

# Let's talk about Thurstone & Co.: An information-theoretical model for comparative judgments, and its statistical translation

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## Abstract

This study revisits Thurstone's law of comparative judgments (CJ) by addressing two key limitations in traditional approaches. Firstly, it addresses the overreliance on the assumptions of Thurstone's Case V in the statistical analysis of CJ data. Secondly, it addresses the apparent disconnect between CJ's approach to trait measurement and hypothesis testing. We put forward a systematic approach based on causal analysis and Bayesian statistical methods, which results in a model that facilitates a more comprehensive understanding of the factors influencing CJ experiments while offering a robust statistical translation. The new model accommodates unequal dispersions and correlations between stimuli, enhancing the reliability and validity of CJ's trait estimation, thereby ensuring the accurate measurement and interpretation of comparative data. The paper highlights the relevance of this updated framework for modern empirical research, particularly in education and social sciences. This contribution advances current research methodologies, providing a robust foundation for future applications in diverse fields.

*Keywords:* causal inference, directed acyclic graphs, structural causal models, bayesian statistical methods, thurstonian model, comparative judgement, probability, statistical modeling

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## 1. Introduction

In *comparative judgment* (CJ) studies, judges assess a specific trait or attribute across different stimuli by performing pairwise comparisons (Thurstone, 1927b,a). Each comparison produces a dichotomous outcome, indicating which stimulus is perceived to have a higher trait level. For example, when assessing writing quality, judges compare pairs of written texts (the stimuli) to

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Preprint submitted to *Psychometrika*

January 17, 2025

determine the relative writing quality each text exhibit (the trait) (Laming, 2004; Pollitt, 2012b; Whitehouse, 2012; van Daal et al., 2016; Lesterhuis, 2018a; Coertjens et al., 2017; Goossens and De Maeyer, 2018; Bouwer et al., 2023).

Numerous studies have documented the effectiveness of CJ in assessing traits and competencies over the past decade. These studies have highlighted three aspects of the method’s effectiveness: its reliability, validity, and practical applicability. Research on reliability suggests that CJ requires a relatively modest number of pairwise comparisons (Verhavert et al., 2019; Cromptvoets et al., 2022) to generate trait scores that are as precise and consistent as those generated by other assessment methods (Coertjens et al., 2017; Goossens and De Maeyer, 2018; Bouwer et al., 2023). In addition, the evidence suggests that the reliability and time efficiency of CJ are comparable, if not superior, to those of other assessment methods when employing adaptive comparison algorithms (Pollitt, 2012b; Verhavert et al., 2022; Mikhailiuk et al., 2021). Meanwhile, research on the validity of CJ scores indicates their capacity to represent the traits under measurement accurately (Whitehouse, 2012; van Daal et al., 2016; Lesterhuis, 2018a; Bartholomew et al., 2018; Bouwer et al., 2023). Moreover, research on CJ’s practical applicability highlights its versatility across both educational and non-educational contexts (Kimbell, 2012; Jones and Inglis, 2015; Bartholomew et al., 2018; Jones et al., 2019; Marshall et al., 2020; Bartholomew and Williams, 2020; Boonen et al., 2020).

Nevertheless, despite the increasing number of CJ studies, the prevalence of unsystematic and fragmented research approaches has left several critical issues unaddressed. The present study primarily focuses on two issues: the overreliance on Thurstone’s Case V assumptions in the statistical analysis of CJ data and the apparent disconnect between CJ’s approach to trait measurement and hypothesis testing. The following sections begin with a brief overview of Thurstone’s theory followed by a detailed examination of these issues. Subsequently, the study introduces a theoretical model for CJ that builds upon Thurstone’s theory, alongside its statistical translation, designed to address the two concerns simultaneously.

## **2. Thurstone’s theory**

In its most general form, Thurstone’s theory addresses pairwise comparisons wherein a single judge evaluates multiple stimuli (Thurstone, 1927a, pp. 267). The theory posits that two key factors determine the dichotomous outcome of these comparisons: the discriminative process of each stimulus and their discriminative difference. The *discriminative process* captures the psychological impact each stimulus exerts on the judge or, more simply, his perception of the stimulus trait. The theory

assumes that the discriminational process for any given stimulus forms a Normal distribution along the trait continuum (Thurstone, 1927a, pp. 266). The mode (mean) of this distribution, known as the *modal discriminational process*, indicates the stimulus position on this continuum, while its dispersion, referred to as the *discriminational dispersion*, reflects variability in the perceived trait of the stimulus.

Figure 1a illustrates hypothetical discriminational processes along a quality trait continuum for two written texts. The figure indicates that the modal discriminational process for Text B is positioned further along the continuum than that of Text A ( $T_B > T_A$ ), suggesting that Text B exhibits higher quality. Additionally, the figure highlights that Text B has a broader distribution compared to Text A, which arises from its larger discriminational dispersion ( $\sigma_B > \sigma_A$ ).

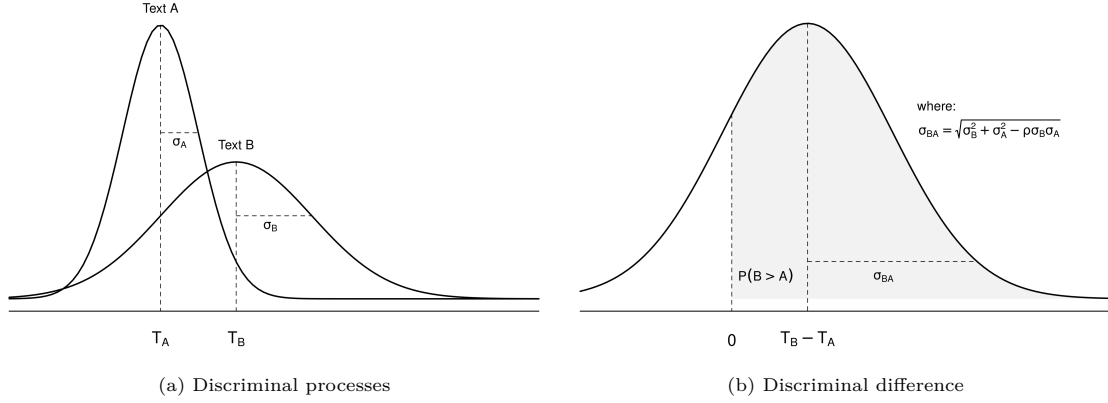


Figure 1: Hypothetical discriminational processes and discriminant difference along a quality trait continuum for two written texts.

However, given that the individual discriminational processes of the stimuli are not directly observable, the theory introduces the *law of comparative judgment*. This law posits that in pairwise comparisons, a judge perceives the stimulus with a discriminational process positioned further along the trait continuum as possessing more of the trait (Bramley, 2008, pp. 251). This suggests that the relative distance between stimuli, rather than their absolute positions on the continuum, likely defines the outcome of pairwise comparisons. Indeed, the theory assumes that the difference between the underlying discriminational processes of the stimuli, referred to as the *discriminational difference*, determines the observed dichotomous outcome. Furthermore, the theory assumes that because the individual discriminational processes form a Normal distribution on the continuum, the discriminational difference will also conform to a Normal distribution (Andrich, 1978). In this distribution, the mode (mean) represents the relative separation between the stimuli, and its dispersion indicates the variability of that separation.

Figure 1b illustrates the distribution of the discriminial difference for the hypothetical texts depicted in Figure 1a. The figure indicates that the judge perceives Text B as having significantly higher quality than Text A. This conclusion is supported by two key observations: the positive difference between their modal discriminial processes ( $T_B - T_A > 0$ ) and the probability area where the discriminial difference distinctly favors Text B over Text A, represented by the shaded gray area denoted as  $P(B > A)$ . As a result, the dichotomous outcome of this comparison is more likely to favor Text B over Text A.

### 3. The two critical issues in CJ literature

This section examines the two critical issues in the CJ literature that serve as the primary focus of the present study. The first is related to the overreliance on Thurstone’s Case V assumptions in the statistical analysis of CJ data. The second concern with the apparent disconnect between CJ’s approach to trait measurement and hypothesis testing.

#### 3.1. The Case V and the statistical analysis of CJ data

Thurstone noted from the outset that the general form of the theory, as outlined in Section 2, gave rise to a problem of trait scaling. The model required estimating more “unknown” parameters than the available pairwise comparisons (Thurstone, 1927a, pp. 267). To address this issue and facilitate the practical implementation of the theory, he developed five cases derived from this general form, each case progressively incorporated additional simplifying assumptions into the model.

In Case I, Thurstone postulated that pairs of stimuli would maintain a constant correlation across all comparisons. In Case II, he allowed multiple judges to undertake comparisons instead of confining evaluations to a single judge. In Case III, he posited that there was no correlation between stimuli. In Case IV, he assumed that the stimuli exhibited similar dispersions. Finally, in Case V, he replaced this assumption with the condition that stimuli had equal discriminial dispersions. Table 1 summarizes the assumptions of the general form and the five cases. For a detailed discussion of these cases and their progression, refer to Thurstone (1927a) and Bramley (2008, pp. 248–253).

Notably, despite relying on the most extensive set of simplifying assumptions (Bramley, 2008, pp. 253; Kelly et al., 2022, pp. 677), Case V remains the most widely used case in the CJ literature. This popularity stems mainly from its simplified statistical representation in the Bradley-Terry-Luce (BTL) model (Bradley and Terry, 1952; Luce, 1959). The BTL model mirrors the assumptions of Case V, with one notable distinction: whereas Case V assumes a Normal distribution for the

Table 1: Thurstones cases and their assumptions

Assumption	General form	Thurstone's					BTL model
		Case I	Case II	Case III	Case IV	Case V	
Discriminal process (distribution)	Normal	Normal	Normal	Normal	Normal	Normal	Logistic
Discriminal dispersion (between stimuli)	Different	Different	Different	Different	Similar	Equal	Equal
Correlation (between stimuli)	One per pair	Constant	Constant	Zero	Zero	Zero	Zero
How many judges compare?	Single	Single	Multiple	Multiple	Multiple	Multiple	Multiple

stimuli’s discriminational processes, the BTL model uses the more mathematically tractable Logistic distribution (Andrich, 1978; Bramley, 2008, pp. 254) (see Table 1). This substitution has little impact on the model’s estimation or interpretation, as the Normal and Logistic distributions exhibit analogous statistical properties, differing only by a scaling factor of approximately 1.7 (van der Linden, 2017a, pp. 16).

However, Thurstone originally developed Case V to provide a “rather coarse scaling” of traits (Thurstone, 1927a, pp. 269), prioritizing statistical simplicity over precision in trait measurement (Kelly et al., 2022, pp. 677). He explicitly warned against its untested application, stating that its use “should not be made without (an) experimental test” (Thurstone, 1927a, pp. 270). Furthermore, he acknowledged that some assumptions could prove problematic when researchers assess complex traits or heterogeneous stimuli (Thurstone, 1927b, pp. 376). Consequently, given that modern CJ applications frequently involve such traits and stimuli, two main assumptions of Case V and, by extension, of the BTL model may not consistently hold in theory or practice, namely the assumption of equal dispersion and zero correlation between stimuli.

### 3.1.1. The assumption of equal dispersions between stimuli

According to the theory, discrepancies in the discriminational dispersions of stimuli shape the distribution of the discriminational difference, exerting a direct influence on the outcome of pairwise comparisons. Figure 2a presents a thought experiment to illustrate this idea. In this experiment, a researcher can observe the discriminational processes for the texts depicted in Figure 1a. Furthermore, the figure assumes that the discriminational dispersion for Text A remains constant and that the texts are uncorrelated ( $\rho = 0$ ). The figure reveals that an increase in the uncertainty associated with the perception of Text B in comparison to Text A, ( $\sigma_B - \sigma_A$ ), broadens the distribution of their discriminational difference. This broadening affects the probability area where the discriminational difference distinctly favors Text B over Text A, expressed as  $P(B > A)$ , ultimately influencing the compari-

son outcome. Additionally, the figure reveals that when the discriminial dispersions of the texts are equal ( $\sigma_B - \sigma_A = 0$ ), the discriminial difference is more likely to favor Text B over Text A (shaded gray area), compared to situations where their dispersions differ.

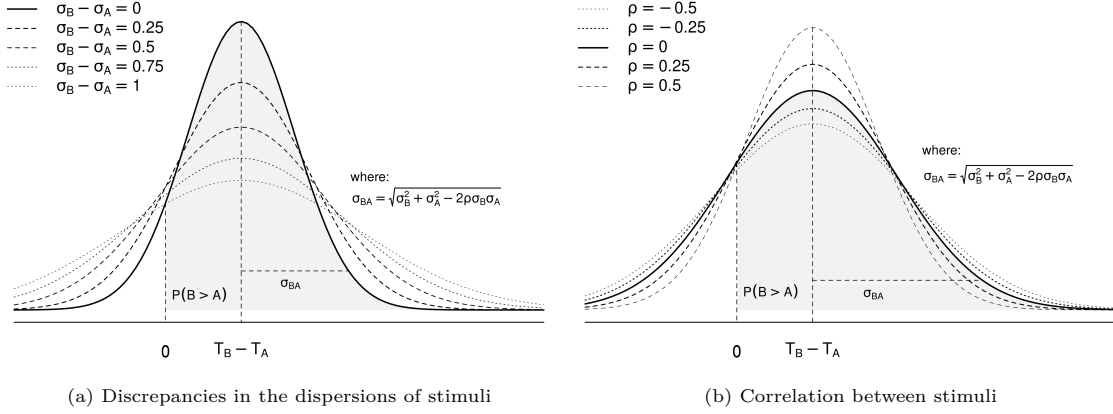


Figure 2: The effect of dispersion discrepancies and stimulus correlation on the distribution of the discriminial difference.

In experimental practice, however, this process occurs in reverse. Researchers first observe the comparison outcome and then use the BTL model to infer the discriminial difference between the stimuli and their respective discriminial processes (Thurstone, 1927b, pp. 373). Therefore, the outcome’s ability to reflect the “true” differences between stimuli largely depends on the validity of the model’s assumptions (Kohler et al., 2019, pp. 150), particularly the assumption of equal dispersions. For instance, when researchers observe a sample of outcomes favoring Text B over Text A and correctly assume equal dispersions between the texts, the BTL model estimates a discriminial difference distribution that accurately represents the “true” discriminial difference of the texts. This scenario is illustrated in Figure 2a, where the model’s discriminial difference distribution aligns with the “true” distribution, represented by the thick continuous line corresponding to  $\sigma_B - \sigma_A = 0$ . The accuracy of these discriminial difference ensures reliable estimates for the texts’ discriminial processes (citation needed?).

However, Thurstone argued that the assumption of equal dispersions may not be applicable when researchers assess complex traits or heterogeneous stimuli (Thurstone, 1927b, pp. 376), as these traits and stimuli can introduce judgment discrepancies due to their unique features (van Daal et al., 2016; Lesterhuis, 2018b; Chambers and Cunningham, 2022). Indeed, evidence of this violation may already be present in the CJ literature in the form of misfit statistics, which measure judgment discrepancies associated with specific stimuli (Pollitt, 2004, pp. 12; Goossens and De Maeyer, 2018,

pp. 20). For example, labeling texts as “misfits” indicates that comparisons involving these texts result in more judgment discrepancies than those involving other texts (Pollitt, 2012a,b; van Daal et al., 2016; Goossens and De Maeyer, 2018). These discrepancies, in turn, suggest that the discriminial differences for “misfit” texts have broader distributions, indicating that their discriminial processes may also exhibit more variation than that of other texts. A similar line of reasoning applies to the concept of “misfit” judges, whose evaluations deviate substantially from the shared consensus due to the unique characteristics of the stimuli or the judges themselves. These “misfit” judges and their associated deviations can give rise to additional statistical and measurement issues, which we discuss in more detail in Section 3.1.2.

Thus, model misspecification, in the form of an erroneous assumption of equal dispersions between stimuli, can give rise to significant statistical and measurement issues. For instance, the model may overestimate the degree to which the outcome accurately reflects the “true” discriminial differences between stimuli. This overestimation can result in researchers drawing spurious conclusions about these differences (McElreath, 2020, pp. 370) and, by extension, about the underlying discriminial processes of stimuli. Figure 2a also illustrates this issue when the model’s discriminial difference distribution aligns with the thick continuous line for  $\sigma_B - \sigma_A = 0$ , while the “true” discriminial difference follows any discontinuous line where  $\sigma_B - \sigma_A \neq 0$ . Additionally, if researchers recognize that misfit statistics highlight these critical differences in dispersions, the conventional CJ practice of excluding stimuli based on these statistics (Pollitt, 2012a,b; van Daal et al., 2016; Goossens and De Maeyer, 2018) can unintentionally discard valuable information. Such exclusions can introduce bias into trait estimates (Zimmerman, 1994; McElreath, 2020, chap. 12). The direction and magnitude of these biases are often unpredictable, as they depend on which stimuli are excluded from the analysis.

### 3.1.2. *The assumption of zero correlation between stimuli*

The correlation, represented by the symbol  $\rho$ , measures how much a judge’s perception of a specific trait in one stimulus depends on their perception of the same trait in another. As with the discriminial dispersions, this correlation shapes the distribution of the discriminial difference, directly impacting the outcomes of pairwise comparisons. Figure 2b presents a similar thought experiment as in Section 3.1.1 to illustrate this idea. The illustration now assumes that the discriminial dispersions for both texts remain constant. The figure reveals that as the correlation between the texts increases, the distribution of their discriminial difference becomes narrower. This narrowing affects the area under the curve where the discriminial difference distinctly favors Text B over Text A, denoted as

$P(B > A)$ , thus influencing the comparison outcome. Furthermore, the figure shows that when two texts are independent or uncorrelated ( $\rho = 0$ ), their discriminial difference is less likely to favor Text B over Text A (shaded gray area) compared to scenarios where the texts are highly correlated.

Off course, in experimental practice, researchers approach this process in reverse. They begin by observing the sample of outcomes favoring Text B over Text A and then use the BTL model to estimate the discriminial difference and the discriminial processes of the stimuli. Given that the BTL model assumes independent discriminial processes across comparisons, if this assumption holds, then the model estimates a discriminial difference distribution that accurately reflects the “true” discriminial difference of the texts. This scenario is also illustrated in Figure 2b when the discriminial difference distribution of the model aligns with the “true” distribution, represented by the thick continuous line corresponding to  $\rho = 0$ . Once more, the estimation accuracy of the discriminial difference ensures reliable estimates for the discriminial processes of the texts (citation needed?).

Notably, Thurstone attributed the independence of stimuli to the cancellation of potential judges’ biases. He argued that this cancellation resulted from two opposing and equally weighted effects occurring during pairwise comparisons (Thurstone, 1927a, pp. 268). Andrich (1978) provided a mathematical demonstration of this cancellation using the BTL model under the assumption of discriminial processes with additive biases. However, it is easy to imagine at least two scenarios in which the zero correlation assumption is almost certainly invalid: when the pairwise comparison involves multidimensional, complex traits with heterogeneous stimuli and when an additional hierarchical structure is relevant to the stimuli.

In the first scenario, the intricate aspects of multidimensional, complex traits may introduce dependencies between the stimuli due to certain judges’ biases that resist cancellation. Research on text quality suggests that when judges evaluate these traits, they often rely on various intricate characteristics of the stimuli to form their judgments (van Daal et al., 2016; Lesterhuis, 2018b; Chambers and Cunningham, 2022). These additional relevant characteristics, which are unlikely to be equally weighted or opposing, can exert an uneven influence on judges’ perceptions, creating biases in their judgments and, ultimately, introducing dependencies between stimuli (van der Linden, 2017b, pp. 346). For example, this could occur when a judge assessing the argumentative quality of a text places more weight on its grammatical accuracy than other judges, thereby favoring texts with fewer errors but weaker arguments. While direct evidence for this particular scenario is lacking,



studies such as [Pollitt and Elliott \(2003\)](#) demonstrate the presence of such biases, supporting the notion that the factors influencing pairwise comparisons may not always cancel out.

In the second scenario, the shared context or inherent connections created by additional hierarchical structures may further introduce dependencies between stimuli, a statistical phenomenon commonly known as clustering ([Everitt and Skrondal, 2010](#)). Despite the CJ literature acknowledging the existence of such hierarchical structures, the statistical handling of this additional source of dependence between stimuli has been inadequate. For instance, when CJ data incorporates multiple samples of stimuli from the same individuals, researchers frequently rely on (average) estimated BTL scores to conduct subsequent analyses and tests at the individual hierarchical level ([Bramley and Vitello, 2019](#); [Boonen et al., 2020](#); [Bouwer et al., 2023](#); [van Daal et al., 2017](#); [Jones et al., 2019](#); [Gijzen et al., 2021](#)). However, this approach can introduce additional statistical and measurement issues, which we discuss in greater detail in Section 3.2.

In any case, similar to Section 3.1.1, model misspecification due to an erroneous assumption of zero correlation between stimuli can lead to significant statistical and measurement issues. For instance, the model may over- or underestimate how accurately the outcome reflects the “true” discriminial differences between stimuli. Such inaccuracies can result in spurious inferences about these differences and, by extension, about the stimuli’s discriminial processes. This scenario is also illustrated by Figure 2b, when the model’s discriminial difference distribution aligns with the thick continuous line for  $\rho = 0$ , while the “true” discriminial difference follows any discontinuous line where  $\rho \neq 0$ .

The misspecification may arise from neglecting additional relevant traits, excluding judges based on misfit statistics, or ignoring hierarchical (grouping) structures. Neglecting relevant traits, such as judges’ biases, can cause dimensional mismatches in the BTL model, artificially inflating the trait’s reliability ([Hoyle, 2023](#), pp. 341) or, worse, introducing bias into the trait’s estimates ([Ackerman, 1989](#)). Excluding judges based on misfit statistics risks discarding valuable information, which may further bias the trait’s estimates ([Zimmerman, 1994](#); [McElreath, 2020](#), chap. 12). Finally, ignoring hierarchical structures may reduce the precision of model parameter estimates, potentially amplifying the overestimation of the trait’s reliability ([Hoyle, 2023](#), pp. 482).

### *3.2. The disconnect between trait measurement and hypothesis testing*

Building on the previous section, it is clear that, despite its limitations, the BTL model is commonly used as a measurement model in CJ assessments. A measurement model specifies how

manifest variables contribute to the estimation of latent variables (Everitt and Skrondal, 2010). For example, when evaluating writing quality, researchers use the BTL model to process the dichotomous outcomes resulting from the pairwise comparisons (the manifest variables) to estimate scores that reflect the underlying level of writing quality (the latent variable) (Laming, 2004; Pollitt, 2012b; Whitehouse, 2012; van Daal et al., 2016; Lesterhuis, 2018a; Coertjens et al., 2017; Goossens and De Maeyer, 2018; Bouwer et al., 2023).

Researchers then typically use these estimated BTL scores, or their transformations, to conduct additional analyses or hypothesis tests. For example, these scores have been used to identify ‘misfit’ judges and stimuli (Pollitt, 2012b; van Daal et al., 2016; Goossens and De Maeyer, 2018), detect biases in judges’ ratings (Pollitt and Elliott, 2003; Pollitt, 2012b), calculate correlations with other assessment methods (Goossens and De Maeyer, 2018; Bouwer et al., 2023), or test hypotheses related to the underlying trait of interest (Bramley and Vitello, 2019; Boonen et al., 2020; Bouwer et al., 2023; van Daal et al., 2017; Jones et al., 2019; Gijzen et al., 2021).

However, the statistical literature advises caution when using estimated scores for additional analyses and tests. A key consideration is that BTL scores are parameter estimates that inherently carry uncertainty. Ignoring this uncertainty can bias the analysis and reduce the precision of hypothesis tests. Notably, the direction and magnitude of such biases are often unpredictable. Results may be attenuated, exaggerated, or remain unaffected depending on the degree of uncertainty in the scores and the actual effects being tested (Kline, 2023, pp. 25; Hoyle, 2023, pp. 137). Finally, the reduced precision in hypothesis tests diminishes their statistical power, increasing the likelihood of committing type-I or type-II errors (McElreath, 2020).

In aggregate, researchers’ inadequate handling of violations to the assumptions of equal dispersion and zero correlation between stimuli, coupled with the apparent disconnect between CJ’s approach to trait measurement and hypothesis testing, can potentially compromise the reliability of the trait estimates and, by extension, their validity (Perron and Gillespie, 2015, pp. 2). Consequently, adopting a more systematic and integrated approach to handling these assumptions and examining the factors influencing CJ experiments could offer several statistical and measurement benefits, including the ability to address these issues.

## 4. Updating the theoretical and statistical model

This section uses the structural approach to causal inference (Pearl, 2009; Pearl et al., 2016) to articulate a theoretical model that captures the core principles of Thurstone’s theory. The model also incorporates various assessment design features relevant to CJ experiments, such as the selection of judges, stimuli, and comparisons. Finally, the section employs Bayesian inference methods to transform these theoretical and practical elements into a statistical model that facilitates the analysis of pairwise comparison data. See Section 7 for an overview of the statistical and causal inference concepts required for the development of this section.

### 4.1. The theoretical model

The theoretical model uses structural causal models (SCMs) and directed acyclic graphs (DAGs) (Pearl, 2009; Pearl et al., 2016; Gross et al., 2018; Neal, 2020) to formally and graphically represent the assumed causal structure of the CJ system. First, *the population model* is created to represent a conceptual population of CJ experiments. The model then integrates various assessment design features relevant to CJ experiments, leading to the development of *the sample-comparison model*.

#### 4.1.1. The population model

In this section we focus on the identification of several parameters of interest from the theoretical model, together with various assessment design features relevant to CJ experiments. The main reason to focus on identification is that if these parameters cannot be identified using “infinite” and error free data, we certainly cannot learn anything about it using finite data. So, for the time being, we assume large samples and error-free measurements (Schuessler and Selb, 2023, pp. 5)

Assuming population data or more commonly known as census data, we ...

The (latent) discriminial difference of the stimuli directly determines the (manifest) outcome of the pairwise comparisons

The (latent) “perceived” discriminial processes for the stimuli directly determines their discriminial difference

The (latent) “true” discriminial processes for the stimuli and the judges’ biases directly determines their (latent) “perceived” discriminial processes

without loosing generality, the (latent) “perceived” and “true” discriminial processes for the stimuli can be depicted in a vector for each judge, as in

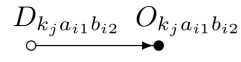


Figure 3

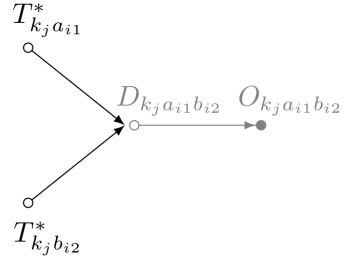


Figure 4

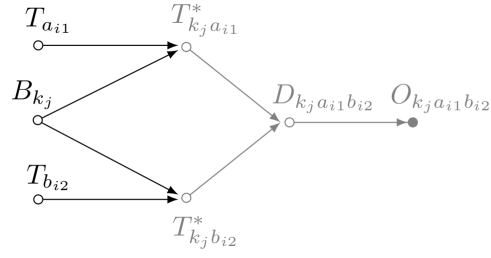


Figure 5

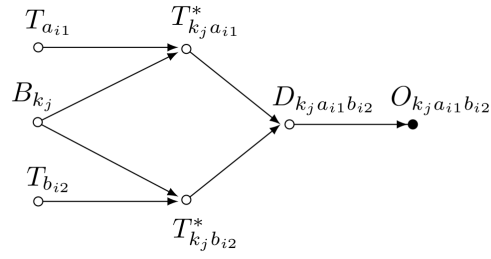


Figure 6

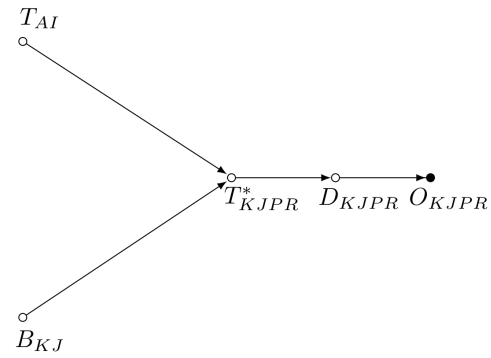


Figure 7

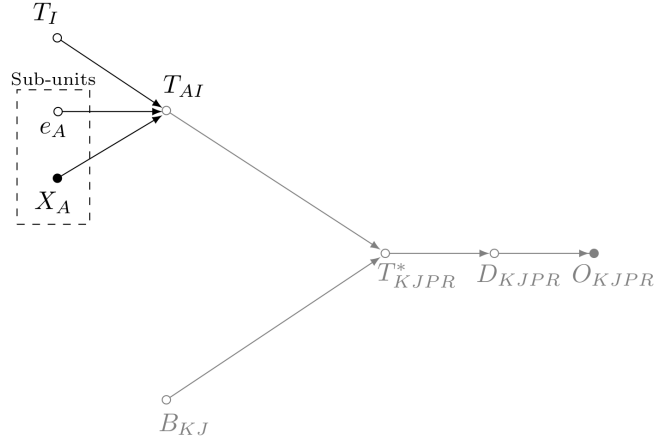


Figure 8

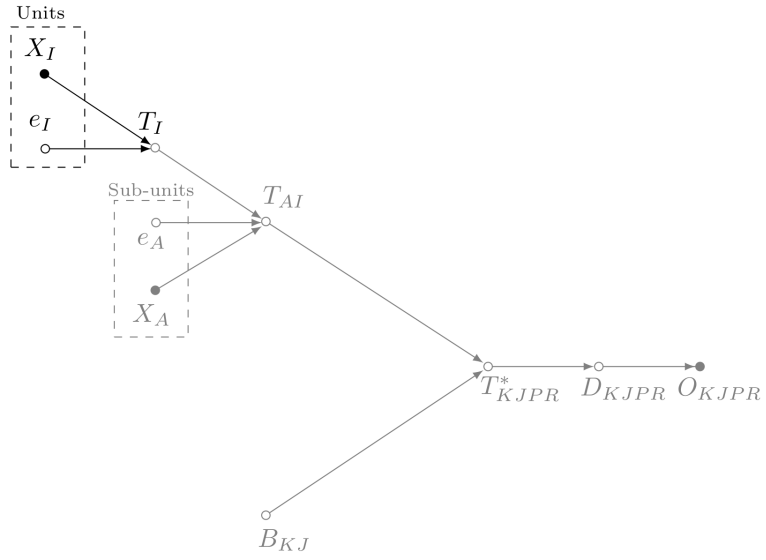


Figure 9

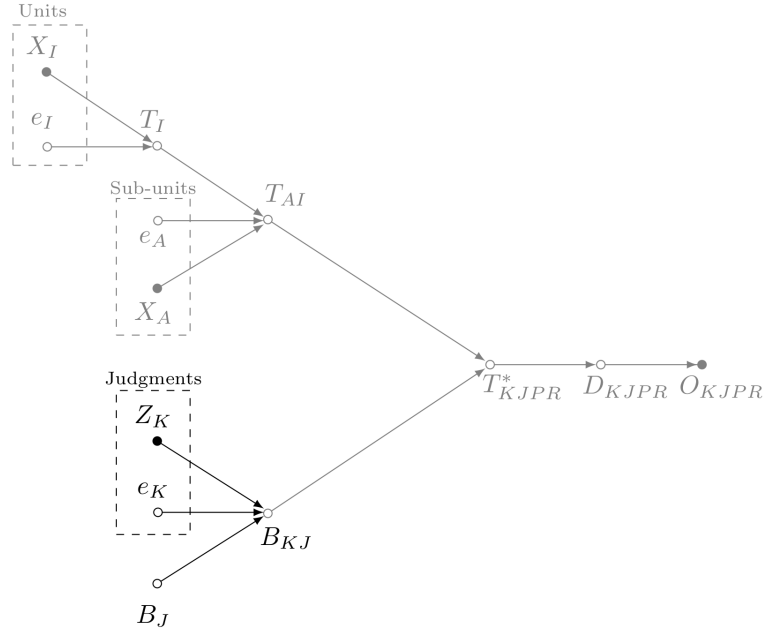


Figure 10

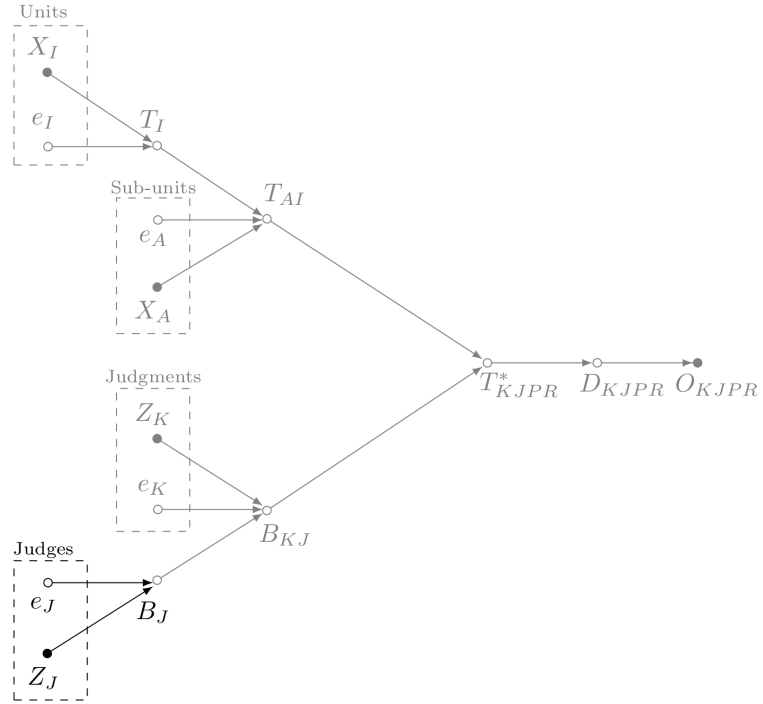


Figure 11

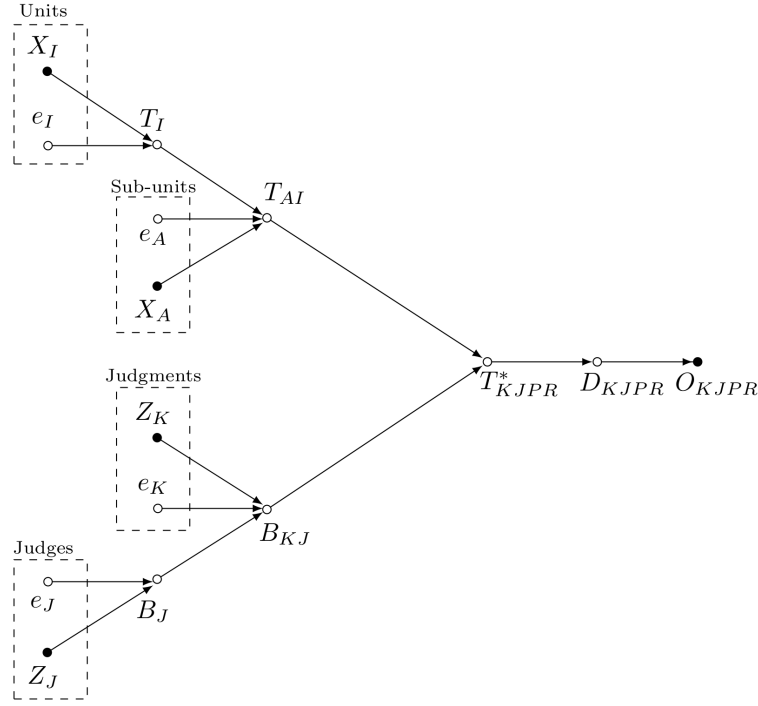


Figure 12

#### 4.1.2. The sample-comparison model

Considering the sampling mechanism

Considering comparison mechanisms

#### 4.2. From theory to statistics

Bayesian inference procedures offer three key advantages. First, they are well-suited to handling complex and overparameterized models, enabling researchers to estimate models where the number of parameters exceeds the number of observations for estimation (Baker, 1998; Kim and Cohen, 1999). Second, they allow researchers to incorporate prior information, which helps constrain parameters within specified bounds. This capability addresses challenges such as non-convergence or improper parameter estimation that often arise in complex models when analyzed with frequentist methods (Martin and McDonald, 1975; Seaman III et al., 2011). Finally, Bayesian methods are particularly effective at drawing inferences from small sample sizes, where relying on the asymptotic properties of frequentist approaches may not be justified (Baldwin and Fellingham, 2013; Lambert et al., 2006; Depaoli, 2014).

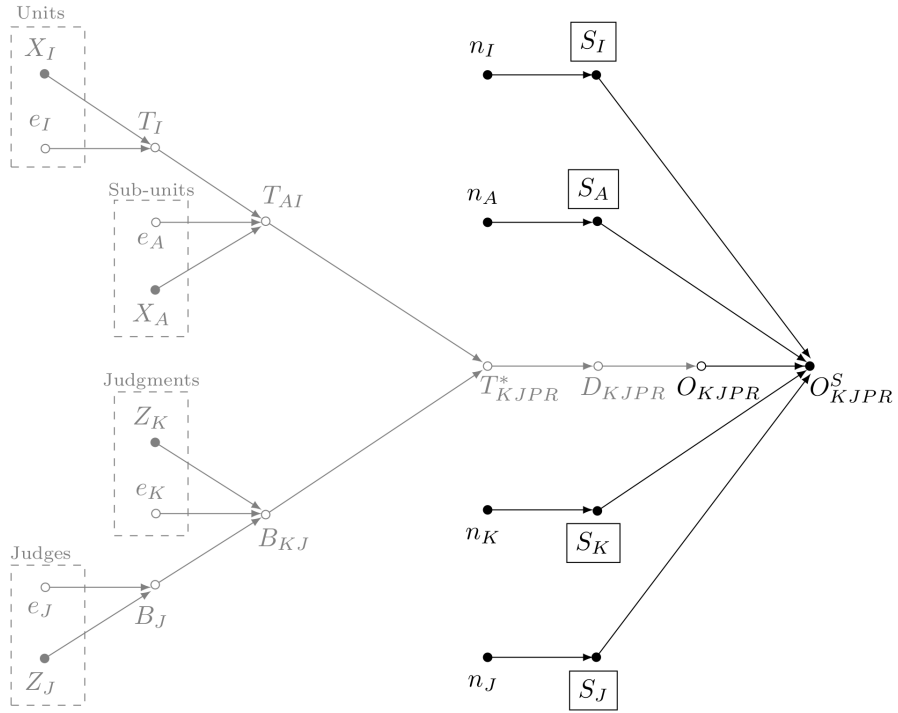


Figure 13

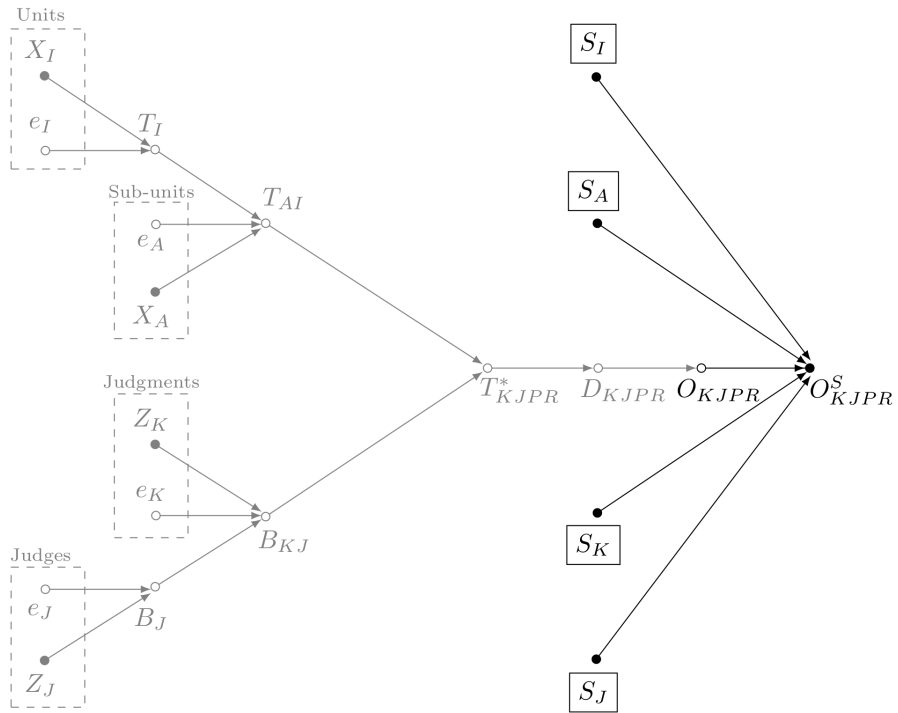


Figure 14



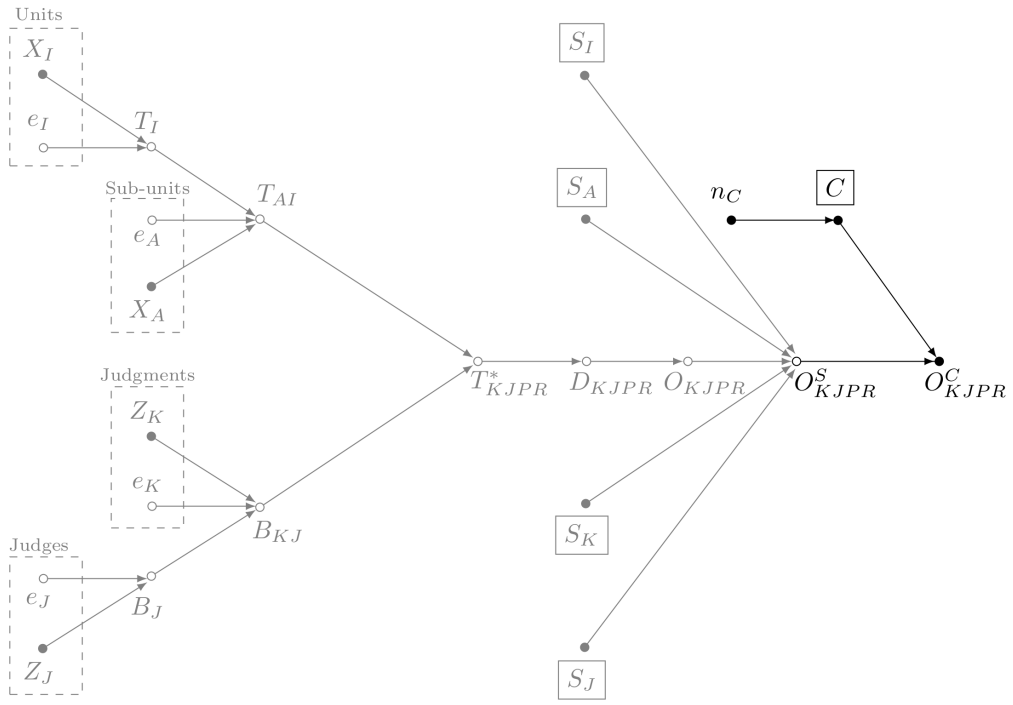


Figure 15

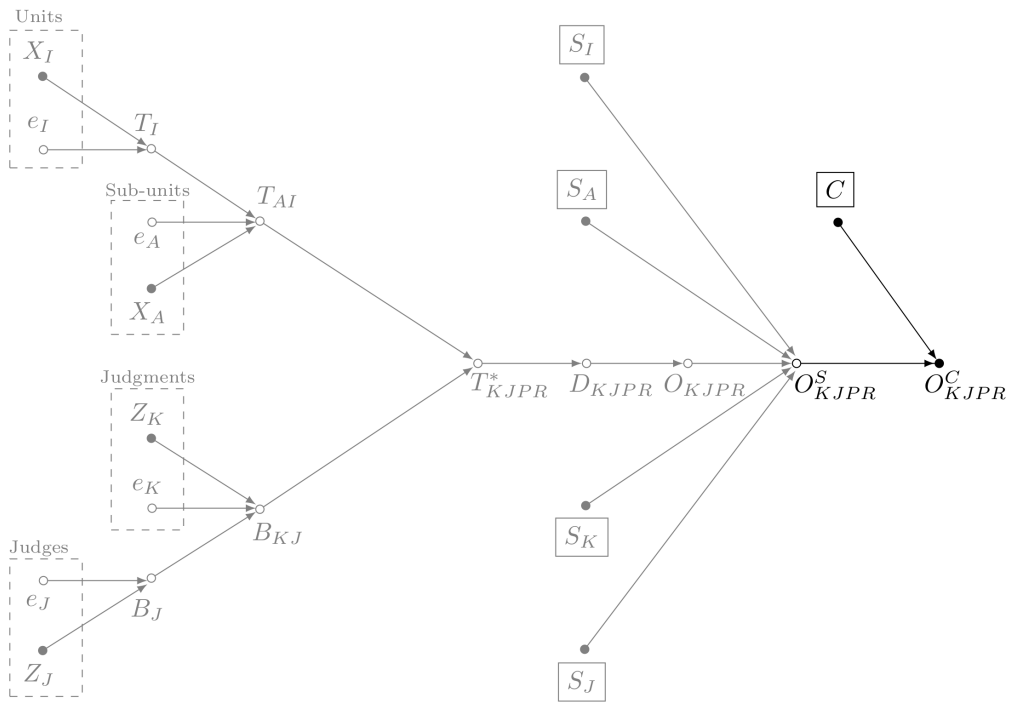


Figure 16

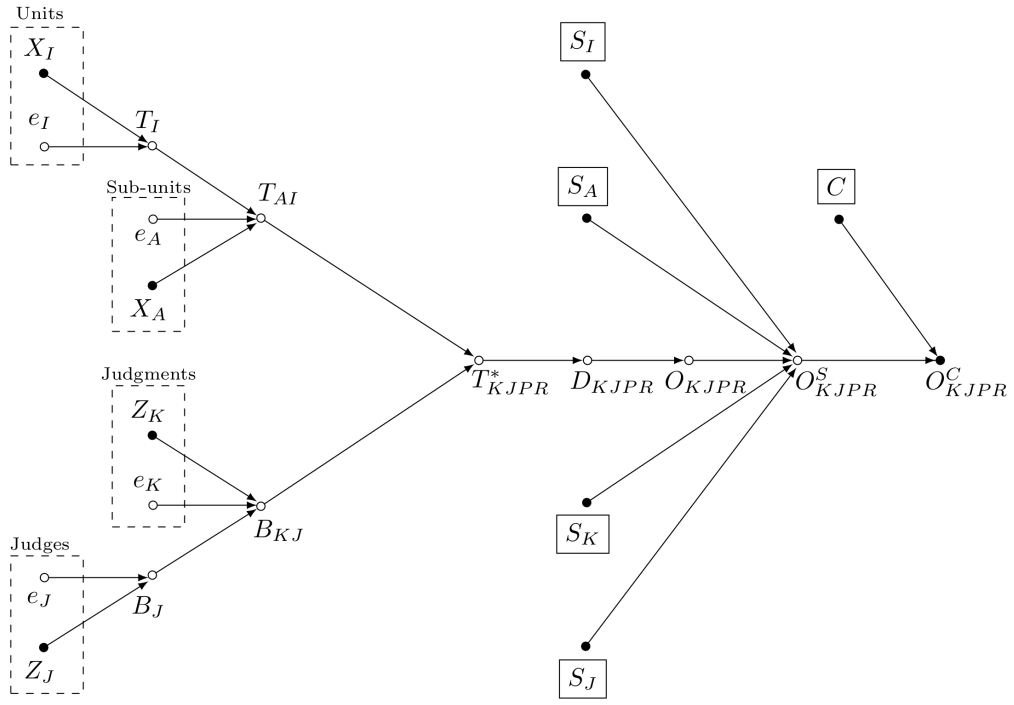


Figure 17

## 5. Discussion

### 5.1. Findings

### 5.2. Limitations and further research

## 6. Conclusion

## Declarations

**Funding:** The project was founded through the Research Fund of the University of Antwerp (BOF).

**Financial interests:** The authors have no relevant financial interest to disclose.

**Non-financial interests:** The authors have no relevant non-financial interest to disclose.

**Ethics approval:** The University of Antwerp Research Ethics Committee has confirmed that no ethical approval is required.

**Consent to participate:** Not applicable

**Consent for publication:** All authors have read and agreed to the published version of the manuscript.

**Availability of data and materials:** No data was utilized in this study.

**Code availability:** All the code utilized in this research is available in the digital document located at: [https://jriverspejo.github.io/paper2\\_manuscript/](https://jriverspejo.github.io/paper2_manuscript/).

**AI-assisted technologies in the writing process:** The authors utilized a range of AI-based language tools throughout the preparation of this work. They occasionally employed the tools to refine phrasing and optimize wording, ensuring appropriate language use and enhancing the manuscript's clarity and coherence. The authors take full responsibility for the final content of the publication.

**CRedit authorship contribution statement:** *Conceptualization:* S.G., S.D.M., T.vD., and J.M.R.E.; *Methodology:* S.D.M., T.vD., and J.M.R.E.; *Software:* J.M.R.E.; *Validation:* J.M.R.E.; *Formal Analysis:* J.M.R.E.; *Investigation:* J.M.R.E.; *Resources:* S.G., S.D.M., and T.vD.; *Data curation:* J.M.R.E.; *Writing - original draft:* J.M.R.E.; *Writing - review and editing:* S.G., S.D.M., and T.vD.; *Visualization:* J.M.R.E.; *Supervision:* S.G. and S.D.M.; *Project administration:* S.G. and S.D.M.; *Funding acquisition:* S.G. and S.D.M.

## 7. Appendix

This section introduces fundamental statistical and causal inference concepts necessary for understanding the core theoretical principles described in this document. It does not, however, offer a comprehensive overview of causal inference methods. Readers seeking more in-depth understanding may wish to explore introductory papers such as [Pearl \(2010\)](#), [Rohrer \(2018\)](#), [Pearl \(2019\)](#), and [Cinelli et al. \(2020\)](#). They may also find it helpful to consult introductory books like [Pearl and Mackenzie \(2018\)](#), [Neal \(2020\)](#), and [McElreath \(2020\)](#). For more advanced study, readers may refer to seminal intermediate papers such as [Neyman \(1923\)](#), [Rubin \(1974\)](#), [Spirtes et al. \(1991\)](#), and [Sekhon \(2009\)](#), as well as books such as [Pearl \(2009\)](#), [Morgan and Winship \(2014\)](#), and [Hernán and Robins \(2020\)](#).

### 7.1. Empirical research and randomized experiments

Empirical research uses evidence from observation and experimentation to address real-world challenges. In this context, researchers typically formulate their research questions as *estimands* or *targets of inference*, i.e., the specific quantities they seek to determine ([Everitt and Skrondal, 2010](#)). For instance, researchers might be interested in answering the following question: “To what extent do different teaching methods ( $T$ ) influence students’ ability to produce high-quality written texts ( $Y$ )?” To investigate this, researchers could randomly assign students to two groups, each exposed to a different teaching method ( $T_i = \{1, 2\}$ ). Then, they would perform pairwise comparisons, generating a dichotomous outcome ( $Y_i = \{0, 1\}$ ) showing which student exhibits more of the ability. In this scenario, the research question can be rephrased as the estimand, “*On average*, is there a difference in the ability to produce high-quality written texts between the two groups of students?” and this estimand can be mathematically represented by the random associational quantity in Equation 1, where  $E[\cdot]$  denotes the expected value.

$$E[Y_i | T_i = 1] - E[Y_i | T_i = 2] \tag{1}$$

Researchers would then proceed to identify the estimands. *Identification* is concerned with determining whether an estimator can accurately compute an estimand based on its assumptions, regardless of random variability ([Schuessler and Selb, 2023](#), pp. 4). An *estimator* is a method or function that transforms data into an estimate ([Neal, 2020](#)). *Estimates* are numerical values that approximate the estimand. They are derived through *estimation*, which refers to the process of integrating data with an estimator ([Everitt and Skrondal, 2010](#)). The Identification-Estimation

flowchart (McElreath, 2020; Neal, 2020) in Figure 18 provides a visual representation of the process of transitioning from estimands to estimates.

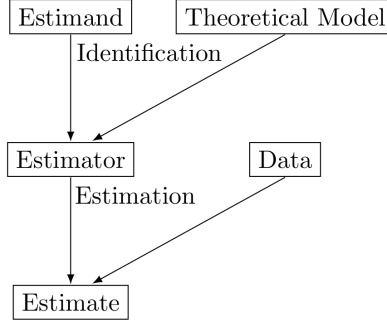


Figure 18: Identification-Estimation flowchart. Extracted and slightly modified from Neal (2020, pp. 32)

While numerous methods can approximate an estimand, researchers prioritize estimators with desirable identification properties that ensure the accuracy of estimates. For instance, the Z-test is an estimator known for its effectiveness when comparing groups' proportions, yielding accurate estimates when the underlying assumptions of the statistic are met (Kanji, 2006). If this is the case, the Z-test is expressed as a signal-to-noise statistic  $Z = (\hat{p}_1 - \hat{p}_2) / \hat{s}_p$ . The signal is defined as the difference between the groups' sample proportions,  $\hat{p}_1 = \sum_{i=1}^{n_1} Y_i / n_1$  and  $\hat{p}_2 = \sum_{i=1}^{n_2} Y_i / n_2$ , analogous to  $E[Y_i | T_i = 1]$  and  $E[Y_i | T_i = 2]$ , respectively. The noise, represented by  $\hat{s}_p$ , is defined as the unpooled sample variability observed between the two groups.

However, researchers often seek to uncover the mechanisms underlying specific data and establish causal relationships rather than simply estimate associations. In the example, researchers can interpret the associational estimate represented by the Z-statistic as causal. This interpretation relies on the data meeting the assumptions of the Z-test and being collected through a randomized experiment.

Randomized experiments are widely recognized as the gold standard in evidence-based science (Hariton and Locascio, 2018; Hansson, 2014). This recognition stems from their ability to enable researchers interpret associational estimates as causal. They achieve this by ensuring data, and by extension an estimator, satisfies several key properties, such as common support, no interference, and consistency (Morgan and Winship, 2014; Neal, 2020). The most critical property, however, is the elimination of confounding. *Confounding* occurs when an external variable  $X$  simultaneously influences the outcome  $Y$  and the variable of interest  $T$ , resulting in spurious associations (Everitt and Skrondal, 2010). Randomization addresses this issue by decoupling the association between

the intervention allocation  $T$  from any other variable  $X$  (Morgan and Winship, 2014; Neal, 2020).

Nevertheless, researchers often face constraints that limit their ability to conduct randomized experiments. These constraints include ethical concerns, such as the assignment of individuals to potentially harmful interventions, and practical limitations, such as the infeasibility of, for example, assigning individuals to genetic modifications or physical impairments (Neal, 2020). In these cases, causal inference offers a valuable alternative for generating causal estimates and understanding the mechanisms underlying specific data. In addition, the framework can provide significant theoretical insights that can enhance the design of experimental and observational studies (McElreath, 2020).

## 7.2. Identification under causal inference

Unlike classical statistical modeling, which focuses primarily on summarizing data and inferring associations, the *causal inference* framework is designed to identify causes and estimate their effects using data (Shaughnessy et al., 2010; Neal, 2020). The framework uses rigorous mathematical techniques to address the *fundamental problem of causality* (Pearl, 2009; Pearl et al., 2016; Morgan and Winship, 2014). This problem revolves around the question, “What would have happened ‘in the world’ under different circumstances?” This question introduces the concept of counterfactuals, which are instrumental in defining and identifying causal effects.

*Counterfactuals* are hypothetical scenarios that are *contrary to fact*, where alternative outcomes resulting from a given cause are neither observed nor observable (Neal, 2020; Counterfactual, 2024). The structural approach to causal inference (Pearl, 2009; Pearl et al., 2016) provides a formal framework for defining counterfactuals. For instance, in the scenario described in Section 7.1, the approach begins by defining the *individual causal effect* (ICE) as the difference between each student’s potential outcomes, as in Equation 2.

$$\tau_i = Y_i | do(T_i = 1) - Y_i | do(T_i = 2) \quad (2)$$

where  $do(T_i = t)$  represents the intervention operator,  $Y_i | do(T_i = 1)$  represents the potential outcome under intervention  $T_i = 1$ , and  $Y_i | do(T_i = 2)$  represents the potential outcome under intervention  $T_i = 2$ . Here, an *intervention* involves assigning a constant value to the treatment variable for each student’s potential outcomes. In contrast, *conditioning* involves restricting the focus to the subset of the population defined by the conditioning variable (Neal, 2020, pp. 32) (see

Equation 1).

Note that if a student is assigned to intervention  $T_i = 1$ , the potential outcome under  $T_i = 2$  becomes a counterfactual, as it is no longer observed nor observable. To address this challenge, the structural approach extends the ICE to the *average causal effect* (ACE, Equation 3), representing the average difference between observed potential outcomes and their counterfactual counterparts.

$$\tau = E[\tau_i] = E[Y_i | do(T_i = 1)] - E[Y_i | do(T_i = 2)] \quad (3)$$

Even though counterfactuals are unobserved and unobservable, researchers can still identify the ACE from associational estimates by leveraging the structural approach. The approach identifies the ACE by statistically conditioning data on a *sufficient adjustment set* of variables  $X$  (Pearl, 2009; Pearl et al., 2016; Morgan and Winship, 2014). This *sufficient* set (potentially empty) must block all non-causal paths between  $T$  to  $Y$  without opening new ones. If such a set exists, then  $T$  and  $Y$  are *d-separated* by  $X$  ( $T \perp Y | X$ ) (Pearl, 2009), and  $X$  satisfies the *backdoor criterion* (Neal, 2020, pp 37).

Conditioning on a sufficient adjustment set enables researchers to estimate the ACE, even when the data comes from an observational study. This process is feasible because such conditioning ensures that the ACE estimator satisfies several critical properties, including confounding elimination (Morgan and Winship, 2014). Naturally, the validity of claims about the causal effects of  $T$  on  $Y$  now hinges on the assumption that  $X$  serves as a sufficient adjustment set. However, as Kohler et al. (2019, pp. 150) noted, drawing conclusions about the real world from observed data inevitably requires assumptions. This requirement holds true for both observational and experimental data.

For instance, If researchers cannot conduct the randomized experiments described in Section 7.1 and must instead rely on observational data, they can still identify the ACE as long as a variable  $X$ , such as the socio-economic status of the school, satisfies the backdoor criterion. Under these circumstances, researchers first identify the *conditional average causal effect* (CACE, Equation 4)

$$E[Y_i | T_i = t, X] \quad (4)$$

From the CACE, they identify the ACE from associational quantities as in Equation 5. Here,

$E_X[\cdot]$  represents the marginal expected value over  $X$  (Morgan and Winship, 2014).

$$\begin{aligned}\tau &= E[Y_i \mid do(T_i = 1)] - E[Y_i \mid do(T_i = 2)] \\ &= E_X [E[Y_i \mid T_i = 1, X] - E[Y_i \mid T_i = 2, X]]\end{aligned}\tag{5}$$

Notably, the approach extends the ACE identification for a continuous variable  $T$  as in Equation 6, ensuring broad applicability across different causal scenarios (Neal, 2020, pp. 45)

$$\begin{aligned}\tau &= E[Y_i \mid do(T_i = t)] \\ &= dE_X [E[Y_i \mid T_i = t, X]] / dt\end{aligned}\tag{6}$$

### 7.3. SCMs and DAGs

The structural approach to causal inference uses SCMs and DAGs to formally and graphically represent the presumed causal structure underlying the ACE (Pearl, 2009; Pearl et al., 2016; Gross et al., 2018; Neal, 2020). Essentially, SCMs and DAGs serve as *conceptual (theoretical) models* that facilitate valid causal inference by guiding researchers in determining which statistical models can identify an estimand (ACE, CACE, or other), assuming the depicted causal structure is correct (McElreath, 2020).

SCMs and DAGs support causal inference through two key advantages. First, regardless of complexity, they can represent various causal structures using only five fundamental building blocks (Neal, 2020; McElreath, 2020). This feature allows researchers to decompose complex structures into manageable components, facilitating their analysis (McElreath, 2020). Second, they depict causal relationships in a non-parametric and fully interactive way. This flexibility enables feasible ACE identification strategies without defining the variables' data types, the functional form between them, or their parameters (Pearl et al., 2016, pp. 35). Figure 18 shows the role of theoretical models in the inference process.

Figures 19, 20, 21, 22, and 23 display these five fundamental building blocks. The left panels of the figures show the formal mathematical models, represented by the SCMs, defined in terms of a set of *endogenous* variables  $X = \{X_1, X_2, X_3\}$ , a set of *exogenous* variables  $E = \{e_{X1}, e_{X2}, e_{X3}\}$ , and a set of functions  $F = \{f_{X1}, f_{X2}, f_{X3}\}$  (Pearl, 2009; Cinelli et al., 2020). Endogenous variables are those whose causal mechanisms a researcher chooses to model (Neal, 2020). In contrast, exogenous variables represent *errors* or *disturbances* arising from omitted factors that the investigator chooses



not to model explicitly (Pearl, 2009, pp. 27,68). Lastly, the functions, referred to as *structural equations*, express the endogenous variables as non-parametric functions of other variables. These functions use the symbol ‘:=’ to denote the asymmetrical causal dependence of the variables and the symbol ‘ $\perp$ ’ to represent *d-separation*, a concept akin to (conditional) independence.

Notably, every SCM has an associated DAG (Pearl et al., 2016; Cinelli et al., 2020). The right panels of the figures display these DAGs. A DAG is a graph consisting of nodes connected by edges, where the nodes represent random variables. The term *directed* means that the edges extend from one node to another, with arrows indicating the direction of causal influence. The term *acyclic* implies that the causal influences do not form loops, ensuring the influences do not cycle back on themselves (McElreath, 2020). DAGs represent observed variables as solid black circles, while they use open circles for unobserved (latent) variables (Morgan and Winship, 2014). Although DAGs typically omit exogenous variables for simplicity (the *standard representation*), including these variables, as demonstrated in the *magnified representation* in the figures, can be advantageous. Their inclusion helps to highlight potential issues related to conditioning and confounding (Cinelli et al., 2020).

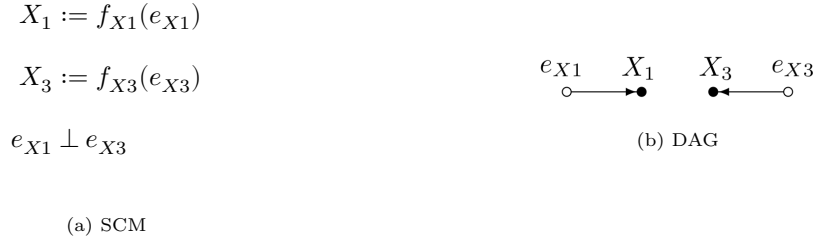


Figure 19: Two unconnected nodes



Figure 20: Two connected nodes or descendant

$$X_1 := f_{X1}(e_{X1})$$

$$X_2 := f_{X2}(X_1, e_{X2})$$

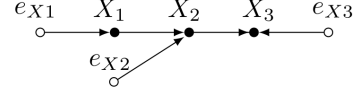
$$X_3 := f_{X3}(X_2, e_{X3})$$

$$e_{X1} \perp e_{X2}$$

$$e_{X1} \perp e_{X3}$$

$$e_{X2} \perp e_{X3}$$

(a) SCM



(b) DAG

Figure 21: Chain or mediator

$$X_1 := f_{X1}(X_2, e_{X1})$$

$$X_2 := f_{X2}(e_{X2})$$

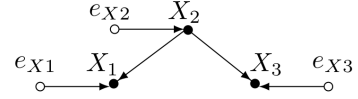
$$X_3 := f_{X3}(X_2, e_{X3})$$

$$e_{X1} \perp e_{X2}$$

$$e_{X1} \perp e_{X3}$$

$$e_{X2} \perp e_{X3}$$

(a) SCM



(b) DAG

Figure 22: Fork or confounder

$$X_1 := f_{X1}(e_{X1})$$

$$X_2 := f_{X2}(X_1, X_3, e_{X2})$$

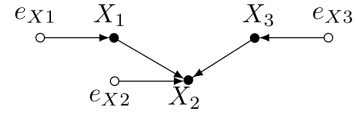
$$X_3 := f_{X3}(e_{X3})$$

$$e_{X1} \perp e_{X2}$$

$$e_{X1} \perp e_{X3}$$

$$e_{X2} \perp e_{X3}$$

(a) SCM



(b) DAG

Figure 23: Collider or immorality

A careful examination of the figures highlights the theoretical assumptions underlying the observed variables in these building blocks. SCM 19a and DAG 19b depict two unconnected nodes, representing a scenario where variables  $X_1$  and  $X_3$  are independent or not causally related. SCM 20a and DAG 20b illustrate two connected nodes, representing a scenario where a *parent* node  $X_1$  exerts a causal influence on a *child* node  $X_3$ . In this setup,  $X_3$  is considered a *descendant* of  $X_1$ . Additionally,  $X_1$  and  $X_3$  are described as *adjacent* because there is a *direct path* connecting. SCM 21a and DAG 21b depict a *chain*, where  $X_1$  influences  $X_2$ , and  $X_2$  influences  $X_3$ . In this configuration,  $X_1$  is a parent node of  $X_2$ , which is a parent node of  $X_3$ . This structure creates a *directed path* between  $X_1$  and  $X_3$ . Consequently,  $X_1$  is an *ancestor* of  $X_3$ , and  $X_2$  fully *mediates* the relationship between the two. SCM 22a and DAG 22b illustrate a *fork*, where variables  $X_1$  and  $X_3$  are both influenced by  $X_2$ . Here,  $X_2$  is a parent node that *confounds* the relationship between  $X_1$  and  $X_3$ . Finally, SCM 23a and DAG 23b show a *collider*, where variables  $X_1$  and  $X_3$  are concurrent causes of  $X_2$ . In this configuration,  $X_1$  and  $X_3$  are not causally related to each other but both influence  $X_2$  (an *immorality*). Notably, all building blocks assume the errors are independent of each other and from all other variables in the graph, as evidenced by the pairwise relations  $e_{X1} \perp e_{X2}$ ,  $e_{X1} \perp e_{X3}$ , and  $e_{X2} \perp e_{X3}$ .

Researchers can then use these building blocks to represent the scenario outlined in Section 7.2. SCM 27a and DAG 24b depict the plausible causal structure for this example. In this context, the variable  $X$  (socio-economic status of the school) acts as a confounder in the relationship between the teaching method  $T$  and the outcome  $Y$ . The figures display multiple descendant relationships such as  $X \rightarrow T$ ,  $X \rightarrow Y$ , and  $T \rightarrow Y$ . They also highlight unconnected node pairs, evident from the relationships  $e_T \perp e_X$ ,  $e_T \perp e_Y$ , and  $e_X \perp e_Y$ . Additionally, the figures show one fork,  $X \rightarrow \{T, Y\}$ , and two colliders:  $\{X, e_T\} \rightarrow T$  and  $\{X, T, e_Y\} \rightarrow Y$ .

#### 7.4. The probabilistic implications of SCMs and DAGs

Beyond their graphical capabilities, SCMs and DAGs can encode the probabilistic information embedded within a causal structure. They achieve this encoding by relying on three fundamental assumptions: the local Markov, the minimality, the causal edges assumption. The *local Markov assumption* encodes probabilistic independencies between variables by declaring that nodes in a graph are independent of all its non-descendants, given its parents (Neal, 2020, pp. 20). Meanwhile, the *minimality assumption* encodes probabilistic dependencies among variables by stating that every pair of adjacent nodes exhibits a dependency (Neal, 2020, pp. 21). Finally, the *causal edges assumption* encodes causal relationships between variables by declaring that each parent node acts

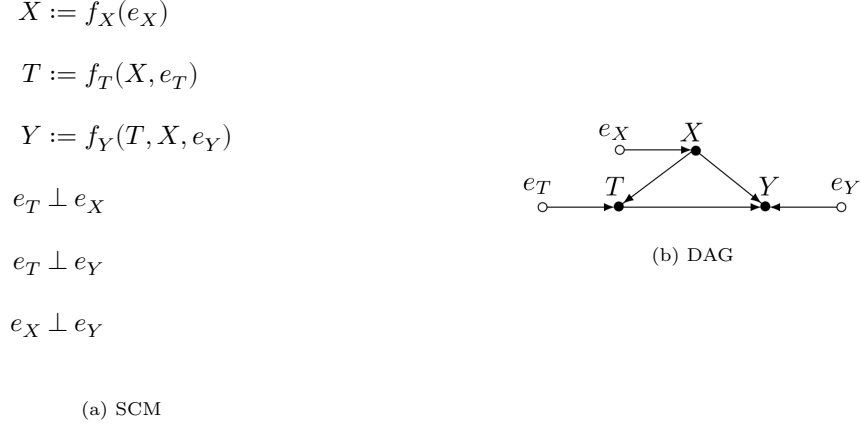


Figure 24: Plausible causal structure the scenario outlined in Section 7.2.

as a direct cause of its children (Neal, 2020, pp. 22). Figure 25 illustrates how these assumptions influence the statistical and causal interpretations of graphs.

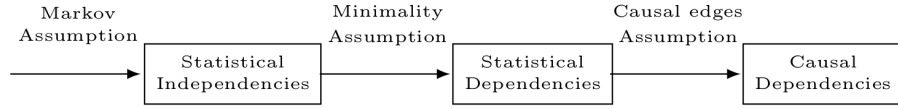


Figure 25: The flow of association and causation in graphs. Extracted and slightly modified from Neal (2020, pp. 31)

A notable implication of the assumptions underlying the probabilistic encoding is that any conceptual model described by an SCM and DAG can represent the joint distribution of variables more efficiently (Pearl et al., 2016, pp. 29). This expression takes the form of a product of conditional probability distributions (CPDs) of the type  $P(\text{child} \mid \text{parents})$ . This property is formally known as the *Bayesian Network factorization* (BNF, Equation 7) (Pearl et al., 2016, pp. 29; Neal, 2020, pp. 21). In this expression,  $pa(X_i)$  denotes the set of variables that are the parents of  $X_i$ .

$$P(X_1, X_2, \dots, X_P) = \prod_{p=1}^P P(X_i \mid pa(X_i)) \quad (7)$$

This encoding enables researchers with conceptual (theoretical) knowledge in the form of an SCM and DAG to predict patterns of (in)dependencies in the data. As highlighted by Pearl et al. (2016, pp. 35), these predictions depend solely on the structure of these conceptual models without requiring the quantitative details of the equations or the distributions of the errors. Moreover, once researchers observe empirical data, the patterns of (in)dependencies in this data can provide significant insights into the validity of the proposed conceptual model.

The five fundamental building blocks described in Section 7.3 clearly illustrate which (in)dependencies can SMCs and DAGs predict. For instance, applying the BNF to the causal structure shown in the SCM 19a and DAG 19b supports the theoretical assumption that the observed variables  $X_1$  and  $X_3$  are unconditionally independent or d-separated ( $X_1 \perp X_3$ ). This conclusion follows from expressing their joint probability distribution as  $P(X_1, X_3) = P(X_1)P(X_3)$  (Neal, 2020, pp. 24). Conversely, when  $X_3$  is unconditionally dependent on  $X_1$  ( $X_1 \not\perp X_3$ ), as depicted in the SCM 20a and DAG 20b, the BNF express their joint probability distribution as  $P(X_1, X_3) = P(X_3 | X_1)P(X_1)$ . Notably, these descriptions demonstrate the clear correspondence between the structural equations illustrated in Section 7.3 and the CPDs.

Beyond the insights gained from two-node structures, researchers can uncover more nuanced patterns of (in)dependencies from chains, forks, and colliders. These (in)dependencies apply to any data generated by these structures. For instance, applying the BNF to the chain structure depicted in the SCM 21a and DAG 21b allow researchers to represent the joint distribution for the observed variables as  $P(X_1, X_2, X_3) = P(X_1)P(X_2 | X_1)P(X_3 | X_2)$ . This expression implies that  $X_1$  and  $X_3$  are unconditionally dependent ( $X_1 \not\perp X_3$ ), but conditionally independent when controlling for  $X_2$  ( $X_1 \perp X_3 | X_2$ ). Moreover, in the fork structure shown in the SCM 22a and DAG 22b, researchers can express the joint distribution of the observed variables as  $P(X_1, X_2, X_3) = P(X_1 | X_2)P(X_2)P(X_3 | X_2)$ . This expression allows researchers to further infer that  $X_1$  and  $X_3$  are unconditionally dependent ( $X_1 \not\perp X_3$ ), but conditionally independent when controlling for  $X_2$  ( $X_1 \perp X_3 | X_2$ ). Finally, researchers analyzing the collider structure illustrated in the SCM 23a and DAG 23b can express the joint distribution of the observed variables as  $P(X_1, X_2, X_3) = P(X_1)P(X_2 | X_1, X_3)P(X_3)$ . This representation allows researchers to infer that  $X_1$  and  $X_3$  are unconditionally independent ( $X_1 \perp X_3$ ), but conditionally dependent when controlling for  $X_2$  ( $X_1 \not\perp X_3 | X_2$ ). The authors Pearl et al. (2016, pp. 37, 40, 41) and Neal (2020, pp. 25–26) provide the mathematical proofs for these conclusions.

Researchers can revisit the scenario in Section 7.2 using these insights. By applying the BNF to the SCM 27a and DAG 26b structures, they can represent the joint probability of the observed variables as  $P(Y, T, X) = P(Y | T, X)P(T | X)P(X)$ . Assuming the *direct effect* of  $T$  on  $Y$  ( $T \rightarrow Y$ , black path) is absent, researchers can infer from the joint probability that  $Y$  is not unconditionally independent of  $T$  ( $Y \not\perp T$ ). This dependency arises because a non-causal path exists between  $T$  and  $Y$  through  $X$  ( $T \leftarrow X \rightarrow Y$ , gray path), indicating that  $X$  is a confounder. However, researchers can observe that conditioning on  $X$ , depicted as a square in DAG 26b, results in  $Y \perp T | X$ , eliminating

the confounding effect and allowing the identification of the direct effect  $T \rightarrow Y$  if it exists.

$$X := f_X(e_X)$$

$$T := f_T(X, e_T)$$

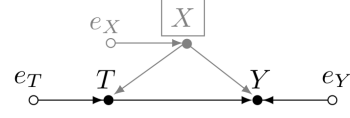
$$Y := f_Y(t, X, e_Y)$$

$$e_T \perp e_X$$

$$e_T \perp e_Y$$

$$e_X \perp e_Y$$

(a) SCM



(b) Conditioned DAG

Figure 26: Plausible causal structure the scenario outlined in Section 7.2.

### 7.5. From probability to causality

In addition to the assumptions required to encode probabilistic information, SCMs and DAGs rely on the modularity assumption to represent interventions  $P(Y_i | do(T_i = t))$  (see Section 7.2). Defining these interventions enables researchers to identify the ACE from associational quantities through statistical conditioning, effectively disentangling causation from mere association.

The modularity assumption underpins the concepts of Truncated Factorization and manipulated graphs, which are essential for representing interventions within SCMs and DAGs. The *modularity assumption* posits that intervening on a node modifies only the causal mechanism of that node, leaving others unchanged (Neal, 2020, pp. 34). Building on this principle, the *Truncated Factorization* (TF) simulates physical interventions by removing specific functions from the conceptual model and substituting them with constants, preserving the rest of the structure unchanged [Pearl\_2010]. The probabilistic implications of this factorization are formalized in Equation 8, where  $S$  denotes the subset of variables  $X_p$  directly influenced by the intervention. Complementing the TF, *manipulated graphs* are DAGs where specific edges have been removed to reflect the effects of interventions (Neal, 2020, pp. 34).

$$P(X_1, X_2, \dots, X_P | do(S)) = \begin{cases} \prod P(X_p | pa(X_p)) & \text{if } p \notin S \\ 1 & \text{otherwise} \end{cases} \quad (8)$$

Using the TF, researchers can define the *backdoor adjustment*, a method for identifying causal

effects. This adjustment specifies that if a variable  $X_p \in S$  serves as a *sufficient adjustment set* for the effect of  $X_a$  on  $X_b$ , then the ACE can be identified using Equation 9 (see Section 7.2).

$$P(X_a \mid do(X_b = x)) = \sum_{X_p} P(X_a \mid X_b = x, X_p)P(X_p) \quad (9)$$

Ultimately, backdoor adjustment enables researchers to express the ACE as:

$$\begin{aligned} \tau &= E[\tau_i] \\ &= E[X_a \mid do(X_b = 1)] - E[X_a \mid do(X_b = 2)] \\ &= \sum_{X_p} X_a \cdot P(X_a \mid X_b = 1, X_p)P(X_p) - \sum_{X_p} X_a \cdot P(X_a \mid X_b = 2, X_p)P(X_p) \end{aligned} \quad (10)$$

$$X := f_X(e_X)$$

$$T := t$$

$$Y := f_Y(t, X, e_Y)$$

$$e_T \perp e_X$$

$$e_T \perp e_Y$$

$$e_X \perp e_Y$$

(a) SCM

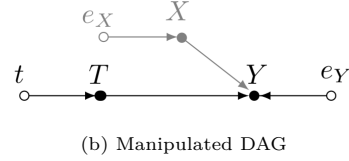


Figure 27: Plausible causal structure the scenario outlined in Section 7.2.

### 7.6. The estimation process

Researchers can use Bayesian inference methods to estimate the ACE. The approach begins by defining two probability distributions: the likelihood of the data,  $P(X_1, X_2, \dots, X_P \mid \theta)$ , and the prior distribution,  $P(\theta)$  (Everitt and Skrongdal, 2010), where  $X_P$  represents a random variable, and  $\theta$  represents a one-dimensional parameter space for simplicity. After observing empirical data, researchers can update the priors to posterior distributions using Bayes' rule (Jeffreys, 1998):

$$P(\theta \mid X_1, X_2, \dots, X_P) = \frac{P(X_1, X_2, \dots, X_P \mid \theta) \cdot P(\theta)}{P(X_1, X_2, \dots, X_P)} \quad (11)$$

Given that the denominator on the right-hand side of Equation 11 serves as a normalizing constant independent of the parameter  $\theta$ , researchers can simplify the posterior updating process into the

three steps depicted by Equation 12. First, they integrate new empirical data through the likelihood. Second, they update the parameters' priors to a posterior distribution according to Equation 12. Ultimately, they normalize these results to obtain a valid probability distribution.

$$P(\theta \mid X_1, X_2, \dots, X_P) \propto P(X_1, X_2, \dots, X_P \mid \theta) \cdot P(\theta) \quad (12)$$

Temporarily setting aside the definition of prior distributions  $P(\theta)$ , note that the posterior updating process depends heavily on the assumptions underlying the likelihood of the data. However, as the number of random variables,  $P$ , increases, this joint distribution becomes quickly intractable (Neal, 2020). This intractability is evident from Equation 13, where the likelihood distribution is expressed by multiple chained CPDs.

$$P(X_1, X_2, \dots, X_P \mid \theta) = P(X_1 \mid \theta) \prod_{p=2}^P P(X_i \mid X_{i-1}, \dots, X_1, \theta) \quad (13)$$

Nevertheless, researchers can manage the complexity of the likelihood by assuming that variables exhibit specific local (in)dependencies. These assumptions enhance model tractability and facilitate the estimation process. As described in Section 7.4, researchers can use SCMs and DAGs to formalize these local (in)dependencies, which leads to the BNF of the likelihood (Equation 14). From this point, any probabilistic programming language can model this simpler structure and generate the parameter's posterior distribution from its priors using Equation 11.

$$P(X_1, X_2, \dots, X_P \mid \theta) = P(X_1 \mid \theta) \prod_{p=2}^P P(X_i \mid pa(X_i), \theta) \quad (14)$$



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