable" (Pearl, 1998, p. 230). Further, when building and testing path models, there is a strong reliance on the global tests of model fit to the data, and some plausible alternative models are often not considered (Vandenberg, 2006). Some find path analytic models problematic if they have a strong view on "no causation without manipulation" (Holland 1986, p. 959), but others suggest that path models are not incompatible with the manipulation view since manipulations/exogenous variations can be incorporated into path models (Bollen & Pearl, 2013). Other ongoing discussions relate to the relative merits of covariance-based versus partial least squares SEM (Aguirre-Urreta & Marakas, 2014; Goodhue et al., 2012; Rigdon, 2022; Rönkkö et al., 2016), use of DAGs with experimental data (Tafti & Shmueli, 2020), and the strengths and weaknesses of DAGs vis-à-vis the potential outcomes view (see Imbens, 2020).

[5] We include Granger causality in path analytic view in the sense that it focuses on robust dependence. We recognize that some of the references and notions sometimes span across different views.
[6] Many researchers using the path analytic approach focus on relationships between indicators of a construct and that construct (the measurement model) *as well as* the relationship(s) among the constructs of interest (the structural model), recognizing that causal claims can be involved in both steps (Borsboom et al. 2004). As noted at the outset of this editorial, we only address the structural model here, but we note that a key focus of the path analytic approach is the ability to address both aspects (Bollen, 1989).

Overall, path analytic models can be quite useful for theory building and testing, and they are especially helpful to understand the underlying mechanisms (MacCallum & Austin, 2000). The associations in path models do not imply causality in and of themselves; rather, each path model "represents and relies upon the causal assumptions of the researcher. These assumptions derive from the research design, prior studies, scientific knowledge, logical arguments, temporal priorities, and other evidence that the researcher can marshal in support of them" (Bollen & Pearl, 2013, p. 15).

Potential Outcomes View of Causality

The core of the potential outcomes (or, counterfactual) view is to consider that each treatment state of a subject is associated with a potential outcome, and to assess causality based on the relationship between treatment assignment and the difference in potential outcomes. Even when a treatment can be randomly assigned in an ideal experimental setting, each subject can still be observed in only one treatment state at any point in time, and not all potential outcomes are observable (the counterfactuals) creating the fundamental problem of causal inference.

The potential outcomes view clarifies two critical challenges in assessing causality (*baseline bias* and *differential treatment effect bias),* and defines various types of causal effects that one can estimate based on the underlying assumptions and availability of data (Mithas et al., 2022; Morgan & Winship, 2015). Baseline bias arises when the counterfactual outcomes for the treated unit in the untreated state differ from the observed baseline outcomes for the control units, likely due to omitted or unknown variables (typically denoted by U), reverse causality, or simultaneity. Likewise, differential treatment effect bias may also render "naive" or standard estimates of causal effects biased with observational data if those who decided to be in the treatment were more likely to benefit from it, even if one assumes that there were no baseline differences in the absence of the treatment. Note that randomization in experimental studies addresses *both* baseline bias and differential treatment effect bias by making both the unobserved factors and potential outcomes independent of the treatment status enabling the estimation of average treatment effect (ATE). However, the lack of randomization in observational studies raises issues related to the self-selection of units into treatment groups based on observables and/or unobservables, which typically limits researchers to estimating the average treatment effect on treated (ATT) or even narrower treatment effects, such as the local average treatment effect (LATE).[7]

Figure 2 illustrates how the potential outcomes perspective addresses issues related to selection on observables and/or selection on unobservables. As illustrated in Figure 2a, a single causal effect can be represented using a directed arrow from the treatment D (e.g., enterprise resource planning (ERP) is implemented) to the outcome Y (e.g., firm performance). This causal effect can be confounded by the unobservable factor U that influences (through dotted lines) both the treatment D and the outcome Y. From the potential outcomes perspective, different causal identification approaches employ different types of data (e.g., cross-sectional, panel/longitudinal, quasi-experimental) to address the issue of the unobservable confounding effect. Despite their technical differences, the common objective of these approaches is to disentangle the effects of unobservables from the causal effect D→Y, so that the causal effect can be identified.

Morgan and Winship (2015) classify the various approaches for identifying causal effects into three broad categories:

(1) Conditioning on observed variables to block back-door paths using Pearl's (2000) framework

(2) Alternative approaches when back-door conditioning is ineffective—these include instrument variables (IVs), front-door mechanisms, and approaches that use repeated observations (e.g., fixed effects, DID) or quasi-experimental designs (e.g., RDD)

(3) Sensitivity analysis and set identification when point identification is not possible.

Figures 2b-2e illustrate some of these approaches (see Table 2 for a summary).

[7] As Morgan and Winship (2015) explain, the focus on "more narrowly defined treatment effects" (p. 39) is consistent with the need to finely articulate "well-defined causal states for … narrow causal effects" (p. 38).

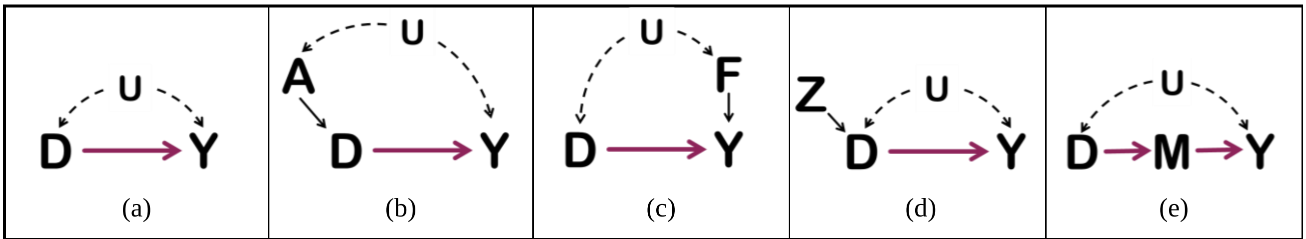

Figure 2. Identification Approaches in Potential Outcomes View

Table 2. Summary of Potential Outcomes Approaches

Potential outcomes approaches	**Empirical identification strategies**	**Useful references / additional reading**
Conditioning to balance and conditioning to adjust	Matching and regression-based approaches maintaining the assumption of strong ignorability	Rosenbaum & Rubin (1983); Iacus et al. (2012)
Instrumental variable	Using IVs to introduce an exogenous source of variation on treatment (i.e., relevance restriction) that is separate from the unobservable (i.e., exclusion restriction).	Angrist & Pischke (2009)
Mechanism-based	Unveiling the underlying causal mechanisms (e.g., mediation analysis, structural models, etc) that are isolated from unobservables.	Morgan & Winship (2015)
RDD	Using observations close to a threshold to approximate randomized treatment assignment	Lee and Lemieux (2010)
Methods for repeated observations	Fixed effects: Accounts for time-invariant unobservables; DID: Accounting for unobservable trends coexisting with the treatment effect	Morgan & Winship (2015)
Sensitivity analysis and set (or partial) identification	Sensitivity analysis for provisional causal effect estimates; Set identification	Manski & Pepper (2000); Rosenbaum (2002)

Conditioning: This approach employs observable factors to block the influence of unobservable factors from the causal effect by invoking a strong ignorability assumption or allowing selection based only on observables. The observable factors are either antecedents of the treatment (as per Figure 2b) or antecedents of the outcome (as per Figure 2c). Conditioning can be divided into *conditioning to balance* and *conditioning to adjust*. ***Conditioning to balance*** (Figure 2b) focuses on observable antecedents that determine the treatment assignment (i.e., A in Figure 2b), and then assesses the balance between the treated group and control group based on these observable antecedents. In empirical studies, conditioning to balance is typically achieved via matching techniques such as propensity score matching (PSM) (Rosenbaum & Rubin, 1983) or coarsened exact matching (CEM) (Iacus et al., 2012). Meanwhile, ***conditioning to adjust*** (Figure 2c) relies on observable antecedents of the outcome (i.e., F in Figure 2c). These antecedents are used to control for the variation of the outcome caused by confounding factors other than the causal effect. Conditioning to adjust is usually realized through the use of covariates in regression-based model specifications.

Instrumental variable (IV): This approach seeks an exogenous source of variation for the treatment using instrumental variables. As illustrated in Figure 2d, this method uses only the variation induced in treatment status via the exogenous variable *Z* to identify the causal effect (Angrist & Pischke, 2009). If the treatment indeed has a causal effect on the outcome, the exogenous source of variation due to *Z* is expected to perform like a "treatment assignment" in an experiment. The conventional IV estimator assumes constant treatment effects, and that assumption can be relaxed in an unrestricted heterogeneous potential outcomes framework enabling an estimate of the local average treatment effect (LATE) in most cases while making LATE instrument-specific and applicable to compliers only.

Isolated and exhaustive mechanisms: This approach seeks to observe an isolated and exhaustive mechanism linking the treatment to the outcome and then estimate the causal effect as it propagates through the mechanism, as illustrated in Figure 2e (Morgan and Winship 2015). An advantage of this approach is that it can provide an elaborated explanation of how the causal effect comes about, as a researcher could describe how the causal effect is propagated through the intermediate variable M. Note that this approach makes strong assumptions (e.g., M is only influenced by D, M is the only pathway via which D influences Y, no unobserved variable influences both D and M) (Imbens, 2020). To some extent, this approach can be reconciled with the path analytic view and the focus on understanding causal pathways to provide "sufficiently deep" mechanism-based explanations (Machamer et al., 2000; Morgan & Winship, 2015).

Regression discontinuity design (RDD): RDD aims to approximate random treatment assignment (Lee & Lemieux, 2010) and, depending on the specific setting (whether sharp or fuzzy), it either invokes the assumption of selection on observables, as in sharp RD, or relaxes it as in fuzzy RD when the forcing variable can be viewed as an instrumental variable. In using RDD, researchers need to be cautious about whether the forcing variable (based on which a cutoff level is introduced) is manipulated (McCrary, 2008), and how much weight is given to observations according to their distance from the cutoff (Imbens & Kalyanaraman, 2012). The causal effect estimated in RDD is essentially a LATE.

Approaches for panel data (e.g., fixed effects, DID): There are specialized approaches for panel data with repeated observations such as fixed effects, analysis of covariance (ANCOVA), and change score (also called difference-in-differences [DID]) (Morgan & Winship, 2015). These approaches are more effective with multiple waves of pretreatment data, and they have their own set of restrictive assumptions. These methods typically assume that the outcomes of treated groups would have evolved in the same way as that of control groups, and such assumptions are assessed by parallel trends in the pretreatment period or placebo tests (Athey & Imbens, 2017). Some techniques, e.g., the synthetic control (Abadie et al., 2010), can help to examine and satisfy parallel trends assumption. Recent work provides new insights on DID designs when multiple units are exposed to the treatment at different times (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021).

Sensitivity analysis and partial identification: These approaches do not provide point identification but can be useful to conduct sensitivity analyses maintaining a strong ignorability assumption (Rosenbaum, 2002), or by providing confidence interval estimates using relatively weaker assumptions (e.g., monotone treatment selection, and/or monotone treatment response) (Manski & Nagin, 1998; Morgan & Winship, 2015).

Considering their pros, the potential outcomes approach allows researchers to consider and address how observables and unobservables can potentially confound the causal effect of a treatment. When combined with domain knowledge and theoretical reasoning, the potential outcomes approaches can be useful for testing theories and conjectures. In comparison to traditional econometrics literature, which relies on statistical controls or functional form assumptions, the potential outcomes approaches draw attention to "design" aspects as well as "choices" such as using narrower hypotheses; using abrupt, short-lived treatments; using multiple treatment assignment mechanisms; nondose nonresponse analyses; using natural blocks (e.g., twins, siblings, schools); and minimizing the need for stability analyses (Rosenbaum, 1999, 2002).

The cons of the potential outcomes approaches are largely due to their assumptions and scope of inquiry. First, most of the potential outcomes applications thus far have focused on binary treatments (Imbens & Wooldridge, 2009), and the extension of this perspective to the cases with continuous treatments is an area of active research (Athey & Imbens, 2017). Potential outcomes approaches also have their own (sometimes untestable) assumptions, and the effectiveness and credibility of these approaches depend on these assumptions. For example, matching approaches assume that selection is based on observables, and therefore do not rule out selection on unobservables. In such cases, sensitivity analyses can help provide confidence in the effectiveness of matching (Mithas & Krishnan, 2009; Oestreicher-Singer & Zalmanson, 2013; Rosenbaum, 2002). Multiple potential outcomes approaches are sometimes used jointly to improve confidence in findings. Second, in our view, the potential outcomes approaches are, in comparison to path analytic or configurational view, not as useful for building new theories because they focus on relatively narrower questions and mostly on the testing of established theories (Keane, 2010; Rosenzweig & Wolpin, 2000).

Configurational View of Causality

The configurational view of causality relies on the notion of complex causality using the logic of conjunctural, equifinal, and asymmetric relations (El Sawy et al., 2010; Mahoney et al., 2013; Misangyi et al., 2017; Ragin, 2014). In this approach, attention shifts from particular independent variables to the "cases" or configurations of variables or interventions in assessing their impact (conjunctural causation), recognizing that multiple configurations may lead to the same outcome (equifinality). Also, conditions that are causally related in one configuration may be unrelated or inversely related in other configurations, and the presence of a condition leading to an outcome does not necessarily mean that its absence will not produce the outcome (asymmetric causation). This view contrasts sharply with other views of causality based on the linear net-effect of independent individual variables, symmetrical relations, and temporal "before and after" changes. The configurational view uses set-relations with Boolean algebra with truth tables serving as the raw material, in contrast to using correlation matrices as raw materials as in the potential outcomes and path analytic approaches.

The configurational view uses the logic of necessity and sufficiency conditions to identify **i**nsufficient but **n**ecessary (or **n**on-redundant) *parts* of **u**nnecessary but **s**ufficient configuration (INUS) causes that often comport better with real-life examples and cases (Mackie, 1965; Shadish et al., 2002). For example, even a lighted match is an INUS cause if one wishes to explain a forest fire, which also needs oxygen and dry leaves, and lightning can be viewed as a substitute for a lighted match (Shadish et al., 2002).

Figure 3 shows an illustrative example of how configurational thinking uses the logic of necessary and sufficient conditions. The table lists five possible configurations for high performance and two possible configurations for not-high performance that might be obtained in an analysis. Depending on the configuration(s) obtained, the status of the same causal attribute D (say customer relationship management (CRM) systems) changes dramatically in influencing high or not-high performance.

	Sufficient configurations for high performance (Y)					**Sufficient configurations for not-high performance (~Y)**	
	S1	S2	S3	S4	S5	F1	F2
D=CRM systems	D	~D	D		D	D	D
A=IT investments	A			A		A	~A
N=Manufacturing		N	N			N	

Notes: (A) The equifinal configurations for high performance above allow researchers to draw inferences such as:

1. Y=D — D is both necessary and sufficient to explain high performance (iff S5)
2. Y=AD + ND — D is necessary but not sufficient to explain high performance (iff S1,S3)
3. Y=AD + N~D — No cause necessary or sufficient to explain high performance (iff S1,S2)
4. Y=AD — A and D necessary but not sufficient alone to explain high performance (iff S1)
5. Y=A + ND — A not necessary but sufficient to explain high performance (iff S4, S3)

(B) The equifinal configurations for not-high performance above allow researchers to draw inferences such as:

6. ~Y=DAN+ D~A — D is necessary, but not sufficient alone to explain not-high performance (iff F1,F2).

Figure 3. Illustration of Configurational View

The configurational view, with a focus on the synergistic effect of the sets of variables, differs from a path analytic view with a focus on the additive linear net effects of individual variables, or from a potential outcome view that focuses attention on one binary intervention with other variables used mainly for identification. The notion of parsimonious configurations differs from and goes beyond the notion of "gestalts," in that configurations also illuminate the differing roles of underlying variables as core, peripheral, present, and absent (Fiss, 2011; Park & Mithas, 2020). Also, the configuration view differs from the complementarity view, which assumes that the complements are individually manipulable. This view is easier to comprehend when one is dealing with two or three complements but becomes limiting with sparse data and when the focus is on "configurations or conjunctions of multiple variables" that simultaneously influence performance (Misangyi & Acharya, 2014; Misangyi et al., 2017). Arguably, the configurational view is highly suitable for studying the organized and emergent complexity so prevalent in contemporary business and society (Benbya et al., 2020; Park & Mithas, 2020).

The configurational view, like the potential outcomes view, uses the language of counterfactuals. However, the counterfactuals here refer to cases of unobserved configurations in the truth table, or "remainders" representing some combinations of causal conditions that are not observed in the data (whereas the potential outcomes view uses the notion of counterfactuals for each case or individual in the observed data to define causality itself) (Mahoney et al., 2013). In contrast to the potential outcomes paradigm in which researchers do not use assumptions about the configurations of causal variables for which there are no cases in the analysis explicitly (although they may invoke assumptions such as linearity and additivity to address the problem of limited diversity implicitly in regression-based models), the configurational view directly uses the assumptions about counterfactual cases in its analysis to identify the so-called "core," or "peripheral" conditions based on "difficult" or "easy" counterfactuals (Fiss, 2011; Mahoney et al., 2013). Note that the configurational view does not distinguish between manipulable causes and attributes as is frequently done in potential outcomes approaches; here, all conditions are treated as attributes (no attribute has any special status).

In terms of its advantages, because the configurational view demands significant domain knowledge and familiarity with cases, it suits small-*n* situations where researchers can leverage their context knowledge for calibration and counterfactual analysis. Also, the resulting solutions preserve case knowledge and enable researchers to conduct a "dialogue with data" to create or refine theories and solutions that can be easier to communicate as causal recipes. On the flip side, because the configurational view conceptualizes causality in terms of conjunctural causation, it does not provide coefficients for conditions but quantitative measures to assess the consistency and coverage of configurations. Also, one has to have faith in researchers' domain knowledge and expertise for the selection of conditions and calibration. The method has limited generalizability, but the notion of generalizability or external validity of causal effects is also equally suspect in the path analytic and potential outcomes approaches (Angrist & Pischke, 2009; Shadish et al., 2002).

Summary of the Pros and Cons of Different Views of Causality

Thus far, we have discussed the three perspectives for viewing causality and their relative merits and demerits in isolation. Table 3 provides a high-level summary of the pros and cons of the alternative views of causality in our view.

Table 3. Pros and Cons of Alternative Views of Causality

View	Pros	Cons
Path analytic view	• Focuses on "why" questions more than "what" questions, clarifies mechanisms / boundary conditions • Focuses on causal explanation and "molecular" causation (vs. causal description / molar causation in counterfactual view)	• Approach can sometimes be unprincipled or ad hoc; inadequate scrutiny of causal assumptions inherent in models • Plausible alternative models often not considered, and strong reliance on global tests of model fit • Robust dependence view of causality does not appeal to some who insist on "no causation without manipulation" (Holland 1986)
Potential outcomes view	• Focuses on the causal effect of one intervention in a rigorous way • Considers pretreatment heterogeneity (baseline) and treatment effect heterogeneity more explicitly	• Focus on estimation of narrower questions, often binary treatments (Imbens, 2020; Imbens & Wooldridge, 2009; Morgan & Winship, 2015; Rosenzweig & Wolpin, 2000) • Typically used for theory testing (Keane, 2010), and few applications so far for theory building
Configurational view	• Case-centered approach, permits dialogue with data • Ability to model causal complexity, equifinality, causal asymmetry • Can leverage context knowledge for calibration, and for counterfactual analysis of cases before doing data analysis or during data analysis • Prescriptive recipes, easy to communicate	• Conclusions dependent on researchers' expertise and choices about calibration, and counterfactual cases • Quantitative and qualitative conclusions at the level of configurations, no separate estimates for causal conditions • Limited generalizability (or method itself does not make claims about generalizability), such claims are heroic or suspect in other approaches as well—see Shadish et al. (2002) and the idea of UTOS

Table 4 tentatively parses these views on several dimensions.[8] In particular, the three perspectives differ in terms of the range of research questions, their suitability for types of causal explanation or causal description, their relative contribution to building or testing theories, the types of interventions or causes, the types of data needed, and the communicability of findings.

Using Table 4, we briefly discuss a few key points. First, the path analytic perspective allows for a much wider variety of research questions than either the potential outcomes perspective or the configurational perspective because of its inherent flexibility to accommodate a variety of effects using causal diagrams and to convey them in a more reader-friendly language. The path analytic perspective also accommodates many different types of data used in survey-based research, laboratory experiments, and archival records studies. That may explain the relative market share of the path analytic view, but that comes at the cost of some lost rigor if researchers do not fully articulate the key assumptions of their models and test alternative models that may also explain covariances/correlations observed in their data. To the extent that such pitfalls also apply to the potential outcomes perspective or the configurational perspective when researchers fail to surface key assumptions or provide sufficient confidence in the validity of assumptions, they do not represent a serious flaw of the path analytic perspective. However, because path analytic methods have been used much more widely than the methods of the other two perspectives, their limitations are much better known than those of the other perspectives, which are in relative infancy—at least compared to the path analytic perspective.

Second, the potential outcomes perspective, in our view, is better suited for examining well-studied phenomena by choosing research questions to test a "broad theory on a narrow instance in which theory's operation may be viewed clearly," as Rosenbaum (2002, p. 342) puts it. Researchers can test or retest older documented findings and assess the boundaries of received theories with perhaps better-quality data or creative use of contextual novelty (e.g., natural experimental settings) that are informed by deep domain knowledge. Doing so requires that researchers using potential outcomes approaches or reviewers insisting on the use of such approaches be explicit about the underlying theories and assumptions. While we see great enthusiasm among some researchers for the potential outcomes perspective, one must realize that it mainly focuses on "causal description" (e.g., whether a particular effect is present or absent and, if present, its magnitude) rather than offering a detailed "causal explanation" for why that effect is observed (e.g., the underlying mechanisms or the conditions in which the effects are stronger or weaker), which is

[8] We used some discretion in Table 4. Other researchers may have a different viewpoint on the relative merits of these different views.

more readily discussed in the context of other perspectives (Shadish et al., 2002).[9] Even among economists, there are debates on types of insights that the potential outcomes approach can or cannot offer (Deaton & Cartwright, 2018; Imbens, 2010; Rosenzweig & Wolpin, 2000). Although the potential outcomes perspective is now being used to incorporate mechanism-based causal inference similar to the flavor of the path analytic view from a methodological perspective (Machamer et al., 2000; Morgan & Winship, 2015), there are few credible applications of such methods and such methods have their own strong assumptions (Imbens, 2020).

Table 4. Contrasting Alternative Views of Causality by Dimension

Dimension	Path analytic view	Potential outcomes view	Configurational view
Range of questions / domain	Wide	Narrow	Moderate
Well-suited for explanation or description	Causal explanation (molecular causation)	Causal description (molar causation)	Causal explanation (molecular causation)
Relative contribution to building or testing theories	Significant, clarifies mediation/moderating mechanisms	Relatively strong in testing theories	Test theories and build new theories
Types of interventions or causes that the view addresses or admits	Continuous, can handle attributes as well as manipulable causes	"Manipulable" causes, typically binary	Hard to study more than 6-8 conditions
Data requirements	Can accommodate small and large-*N* analyses	Needs large *N*, exogenous variation (typically via natural experiments), pretreatment longitudinal data for assessing the validity of assumptions	Well-suited for small *n*, increasingly used for large-*N* analyses
Communicability of findings to managers and policy makers	Moderate	High	High, prescriptive qualitative causal recipes

Third, the configurational view, being relatively recent in origin, is better suited for studying a new phenomenon or established phenomena where it may be easier to focus on a small number of causal conditions (e.g., up to six to eight). Researchers can leverage low to moderate samples about which they have deep domain knowledge or contextual knowledge (Mahoney et al., 2013), but that becomes hard for large sample studies. This view provides opportunities for researchers to leverage their contextual understanding to conduct complex and novel theorizing that may spawn new empirical work using path analytic or potential outcomes approaches. Configurational perspective can also provide qualitative causal recipes, and quantitative estimates (Ragin & Fiss, 2017). Shadish et al. (2002, p. 6) remind us that causal inference is "fundamentally qualitative" (Shadish et al., 2002, p. 6). Hence to the extent that one relies on qualitative knowledge of cases in the configurational view, causal inference using the configurational view is not a serious problem in and of itself.

Finally, although it is tempting to think that the use of multiple perspectives or methods may help us better understand the complexity of causal relationships, that thought must be tempered by the realization that different perspectives focus on different types of research questions, and the practices of one perspective may not always make sense from another perspective.[10] For example, the configurational view of causality that uses a necessity/sufficiency logic is almost orthogonal to the potential outcome's logic, and while the potential outcomes approaches thrive on focusing on the causal effect of one intervention, such an effect may be considered underwhelming, if not simplistic, in the configurational view. To the extent that various perspectives ask different types of causal questions with different sets of assumptions, it may be time for social science researchers to embrace causal uncertainty as a fact of scientific inquiry just as physicists reconciled themselves to the wave-matter uncertainty. In other words, the diversity of research questions, data, and the background and training of researchers calls for adopting a pluralistic stance toward causality that accommodates diversity.

[9] This point that causal estimates from IV or natural experiments by themselves do not "explain why" is well accepted in economics (Heckman & Pinto, 2022; Keane, 2010). Angrist and Pischke (2009, p. 156) note: "There is nothing in IV formulas to explain *why* Vietnam-era service affects earnings; for that, you need a theory" (emphasis in original). Likewise, Rosenzweig and Wolpin argue that studies using natural experiments do not provide "dramatically different" or "conclusive" estimates "that can be unambiguously interpreted" (p. 872) and conclude that "measurement without theory … is not significantly more valuable than it ever was before the use of natural experiments" (p. 873).

[10] Similar sentiments have been stated about the nature and state of different theoretical paradigms in organizational science (Davis, 2010).

Reflecting on the Role of Causality in Developing Cumulative Knowledge in Information Systems

Much of this editorial has focused on discussing three perspectives on causality and how they apply to IS research, with a particular focus on contributions appropriate for *MISQ*. We next discuss a few overarching ideas and reflections that may be helpful for fellow IS researchers and stakeholders. Some of these comments are also offered to address questions and comments that arose when we presented these ideas in our sessions with editors, authors, and reviewers during 2021-2022.

First, given the social nature of science, no single study can "prove" causality, nor can any single perspective or approach address it completely (Rein & Winship, 1999). We see no reason to privilege any one perspective or approach over the other, even if one is interested solely in causation, because much will depend on a variety of considerations such as the nature of the question, the type of data one has, the type of theory building/testing one has in mind, and the assumptions one is willing to make or defend. Some approaches may indeed be better suited in a particular context than others, but even there, scholars may have reasonable disagreements. That is not to say that every conclusion is equally legitimate. Rather, our point is that each method of causal inquiry comes with its package of what scholars in that tradition will likely consider reasonable based on the state-of-the-art at the time. There are contentious debates and arguments about the merits or otherwise of almost any paradigm for thinking about causality or any method within it. These debates and discussions have been witnessed in all the perspectives discussed here: path analytic (Aguirre-Urreta & Marakas, 2014; Bollen & Pearl, 2013); potential outcomes (Deaton, 2010; Deaton & Cartwright, 2018; Heckman & Pinto, 2022; Heckman & Urzua, 2010; Imbens, 2010; Rosenzweig & Wolpin, 2000); and configuration (Achen, 2005; Seawright, 2005). There is no magical panacea that delivers a nirvana of causality. Fixed effects (Hill et al., 2020; Plumper & Troeger, 2019), and other identification methods (e.g., IV, RD, DID, staggered DID, synthetic controls) have their own sets of untestable assumptions and must be viewed with an appropriate dose of skepticism and the merely provisional faith that should characterize any scientific activity.

Second, the pursuit of causality must strike a balance between the importance and relevance of questions/domains for IS. The relative importance of causality in a particular study depends on the study's intended contribution. If a study articulates its core contribution as establishing causality—and we welcome such studies—then it should receive greater scrutiny of causal approaches and assumptions. However, we place no priority or preference for such studies over any other studies at MISQ. There is always the concern that fascination with a particular methodological fad may put the cart before the horse and may lead to the selection of questions that are amenable to a particular method. Despite the resurgence of interest in approaches to infer causality in the last decade, top IS journals are ultimately geared toward building and supporting the science of information systems where causality considerations play a role depending on the aims and genre of the research.

Third, causality considerations in the evaluation of research are often intertwined with the consideration of relevant theories and mechanisms. For example, theory helps to specify the variables that determine selection into treatment (s) and/or influence outcomes, and which unobserved variables (if any) may influence the causal relationships of interest. Theories and mechanisms also help to explain how causes influence outcomes, even if the invoked theoretical mechanisms themselves are sometimes not observable or testable. We do not subscribe to the view that big data or AI imply the "end of theory," or the end of the "scientific method" (Anderson, 2008). We see more value in Goldthorpe's advice for researchers to take a holistic and programmatic stance and adopt "a generative process view of causality" that values descriptive research, respects the search for theories and mechanisms at a more microscopic level than that at which the association is established, and seeks to provide a "corroboration pending account" rather than drawing overly strong conclusions (Goldthorpe, 2001, p. 15). Rozin (2001) makes similar observations in the context of social psychology. Given the complexity of causal inference, it is hard to deny the importance of programmatic work that may require researchers to work in a domain for years or even decades to build cumulative knowledge (Burton-Jones, 2009; Jarvenpaa et al., 1985). Being aware of and inclusive of different perspectives on causality in such programmatic work will help the field build strong and rich theories and insights for research and practice (Miranda et al., 2022).

Fourth, the comments in this editorial have implications for editorial roles and review processes. No editorial can solve all the problems we noted earlier, such as reviewers asking for "yet another robustness check" without adequate justification or judging papers against an inappropriate causal standard. Senior editors have a particularly strong responsibility to address these issues by adjudicating among competing claims of authors and other participants in the review process by engaging in appropriate "sensemaking of reviews" and using their "own assessment, and revisability of paper within reasonable time and effort" (Goes, 2014; MISQ, 2022). Editors can also shape expectations of what is reasonable and the relative role of causality considerations in a paper and a research program. They must be willing and able to adopt a broad view of causality even if their own training or preference is for a particular view of it. With their overall view of the field, they can help to provide a more level playing field for authors by engaging an appropriate reviewer pool that can judge a manuscript based on the view of causality and standards appropriate to it or by defining the review roles (Pondy, 1985). Editors also have a key role in developing a shared understanding of the cost/benefit and return of investment of extra robustness checks for causality, and other broader trade-offs, such as those among generality, parsimony, and accuracy (Thorngate, 1989). The aim of a review process is not to satisfy every reviewer's whim or curiosity in the name of "robustness" or "state of the art," nor is it to favor any specific type of data (e.g., "big data," individual-level data, or quasi-experimental data). For example, it makes little sense to force authors of a paper using the configurational view to show "robustness" using potential outcomes approaches, or to criticize such studies for the statistical "representativeness" of their data. Likewise, authors using traditional econometric approaches that use continuous treatments and focus on complex relationships (e.g., mediation, moderation, and hierarchical models) should not be forced by reviewers to reengineer their paper into a potential outcomes logic (focusing on a particular causal effect that a reviewer has in mind) if it involves abandoning or radically changing the original inquiry. To prevent such cases, we see no substitute for engaged editorial processes such as those we strive to enact at MISQ.

Editors can also leverage MISQ's initiative on research transparency (Burton-Jones et al., 2021) to direct a review process that is rigorous and author-friendly at the same time. We see plenty of room to use the transparency initiative for "descriptive," "theoretical," and other types of contributions (e.g., new algorithms, artifacts), so that arguments can be presented with greater rigor and elucidation and meet commonly accepted criteria that these other genres of research have "an equal obligation to attend" (Guba & Lincoln, 1982, p. 246). Editors are also in the best position to assess when a manuscript is "good enough" (Spiegel, 2012). As Shadish et al. (2002, p. 16) note, "it is neither feasible nor desirable to rule out all *possible* alternative interpretations of a causal relationship. Instead, only *plausible* alternatives constitute the major focus." For all these reasons, if readers (in their roles as authors and/or editors or reviewers) focus on "plausible" and well-reasoned alternatives, that will help make review processes more constructive and useful for advancing science.

Finally, one critical challenge for IS researchers and the broader scientific community is to reflect on the educational side of causal thinking. How should we educate students and the public to improve scientific literacy in general, and to address psychological motivations to reject even sound science (Hornsey, 2020; Salmon, 1998)? What causes them to reject causal evidence? Unfortunately, this issue also brings to the fore the dark side of IT, which can help to amplify bad science or confusion in public and create distrust in science. Perhaps IS scholars can use three perspectives on causality to shed light on these issues.

To conclude, causality is one of the most central and complex notions in science. It is also a very practical concept because it is causal knowledge that gives us some element of control over the world around us. Given how extensively information systems are now infused into business and social life (and the pervasive consequences that can ensue from them), the search for causal knowledge related to information systems is important and will surely only grow in importance. It will be increasingly vital, therefore, for our authors and reviewers to appreciate different approaches to causality and be able to apply and judge them. The rapidly developing advances in the methodological literature on causality provide significant opportunities for IS researchers to pose and answer research questions in new and insightful ways. Intellectual arbitrage across adjacent disciplines is part and parcel of science. However, we are also clear-eyed in that not all research questions or research programs need to be driven by a focus on causal effects alone. We must remain open to other means of inquiry and must retain the ability to pose more complex questions that use a different way of thinking about causality or to even pursue research that is initially descriptive and only later leads to theorizing and alternative ways of addressing causation (Barua & Mukhopadhyay, 2000; Brynjolfsson et al., 2021; Levina, 2021). Overall, the science of information systems should be energized by the search for causality but not driven by it.

Acknowledgments

We thank the *MISQ* editorial board for their comments in our masterclass with them and the ~400 participants in our two knowledge-sharing sessions for authors and reviewers. We thank Miguel Aguirre-Urreta, Hilal Atasoy, Denny Borsboom, Yanzhen Chen, Ron Cenfetelli, John D'Arcy, Peer Fiss, Gordon Gao, William Greene, Varun Grover, Hilah Levin Geva, Hank Lucas, Miguel Godinho de Matos, Stephen L. Morgan, YoungKi Park, Jui Ramaprasad, Arun Rai, Sean Xin Xu, Xiaoquan (Michael) Zhang, and Daniel Zantedeschi for thoughtful comments, and we alone remain responsible for any errors. Finally, we thank Yifan Ren, Alysson Silveira, Yingxin Zhou, and Wenping Zou for their research assistance.

References

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of American Statistical Association*, *105*(490), 493-505.
Achen, C. (2005). Two cheers for Charles Ragin. *Studies in Comparative International Development*, *40*(1), 27-32.
Aguirre-Urreta, M. I., & Marakas, G. M. (2014). Partial least squares and models with formatively specified endogenous constructs: A cautionary note. *Information Systems Research*, *25*(4), 761-778.
Anderson, C. (2008). *The end of theory: The data deluge makes the scientific method obsolete*. Wired. https://www.wired.com/2008/06/pb-theory/.
Angrist, J. D., & Krueger, A. B. (1999). Empirical strategies in labor economics. In O. Ashenfelter & D. Card (Eds.), *Handbook of*

labor economics (Vol. 3A, pp. 1277-1366). Elsevier.
Angrist, J. D., & Pischke, J.-S. (2009). *Mostly harmless econometrics: An empiricist's companion*. Princeton University Press.
Athey, S., & Imbens, G. W. (2017). The state of applied econometrics: Causality and policy evaluation. *Journal of Economic Perspectives*, *31*(2), 3-32.
Avgerou, C. (2013). Social mechanisms for causal explanation in social theory based IS research. *Journal of the Association for Information Systems*, *14*(8), 399-419.
Banker, R., Kauffman, R., & Morey, R. (1990). Measuring gains in operational efficiency from information technology: A case study of the Positran deployment at Hardee's Inc. *Journal of Management Information Systems 7*(2), 29-54.
Barringer, S. N., Eliason, S. R., & Leahey, E. (2013). A history of causal analysis in the social sciences. In S. L. Morgan (Ed.), *Handbook of causal analysis for social research* (pp. 9-26). Springer.
Barua, A., & Mukhopadhyay, T. (2000). Information technology and business performance: Past, present, and future. In R. W. Zmud (Ed.), *Framing the domains of information technology management: Projecting the future ... through the past* (pp. 65-84). Pinnaflex Press.
Benbya, H., Nan, N., Tanriverdi, H., & Yoo, Y. (2020). Complexity and information systems research in the emerging digital world. *MIS Quarterly*, *44*(1), 1-17.
Bollen, K. A. (1989). *Structural equations with latent variables.* Wiley.
Bollen, K. A., & Pearl, J. (2013). Eight myths about causality and structural equation models. In S. L. Morgan (Ed.), *Handbook of causal analysis for social research* (pp. 301-328). Springer.
Borsboom, D., Mellenbergh, G. J., & Van Heerden, J. (2004). The concept of validity. *Psychological Review*, *111*(4), 1061-1071.
Brynjolfsson, E., Wang, C., & Zhang, X. (2021). The economics of IT and digitization: Eight questions for research. *MIS Quarterly*, *45*(1), 473-477.
Bunge, M. (2009). *Causality and modern science* (4th ed). Routledge.
Burton-Jones, A. (2009). Minimizing method bias through programmatic research. *MIS Quarterly*, *33*(3), 445-471.
Burton-Jones, A., Boh, W. F., Oborn, E., & Padmanabhan, B. (2021). Editor's comments—Advancing research transparency at MIS Quarterly: A pluralistic approach. *MIS Quarterly*, *45*(4), iii-xviii.
Callaway, B., & Sant'Anna, P. H. C. (2021). Difference-in-differences with multiple time periods and an application on the minimum wage and employment. *Journal of Econometrics*, *225*(2), 200-230.
Chen, P. Y., Y., H., & Y., L. (2018). Value of multi-dimensional rating systems: Evidence from a natural experiment and lab experiments. *Management Science*, *64*(10), 4629-4747.
Cornfield, J., Haenszel, W., Hammond, E. C., Lilienfeld, A. M., Shimkin, M. B., & Wynder, E. L. (1959). Smoking and lung cancer: Recent evidence and a discussion of some questions. *Journal of the National Cancer Institute*, *22*, 173-203.
Cronbach, L. J. (1957). The two disciplines of scientific psychology. *American Psychologist*, *12*, 671-684.
Davis, G. F. (2010). Do theories of organizations progress? *Organizational Research Methods*, *13*(4), 690-709.
Deaton, A. (2010). Instruments, randomization, and learning about development. *Journal of Economic Literature*, *48*(2), 424-455.
Deaton, A., & Cartwright, N. (2018). Understanding and misunderstanding randomized controlled trials. *Social Science & Medicine*, *210*, 2-21.
Duncan, O. D. (1966). Path analysis: Sociological examples. *American Journal of Sociology*, *72*(1), 1-16.
El Sawy, O. A., Malhotra, A., Park, Y., & Pavlou, P. A. (2010). Seeking the configurations of digital ecodynamics: It takes three to tango. *Information Systems Research*, *21*(4), 835-848.
Fisher, R. A. (1935). *The design of experiments*. Oliver & Boyd.
Fiss, P. C. (2011). Building better causal theories: A fuzzy set approach to typologies in organization research. *Academy of Management Journal*, *54*(2), 393-420.
Goes, P. (2014). The MISQ review system: Operational perspectives. *MIS Quarterly*, *38*(4), iii-vii.
Goldberger, A. S. (1972). Structural equation methods in the social sciences. *Econometrica*, *40*(6), 979-1001.
Goldthorpe, J. H. (2001). Causation, statistics and sociology. *European Sociological Review*, *17*(1), 1-20.
Goodhue, D. L., Lewis, W., & Thompson, R. (2012). Does PLS have advantages for small sample size or non-normal data? *MIS Quarterly*, *36*(3), 981-1001.
Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254-277.
Gow, I. D., Larcker, D. F., & Reiss, P. C. (2016). Causal inference in accounting research. *Journal of Accounting Research*, *54*(2), 477-523.
Greene, W. H. (2000). *Econometric analysis* (4th ed.). Prentice Hall.
Gregor, S. (2006). The nature of theory in information systems. *MIS Quarterly*, *30*(3), 611-642.
Grover, V., Kuai, L., Wei, J., & Zhu, Y. (2020). The complexity of establishing causality in a digital environment: An eye to the future. *Journal of Information Technology Case and Application Research*, *22*(1), 3-7.
Guba, E. G., & Lincoln, Y. S. (1982). Epistemological and methodological bases of naturalistic inquiry. *Educational Communication and Technology Journal*, *30*, 233-252.
Gupta, A., Kannan, K., & Sanyal, P. (2018). Economic experiments in information systems. *MIS Quarterly*, *42*(2), 595-606.
Hannon, P., & Harrison, D. (2021). Nobel economics prize is awarded to U.S.-based trio. *Wall Street Journal*. https://www.wsj.com/articles/nobel-economics-prize-awarded-to-david-card-joshua-d-angrist-and-guido-w-imbens-11633947212
Heckman, J. J., Ichimura, H., & Todd, P. (1997). Matching as an econometric evaluation estimator. *Review of Economic Studies*, *65*(2), 261-294.
Heckman, J. J., & Pinto, R. (2022). *Causality and econometrics* [Working paper: NBER paper 29787]. Available at https://www.nber.org/papers/w29787.
Heckman, J. J., & Urzua, S. (2010). Comparing IV with structural models: What simple IV can and cannot identify. *Journal of*

Econometrics, *156*, 27-37.
Hill, T. D., Davis, A. P., Roos, J. M., & French, M. T. (2020). Limitations of fixed-effects models for panel data. *Sociological Perspectives*, *63*(3), 357-369.
Ho, T.-H., Lim, N., Reza, S., & Xia, X. (2017). Causal inference models in operations management. *Manufacturing & Service Operations Management*, *19*(4), 509-525.
Holland, P. (1986). Statistics and causal inference (with discussion). *Journal of American Statistical Association*, *81*(396), 945-970.
Hornsey, M. (2020). Why facts are not enough: Understanding and managing the motivated rejection of science. *Association for Psychological Science*, *29*(6), 583-591.
Huang, N., Mojumder, P., Sun, T., Lv, J., & Golden, J. M. (2021). Not registered? Please sign up first: A randomized field experiment on the ex ante registration request. *Information Systems Research*, *32*(3), 914-931.
Hume, D. (1740/1977). *A treatise of human nature*. J. M. Dent & Sons Ltd.
Iacus, S. M., King, G., & Porro, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political Analysis*, *20*(1), 1-24.
Imai, K., Keele, L., & Tingley, D. (2010). A general approach to causal mediation analysis. *Psychological Methods*, *15*, 309-334.
Imai, K., Keele, L., & Yamamoto, T. (2010). Identification, inference and sensitivity analysis for causal mediation effects. *Statistical Science*, *25*, 51-71.
Imbens, G. W. (2010). Better LATE than nothing: Some comments on Deaton (2009) and Heckman and Urzua (2009). *Journal of Economic Literature*, *48*(2), 399-423.
Imbens, G. W. (2020). Potential outcome and directed acyclic graph approaches to causality: Relevance for empirical practice in economics. *Journal of Economic Literature*, *58*(4), 1129-1179.
Imbens, G. W., & Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies*, *79*(2), 933-959.
Imbens, G. W., & Rubin, D. B. (2015). *Causal inference for statistics, social, and biomedical sciences: An introduction*. Cambridge.
Imbens, G. W., & Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, *47*(1), 5-86.
Jarvenpaa, S., Dickson, G. W., & DeSanctis, G. (1985). Methodological issues in experimental IS research: Experiences and recommendations. *MIS Quarterly*, *9*(2), 141-156.
Jöreskog, K. G. (1978). Structural analysis of covariance and correlation matrices. *Psychometrika*, *43*(4), 443-477.
Karahanna, E., Benbasat, I., Bapna, R., & Rai, A. (2018). Editor's comments: Opportunities and challenges for different types of online experiments. *MIS Quarterly*, *42*(4), iii-x.
Keane, M. P. (2010). Structural vs. atheoretic approaches to econometrics. *Journal of Econometrics*, *156*, 3-20.
Lee, B., Barua, A., & Whinston, A. B. (1997). Discovery and representation of causal relationships in MIS research: A methodological framework. *MIS Quarterly*, *21*(1), 109-136.
Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, *48*(2), 281-355.
Levallet, N., Denford, J. S., & Chan, Y. E. (2021). Following the MAP (methods, approaches, perspectives) in information systems research. *Information Systems Research*, *32*(1), 130-146.
Levina, N. (2021). All information systems theory is grounded theory. *MIS Quarterly*, *45*(1), 489-494.
Little, R. J., & Rubin, D. (2000). Causal effects in clinical and epidemiological studies via potential outcomes: concepts and analytical approaches. *Annual Review of Public Health*, *21*, 121-145.
Losee, J. (2011). *Theories of causality: From antiquity to the present*. Routledge.
Lucas, H. C. (1975). Performance and the use of an information system. *Management Science*, *21*(8), 908-919.
Lucas, H. C. (1978). Empirical evidence for a descriptive model of implementation. *MIS Quarterly*, *2*(2), 27-42.
Lucas, H. C. (1980). The impact of the mode of information presentation on learning and performance. *Management Science*, *26*(10), 982-993.
MacCallum, R. C., & Austin, J. T. (2000). Applications of structural equation modeling in psychological research. *Annual Review of Psychology*, *51*, 201-226.
Machamer, P., Darden, L., & Craver, C. F. (2000). Thinking about mechanisms. *Philosophy of Science*, *67*(1), 1-25.
Mackie, J. L. (1965). Causes and conditions. *American Philosophical Quarterly*, *2*(4), 245-264.
Mahoney, J., Goertz, G., & Ragin, C. C. (2013). Causal models and counterfactuals. In S. L. Morgan (Ed.), *Handbook of causal analysis for social research* (pp. 75-90). Springer.
Manski, C. F., & Nagin, D. S. (1998). Bounding disagreements about treatment effects: A case study of sentencing and recidivism. *Sociological Methodology*, *28*, 99-137.
Manski, C. F., & Pepper, J. V. (2000). Monotone instrumental variables: With an application to the returns to schooling. *Econometrica*, *68*(4), 997-1010.
Markus, M. L., & Rowe, F. (2018). Is IT changing the world? Conceptions of causality for information systems theorizing. *MIS Quarterly*, *42*(4), 1255-1280.
McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, *142*(2), 698-714.
Mill, J. S. (1843/1882). *A system of logic, ratiocinative and inductive: Being a connected view of the principles of evidence and the methods of scientific investigation*. Harper & Brothers.
Miranda, S., Berente, N., Seidel, S., Safadi, H., & Burton-Jones, A. (2022). Editor's comments: Computationally intensive theory construction: A primer for authors and reviewers. *MIS Quarterly*, *46*(2), i-xvi.
Misangyi, V. F., & Acharya, A. G. (2014). Substitutes or complements? A configurational examination of corporate governance mechanisms. *Academy of Management Journal*, *57*(6), 1681-1705.
Misangyi, V. F., Greckhamer, T., Furnari, S., Fiss, P. C., Crilly, D., & Aguilera, R. (2017). Embracing causal complexity: The

emergence of a neo-configurational perspective. *Journal of Management*, *43*(1), 255-282.
MISQ. (2022). *The SE role at MISQ*. https://misq.umn.edu/skin/frontend/default/misq/pdf/SERole.pdf.
Mithas, S., Almirall, D., & Krishnan, M. S. (2006). Do CRM systems cause one-to-one marketing effectiveness? *Statistical Science*, *21*(2), 223-233.
Mithas, S., Almirall, D., & Krishnan, M. S. (2014). A potential outcomes approach to assess causality in information systems research. In R. J. Kauffman & P. P. Tallon (Eds.), *Economics, information systems and electronic commerce research: Advanced empirical methodologies* (Vol. 2, pp. 63-85). M. E. Sharpe.
Mithas, S., Chen, Y., Lim, Y., & Silveira, A. D. O. (2022). On causality and plausibility of treatment effects in operations management research. *Production and Operations Management, Forthcoming*.
Mithas, S., & Krishnan, M. S. (2009). From association to causation via a potential outcomes approach. *Information Systems Research*, *20*(2), 295-313.
Moore, J. H. (1979). A framework for MIS software development projects. *MIS Quarterly*, *3*(1), 29-38.
Morgan, S. L., & Winship, C. (2015). *Counterfactuals and causal inference: Methods and principles for social research*. Cambridge University Press.
Oestreicher-Singer, G., & Zalmanson, L. (2013). Content or community? A digital business strategy for content providers in the social age. *MIS Quarterly*, 37(2), 591-616.
Padmanabhan, B., Fang, X., Sahoo, N., & Burton-Jones, A. (2022). Editor's comments: Machine learning in information systems research. *MIS Quarterly*, *46*(1), i-xvii.
Park, Y., Fiss, P. C., & El Sawy, O. A. (2020). Theorizing the multiplicity of digital phenomena: The ecology of configurations, causal recipes, and guidelines for applying QCA. *MIS Quarterly*, *44*(4), 1493-1520.
Park, Y., & Mithas, S. (2020). Organized complexity of digital business strategy: A configurational perspective. *MIS Quarterly*, *44*(1), 85-127.
Pearl, J. (1998). Graphs, causality, and structural equation models. *Sociological Methods & Research*, *27*(2), 226-284.
Pearl, J. (2000). *Causality: Models, reasoning, and inference*. Cambridge University Press.
Pearl, J., & Mackenzie, D. (2018). *The book of why: The new science of cause and effect*. Basic Books.
Peng, J. (in press). Identification of causal mechanisms from randomized experiments: A framework for endogenous mediation analysis. *Information Systems Research*. https://doi.org/10.1287/isre.2022.1113
Plumper, T., & Troeger, V. E. (2019). Not so harmless after all: The fixed-effects model. *Political Analysis*, *27*(1), 21-45.
Pondy, L. R. (1985). The reviewer as defense attorney (Ch. 13). In L. L. Cummings & P. J. Frost (Eds.), *Publishing in the organizational sciences* (pp. 210-219). Richard D. Irwin.
Ragin, C. C. (1987). *The comparative method: Moving beyond qualitative and quantitative strategies*. University of California Press.
Ragin, C. C. (2000). *Fuzzy-set social science*. University of Chicago Press.
Ragin, C. C. (2014). *The comparative method: Moving beyond qualitative and quantitative strategies*. University of California Press.
Ragin, C. C., & Fiss, P. C. (2017). *Intersectional inequality: Race, class, test scores & poverty*. University of Chicago Press.
Raudenbush, S. W., & Bryk, A. S. (2002). *Hierarchical linear models: Applications and data analysis methods* (2nd. ed.). SAGE.
Rein, M., & Winship, C. (1999). The dangers of "strong" causal reasoning in social policy. *Society*, 37, 38-46.
Rigdon, E. (2022). Needed developments in the understanding of quasi factor methods. *Communications of the Association for Information Systems Forthcoming*.
Roberts, M. R., & Whited, T. M. (2013). Endogeneity in empirical corporate finance. In G. M. Constantinides, M. Harris, & R. M. Stulz (Eds.), *Handbook of the economics of finance* (Vol. 2A, pp. 493-572). Elsevier.
Rönkkö, M., McIntosh, C. N., Antonakis, J., & Edwards, J. R. (2016). Partial least squares path modeling: Time for some serious second thoughts. *Journal of Operations Management*, *47*, 9-27.
Rosenbaum, P. (1999). Choice as an alternative to control in observational studies. *Statistical Science*, *14*(3), 259-304.
Rosenbaum, P. (2002). *Observational studies* (Second ed.). Springer.
Rosenzweig, M. R., & Wolpin, K., I. (2000). Natural "natural experiments" in economics. *Journal of Economic Literature*, *38*, 827-874.
Rozin, P. (2001). Social psychology and science: Some lessons from Solomon Asch. *Personality and Social Psychology Review*, *5*(1), 2-14.
Rubin, D. B. (1974). Estimating causal effects of treatments in randomized and nonrandomized studies. *Journal of Educational Psychology*, *66*(5), 688-701.
Rubin, D. B. (1978). Bayesian inference for causal effects: The role of randomization. *Annals of Statistics*, *6*(1), 34-58.
Salmon, W. C. (1998). *Causality and explanation*. Oxford University Press.
Seawright, J. (2005). Qualitative comparative analysis vis-a-vis regression. *Studies in Comparative International Development*, *40*(1), 3-26.
Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and Quasi-Experimental Designs for Generalized Causal Inference* Wadsworth Cengage Learning.
Singleton, R. A., & Straits, B. C. (2018). *Approaches to social research* (6th ed.). Oxford University Press.
Sobel, M. E. (1996). An introduction to causal inference. *Sociological Methods and Research*, *24*(3), 353-379.
Spiegel, M. (2012). Reviewing less—Progressing more. *The Review of Financial Studies*, *25*(5), 1331-1338.
Splawa-Neyman, J., (1923/1990). On the application of probability theory to agricultural experiments: Essay on principles: Section 9 [trans. and ed. D. M. Dabrowska & T. P. Speed]. *Statistical Science*, *5*(4), 465-472.
Tafti, A. R., & Shmueli, G. (2020). Beyond overall treatment effects: Leveraging covariates in randomized experiments guided by causal structure *Information Systems Research*, *31*(4), 1183-1199.
Thorngate, W. (1989). Possible limits on a science of social behavior. In J. H. Strickland, F. E. Aboud, & K. J. Gergen (Eds.), *Social psychology in transition* (pp. 121-139). Plenum Press.

Vandenberg, R. J. (2006). Statistical and methodological myths and urban legends: Where, pray tell, did they get this idea? *Organizational Research Methods*, *9*, 194-201.
Wooldridge, J. M. (2002). *Econometric analysis of cross section and panel data*. The MIT Press.
Wright, S. (1921). Correlation and causation. *Journal of Agricultural Research*, *20*, 557-585.
Xie, Y. (2012). Estimating heterogeneous treatment effects with observational data. *Sociological Methodology*, *42*, 314-347.
Zhao, X., Lynch Jr., J. G., & Chen, Q. (2010). Reconsidering Baron and Kenny: Myths and truths about mediation analysis. *Journal of Consumer Research*, *37*(2), 197-206.