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

Jose Manuel Rivera Espejo[a,*], Tine van Daal[a], Sven De Maeyer[a], Steven Gillis[b]

[a] *University of Antwerp, Training and education sciences,*

[b] *University of Antwerp, Linguistics,*

Abstract

This study revisits Thurstone's law of comparative judgment (CJ), focusing on two prominent issues of traditional approaches. First, it critiques the heavy reliance on Thurstone's Case V assumptions and, by extension, the Bradley-Terry-Luce (BTL) model when analyzing CJ data. Specifically, the study raises concerns about the assumptions of equal discriminal dispersions and zero correlation between the stimuli. While these assumptions simplify the trait measurement model, they may fail to capture the complexity of CJ data, potentially leading to unreliable and inaccurate trait estimates. Second, the study highlights the apparent disconnect between CJ's trait measurement and hypothesis testing processes. Although separating these processes simplifies the analysis of CJ data, it may also undermine the reliability of various statistical results derived from these processes.

To address these issues, the study extends Thurstone's general form using a systematic and integrated approach based on Causal and Bayesian inference methods. This extension integrates core theoretical principles alongside key assessment design features relevant to CJ experiments, such as the selection of judges, stimuli, and comparisons. It then translates these elements into a probabilistic statistical model for analyzing dichotomous CJ data, overcoming the rigid assumptions of Case V and the BTL model.

Finally, the study emphasizes the relevance of this extension for contemporary empirical CJ research, particularly stressing the need for bespoke CJ models tailored to the experiments and data assumptions. It also lays the foundation for broader applications, encouraging researchers across the social sciences to adopt more robust and interpretable methodologies.

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

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 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; Crompvoets 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 validity indicates the capacity of CJ scores to represent accurately the traits under measurement (Whitehouse, 2012; van Daal et al., 2016; Lesterhuis, 2018a; Bartholomew et al., 2018; Bouwer et al., 2023). Lastly, research on its practical applicability highlights CJ's 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, research in this domain remains unsystematic and fragmented, leaving several critical issues unresolved. This study identifies and discusses two prominent issues of traditional approaches that can undermine the reliability and validity of CJ's trait estimates. First, it critiques the heavy reliance on Thurstone's Case V assumptions (Thurstone, 1927a) and, by extension, the Bradley-Terry-Luce (BTL) model (Bradley and Terry, 1952; Luce, 1959) when analyzing CJ data. Specifically, the study raises concerns about the assumptions of equal discriminal dispersions and zero correlation between the stimuli. While

*Corresponding author
Email addresses: JoseManuel.RiveraEspejo@uantwerpen.be (Jose Manuel Rivera Espejo), tine.vandaal@uantwerpen.be (Tine van Daal), sven.demaeyer@uantwerpen.be (Sven De Maeyer), steven.gillis@uantwerpen.be (Steven Gillis)

these assumptions simplify the trait measurement model, they may fail to capture the complexity of CJ data, potentially leading to unreliable and inaccurate trait estimates. Second, the study highlights the disconnect between CJ's trait measurement and hypothesis testing processes. Although separating these processes simplifies the analysis of CJ data, it may also undermine the reliability of various statistical inferences derived from these processes.

To address these issues, this study extends Thurstone's general form through a systematic and integrated approach that combines Causal and Bayesian inference methods. In addition to potentially enhancing measurement reliability and validity, and improving statistical accuracy in hypothesis testing, this approach offers two key advantages. First, it clarifies the interactions among all actors and processes involved in CJ experiments. Second, it shifts the current comparative data analysis paradigm from passively accepting the BTL model assumptions to actively testing whether those assumptions fit the data under analysis.

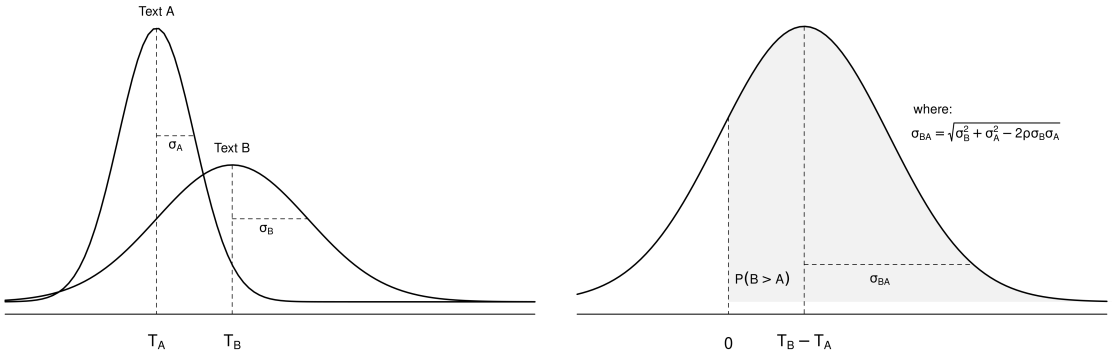
As a result, the study divides its content into six main sections. Section 2 provides an overview of Thurstone's theory. Section 3 discusses the identified issues in detail. Section 4 extends Thurstone's general form to address these challenges. The extension integrates core theoretical principles alongside key CJ experimental design features, such as the selection of judges, stimuli, and comparisons. Section 5 translates these theoretical and practical elements into a probabilistic statistical model to analyze dichotomous pairwise comparison data. Finally, Section 6 discusses the findings, explores avenues for future research, and detail the challenges for future researchers.

2. Thurstone's theory

In its most general form, Thurstone's theory addresses pairwise comparisons wherein a single judge evaluates multiple stimuli (Thurstone, 1927a). The theory posits that two key factors determine the dichotomous outcome of these comparisons: the discriminal process of each stimulus and their discriminal difference. The *discriminal 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 discriminal process for any given stimulus forms a Normal distribution along the trait continuum (Thurstone, 1927a). The mode (mean) of this distribution, known as the *modal discriminal process*, indicates the stimulus position on this continuum, while its dispersion, referred to as the *discriminal dispersion*, reflects variability in the perceived trait of the stimulus.

Figure 1a illustrates the hypothetical discriminal processes along a quality trait continuum for

two written texts. The figure indicates that the modal discriminal 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 discriminal dispersion ($\sigma_B > \sigma_A$).



(a) Discriminal processes

(b) Discriminal difference

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

However, since the individual discriminal 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 discriminal process positioned further along the trait continuum as possessing more of the trait (Bramley, 2008). This suggests that pairwise comparison outcomes depend on the relative distance between stimuli, not their absolute positions on the continuum. Indeed, the theory assumes that the difference between the underlying discriminal processes of the stimuli, referred to as the *discriminal difference*, determines the observed dichotomous outcome. Furthermore, the theory assumes that because the individual discriminal processes form a Normal distribution on the continuum, the discriminal difference will also conform to a Normal distribution (Andrich, 1978). In this distribution, the mode (mean) represents the average relative separation between the stimuli, and its dispersion indicates the variability of that separation.

Figure 1b illustrates the distribution of the discriminal difference for the two hypothetical texts. The figure indicates that the judge perceives Text B as having significantly higher quality than Text A. Two key observations support this conclusion: the positive difference between their modal discriminal processes ($T_B - T_A > 0$) and the probability area where the discriminal 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 A.

Table 1: Thurstones cases and their asumptions

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

3. Two Prominent Issues in Traditional CJ Practice

Thurstone noted from the outset that his general form, described in Section 2, led to a *trait scaling problem*. Specifically, the model required estimating more "unknown" parameters than the number of available pairwise comparisons (Thurstone, 1927a). For instance, in a CJ experiment with five texts, the general form would require estimating 20 parameters: five modal discriminal processes, five discriminal dispersions, and 10 correlations –one per comparison (see Table 1). However, a single judge could only provide $\binom{5}{2} = 10$ unique comparisons, an insufficient data set to estimate the required parameters.

To address this issue and facilitate the practical implementation of the theory, Thurstone developed five cases derived from this general form, each progressively incorporating additional simplifying assumptions (Thurstone, 1927a). 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 discriminal 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).

However, Thurstone developed Case V prioritizing statistical simplicity over precise trait measurement and offering no guidance on how to use its trait estimates for statistical inference or hypothesis testing. Specifically, Thurstone cautioned that its use "should not be made without (an) experimental test" (Thurstone, 1927a, pp. 270), as it imposes the most extensive set of simplifying assumptions (Bramley, 2008; Kelly et al., 2022) (see Table 1). Moreover, because Thurstone's primary goal was to produce a "rather coarse scaling" of traits and "allocate the compared stimuli on

this continuum" (Thurstone, 1927a, pp. 269), his theory did not support formal statistical inference. Despite these limitations, it is surprising that CJ research has predominantly relied on Case V to measure different traits, which raises significant concerns about the reliability and validity of such measurements in contexts where the case's assumptions may not hold (Kelly et al., 2022; Andrich, 1978). Furthermore, although the CJ tradition has attempted to address the gap of hypothesis testing by using the point estimates of the traits–or their transformations–a critical question remains: Does this approach provide a valid foundation for statistical inference?

Thus, this section discusses these two prominent issues. Specifically, Section 3.1 examines the heavy reliance on Thurstone's Case V assumptions in the statistical analysis of CJ data. Conversely, Section 3.2 focuses on the apparent disconnect between the approaches to trait measurement and hypothesis testing in CJ.

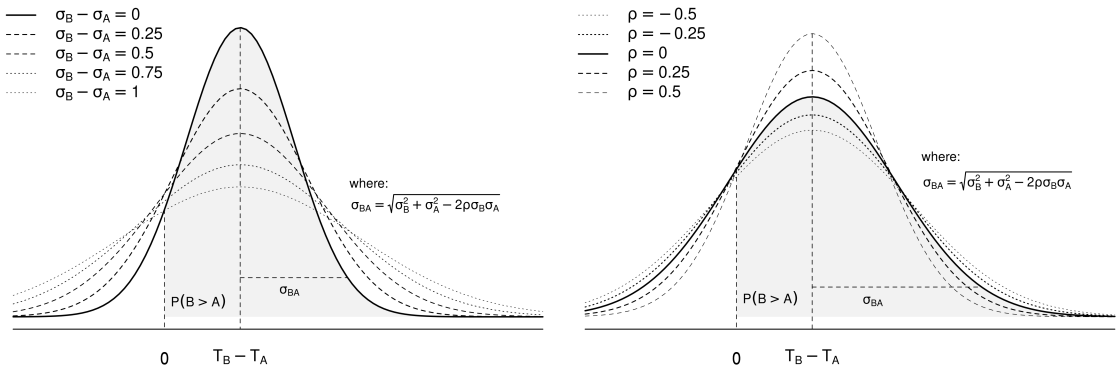
3.1. The Case V and the statistical analysis of CJ data

As previously discussed, Case V remains the most widely used model in CJ literature. This preference largely stems from the widespread adoption of the BTL model, which provides a simplified statistical representation of the case. The BTL model mirrors most of Case V's assumptions, with one notable distinction. While Case V assumes a Normal distribution for the stimuli' discriminal processes, the BTL model uses the more mathematically tractable Logistic distribution (Andrich, 1978; Bramley, 2008) (see Table 1). However, this substitution has minimal impact on trait estimation or model interpretation because the scale of the discriminal process (i.e., the latent trait) is arbitrary up to a non-monotonic transformation (van der Linden, 2017a; McElreath, 2021). That is, as long as the substitution (transformation) preserves the data rank order, the choice of distribution for the discriminal processes is inconsequential. This condition is satisfied in this case, as the Normal and Logistic distributions exhibit analogous statistical properties, differing only by a scaling factor of approximately 1.7 (van der Linden, 2017a).

However, Thurstone acknowledged that some assumptions of Case V could be problematic when researchers assess complex traits or heterogeneous stimuli (Thurstone, 1927b). Thus, given that modern CJ applications often involve such traits and stimuli, two key assumptions of Case V, and by extension, the BTL model, may not always hold in theory or practice. These assumptions are the equal dispersion and zero correlation between stimuli.

3.1.1. The assumption of equal dispersions between stimuli

According to the theory, discrepancies in the discriminal dispersions of stimuli shape the distribution of the discriminal difference, directly influencing the outcome of pairwise comparisons. A thought experiment can help illustrate this idea. In it, researchers observe the discriminal processes for two texts, A and B, assuming that the dispersion for Text A remains constant and that the two texts are uncorrelated ($\rho = 0$). Figure 2a demonstrates that an increase in the uncertainty associated with the perception of Text B relative to Text A ($\sigma_B - \sigma_A$), broadens the distribution of their discriminal difference. This broadening affects the probability area where the discriminal difference distinctly favors Text B over Text A, expressed as $P(B > A)$, ultimately influencing the comparison outcome. Additionally, the figure reveals that when the discriminal dispersions of the texts are equal, as in the BTL model ($\sigma_B - \sigma_A = 0$), the discriminal difference distribution is more narrow than when the dispersions differ. As a result, the discriminal difference is more likely to favor Text B over Text A, as it is represented by the shaded gray area.



(a) Discriminal Difference distribution under varying discrepancies in stimuli dispersions

(b) Discriminal Difference distribution under varying levels of correlation between stimuli

Figure 2: The effect of dispersion discrepancies and stimuli correlation on the distribution of the discriminal difference.

In experimental practice, however, the thought experiment occurs in reverse. Researchers first observe the comparison outcome and then use the BTL model to infer the discriminal difference between stimuli and their respective discriminal processes (Thurstone, 1927b). Consequently, the outcome's ability to reflect *true* differences between stimuli largely depends on the validity of the model's assumptions (Kohler et al., 2019), in this case, the assumption of equal dispersions. For instance, when the assumption accurately captures the complexity of the data, the BTL model estimates a discriminal difference distribution that accurately represents the *true* discriminal difference between the texts. This scenario is illustrated in Figure 2a, when the model's discriminal difference distribution aligns with the *true* discriminal difference distribution, represented by the

thick continuous line corresponding to $\sigma_B - \sigma_A = 0$. The accuracy of this discriminal difference then ensures reliable estimates for the texts' discriminal processes.

Notably, while assuming equal dispersions simplifies the trait measurement model, evidence from the CJ literature suggests that this assumption may fail to capture the complexity of some traits or account for heterogeneous stimuli, such as handwritten texts or English compositions (Thurstone, 1927b; Andrich, 1978; Bramley, 2008; Kelly et al., 2022). Indeed, the presence of the so-called *misfit* texts may already signal these limitations, as these are texts that elicit more judgment discrepancies than others (Pollitt, 2004, 2012b,a; Goossens and De Maeyer, 2018), possibly due to larger discriminal dispersions or because they are genuine outliers–meaning, texts with distinctive characteristics that deviate markedly from the rest of the sample (Grubbs, 1969). In either case, the BTL model neither accounts for these anomalies nor provides tools for addressing them, apart from excluding the "problematic" texts from analysis.

Significant statistical and measurement issues can arise when the assumption of equal dispersions between stimuli does not hold. Specifically, the BTL model may overestimate the trait's reliability, that is, the degree to which the outcome accurately reflects the *true* discriminal differences between stimuli. This overestimation, in turn, results in spurious conclusions about these differences (McElreath, 2020; Wu et al., 2022) and, by extension, about the underlying discriminal processes of stimuli. Figure 2a also illustrates this scenario when the model's discriminal difference distribution aligns with the thick continuous line for $\sigma_B - \sigma_A = 0$, while the *true* discriminal difference follows any discontinuous line where $\sigma_B - \sigma_A \neq 0$. Furthermore, if researchers acknowledge that misfit statistics may represent texts with different dispersions or outlying observations, the common CJ practice of excluding stimuli based on these statistics, as seen in Pollitt (2012a), Pollitt (2012b), van Daal et al. (2016), and Goossens and De Maeyer (2018), may unintentionally discard valuable information, introducing bias into the trait estimates (Zimmerman, 1994; McElreath, 2020). The direction and magnitude of these biases remain unpredictable, as they depend on which stimuli researchers exclude from the analysis.

3.1.2. The assumption of zero correlation between stimuli

The correlation ρ, illustrated in Figure 1b, measures how much the judges' perception of a specific trait in one stimulus depends on their perception of the same trait in another. Similar to the discriminal dispersions, this correlation shapes the distribution of the discriminal difference, directly impacting the outcomes of pairwise comparisons. A thought experiment, akin to the one presented in

Section 3.1.1, can illustrate this idea. Assuming that the discriminal dispersions for both texts remain constant, Figure 2b shows that as the correlation between the two texts increases, the distribution of their discriminal difference becomes narrower. This narrowing, in turn, affects the probability that the discriminal difference distinctly favors Text B over Text A–denoted as $P(B > A)$–and thus directly influences the comparison outcome. Furthermore, the figure shows that when two texts are independent or uncorrelated, as assumed in the BTL model ($\rho = 0$), the distribution of their discriminal difference is less narrow than in scenarios where the texts are positively correlated. As a result, it becomes less likely for the comparison to favor Text B over Text A, as indicated by the larger shaded area.

Despite these notable differences in the distribution of the discriminal difference under various correlational assumptions, in practice, experimental designs often adopt the assumption of no correlation between stimuli based on an old theoretical justification. Specifically, Thurstone (1927a) argued that stimuli could be treated as uncorrelated because judges' biases–arising from two opposing and equally weighted effects occurring during the pairwise comparisons–would cancel each other out. This idea was later formalized by Andrich (1978), who provided a mathematical demonstration of this cancellation using the BTL model under the assumption of discriminal processes with additive biases. However, evidence from the CJ literature indicates that the assumption of zero correlation does not hold in practice in at least two scenarios: when intricate aspects of multidimensional, complex traits or heterogeneous stimuli influence judges' perceptions or when additional hierarchical structures are relevant to the stimuli.

In the first scenario, research on text quality suggests that when judges evaluate multidimensional, complex traits or heterogeneous stimuli, they often rely on a variety of intricate stimulus characteristics to inform their judgments (van Daal et al., 2016; Lesterhuis, 2018b; Chambers and Cunningham, 2022). These characteristics, regardless of their relevance, are unlikely to be equally weighted or to oppose one another across comparisons. As a result, they may exert a disproportionate influence on judges' perceptions, generating biases that persist rather than cancel out. For example, this could occur when a judge assessing the argumentative quality of a text may place disproportionate emphasis on handwriting clarity, thereby favoring neatly written texts despite their weaker arguments. Moreover, because the discriminal process of stimuli becomes an observable outcome only through the judges' perceptions, these biases could introduce dependencies between the stimuli (van der Linden, 2017b). Evidence from traditional marking contexts confirms a similar phenomenon (Morin et al., 2018), and additional studies (Pollitt and Elliott, 2003; van Daal et al., 2016; Bartholomew

et al., 2020) support the presence of these biases in CJ–reinforcing the argument that the factors influencing pairwise comparisons do not always cancel each other out.

In the second scenario, the shared context or inherent connections introduced by additional hierarchical structures may create dependencies between stimuli–a statistical phenomenon known as clustering (Everitt and Skrondal, 2010). For instance, when the same individual produces multiple texts, those texts often exhibit common features, such as writing style or overall quality, that judges can easily recognize. In this regard, although the CJ literature acknowledges the existence of such hierarchical structures, the statistical approaches to account for this additional source of dependence have been insufficient. For instance, when CJ data incorporates multiple samples of stimuli from the same individuals, researchers frequently rely on (averaged) point estimates of the BTL scores to conduct subsequent analyses and tests at the individual level (Bramley and Vitello, 2019; Boonen et al., 2020; Bouwer et al., 2023; van Daal et al., 2017; Jones et al., 2019; Gijsen et al., 2021). However, this approach can introduce additional statistical and measurement issues, similar to the ones discussed in Section 3.2.

Thus, erroneously assuming zero correlation between stimuli can also lead to significant statistical and measurement issues. Specifically, neglecting judges' biases or relevant hierarchical structures can create dimensional mismatches in the model, leading to the over- or underestimation of trait reliability (Ackerman, 1989; Hoyle, 2023) and even biases (Wu et al., 2022). These inaccuracies can result in spurious conclusions about the discriminal differences (McElreath, 2020) and, by extension, the underlying discriminal processes of the stimuli. This issue is illustrated in Figure 2b when the discriminal difference distribution of the BTL scores follows the thick continuous line ($\rho = 0$), while the *true* discriminal difference follows any discontinuous line where $\rho \neq 0$.

Finally, as discussed in the previous section, removing *misfit* judges–that is, judges whose assessments deviate markedly from the shared consensus (Pollitt, 2012a,b; van Daal et al., 2016; Goossens and De Maeyer, 2018), and considered outliers under the BTL model (Grubbs, 1969; Wu et al., 2022)–risk discarding valuable information and introducing bias into trait estimates. The direction and magnitude of these biases remain unpredictable, as they depend on which judges researchers exclude from the analysis (Zimmerman, 1994; O'Hagan, 2018; McElreath, 2020).

3.2. The disconnect between trait measurement and hypothesis testing

Building on the previous section, it is clear that, researchers in CJ studies typically use the BTL model to measure traits and position the compared stimuli along a latent continuum (Thurstone,

1927a). The CJ literature also show that they frequently rely on point estimates of these traits–typically the BTL scores or its transformations–to conduct statistical inference or hypothesis testing. For example, researchers have used these scores 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 (Casalicchio et al., 2015; Bramley and Vitello, 2019; Boonen et al., 2020; Bouwer et al., 2023; van Daal et al., 2017; Jones et al., 2019; Gijsen et al., 2021).

Nevertheless, while separating the trait measurement and hypothesis testing processes simplifies the analysis of CJ data, the statistical literature cautions against relying solely on the point estimates of BTL scores to conduct statistical inference or hypothesis tests, as this practice can undermine the resulting statistical conclusions. A key consideration is that BTL scores are parameter estimates that inherently carry uncertainty (measurement error). Ignoring this uncertainty can bias the analysis and reduce the precision of hypothesis tests. 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 (McElreath, 2020; Kline, 2023; Hoyle, 2023). Furthermore, 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, the heavy reliance on Thurstone's Case V assumptions in the statistical analysis of comparative data can compromise the reliability of trait estimates. This overreliance may also undermine their validity (Perron and Gillespie, 2015), particularly when coupled with the disconnect between the trait measurement and hypothesis testing processes. However, the structural approach to causal inference can address these issues by offering a systematic and integrated framework that strengthens measurement reliability and validity while enhancing the statistical accuracy of hypothesis tests.

4. Extending Thurstone's general form

The *structural approach* to causal inference provides a formal framework for identifying causes and estimating their effects using data. The approach 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 a system, such as the one found in CJ experiments. Essentially, SCMs and DAGs function as *conceptual models* on which identification

analysis rests. *Identification analysis* determines whether an estimator can accurately compute an estimand based solely on its (causal) assumptions, regardless of random variability (Schuessler and Selb, 2023). Here, *estimands* represent the specific quantities researchers aim to determine (Everitt and Skrondal, 2010). *Estimators* denote the methods or functions that transform data into an estimate, while *estimates* are the numerical values approximating the estimand (Neal, 2020; Everitt and Skrondal, 2010).

A motivating example that will appear in the rest of the document clarifies these concepts. In this example, researchers aim to determine: "To what extent do different teaching methods influence students' ability to produce high-quality written texts?" To investigate this, a researcher designs a CJ experiment by randomly assigning students (individuals) to two groups, each receiving a different teaching method. Judges then compare pairs of students' written texts (stimuli) to produce a dichotomous outcome reflecting the relative quality of each text (trait). Based on this setup, researchers can reformulate the research question as the estimand: "*On average*, is there a difference in the ability to produce high-quality written texts between the two groups of students?". Following current CJ practices, researchers rely on estimates from the BTL model, or its transformations, to approximate this estimand.

However, Section 3 presents compelling evidence that Thurstone's Case V, and by extension the BTL model, suffers from several statistical and measurement limitations. These limitations hinder the model's ability to identify various estimands relevant to CJ inquiries, including the one described in the example. Identification is crucial because it is a necessary condition for ensuring consistent estimators. *Consistency* refers to the property of an estimator whose estimates converge to the "true" value of the estimand as the data size approaches infinity (Everitt and Skrondal, 2010). Without identification, consistency cannot be achieved, even with "infinite" and error-free data. Thus, deriving meaningful insights from finite data becomes impossible (Schuessler and Selb, 2023).

Fortunately, SCMs and DAGs support identification analysis through two key advantages[1]. First, regardless of complexity, they can represent various causal structures using only five fundamental building blocks (Neal, 2020; McElreath, 2024). This feature allows researchers to decompose complex

[1]These topics are beyond the scope of this study, thus, readers seeking a more profound understanding can refer to introductory papers such as Pearl (2010), Rohrer (2018), Pearl (2019), and Cinelli et al. (2020), and introductory books like Pearl and Mackenzie (2018), Neal (2020), and McElreath (2020) are useful. For more advanced study, seminal papers such as Neyman (1923), Rubin (1974), Spirtes et al. (1991), and Sekhon (2009), along with books such as Pearl (2009), Morgan and Winship (2014), and Hernán and Robins (2020), are recommended.

structures into manageable components, facilitating their analysis. Second, they depict causal relationships in a non-parametric way. This flexibility enables feasible identification strategies without requiring specification of the types of variables, the functional forms relating them, or the parameters of those functional forms (Pearl et al., 2016).

Thus, this section addresses the issues identified in Section 3 by extending Thurstone's general form using the structural approach to Causal inference. Specifically, it combines the core theoretical principles outlined in Section 2 with key assessment design features relevant to CJ experiments, such as the selection of judges, stimuli, and comparisons. In addition to improving statistical accuracy and strengthening measurement reliability and validity, the approach offers two key advantages. First, it clarifies the interactions among all actors and processes involved in CJ experiments. Second, it shifts the current comparative data analysis paradigm from passively accepting the model assumptions to actively testing whether those assumptions fit the data under analysis.

Accordingly, Section 4.1 incorporates the theoretical principles into what we refer to as the *conceptual-population model*. This model assumes an idealized scenario where researchers have access to a *conceptual population* of comparative data, that is, data representing all repeated judgments made by every available judge for each pair of stimuli produced by each pair of individuals in the population. Conversely, Section 4.2 integrates the assessment design features into what we refer to as the *sample-comparison model*. This model assumes a more realistic scenario where researchers only have access to a sample of judges, individuals, stimuli, and comparisons from the conceptual population.

4.1. The conceptual-population model

In the conceptual-population model, the idealized scenario of a *conceptual population* of comparative data enables the integration of Thurstone's theoretical principles and provides a foundation for proposing innovations aimed at addressing some of the issues discussed in Section 3.

4.1.1. Integrating the first theoretical principles

Before incorporating the first theoretical principles of Thurstone's theory, it is essential to further define SCMs. SCMs are formal mathematical models characterized by a set of *endogenous* variables V, a set of *exogenous* variables E, and a set of functions F (Pearl, 2009; Pearl et al., 2016; 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). Lastly, the functions,

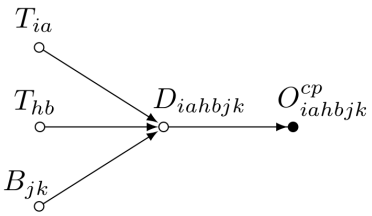
referred to as *structural equations*, express the endogenous variables as non-parametric functions of other endogenous and exogenous variables. These functions use the symbol ':=' to denote the asymmetrical causal dependence between variables and the symbol ' $\perp$ ' to represent *d-separation*, a concept akin to statistical (conditional) independence.

SCM 3a presents the first theoretical principles embedded in the conceptual-population model, which evaluates the impact of different teaching methods on students' writing ability. This SCM outlines the relationship between the conceptual-population outcome (O^{cp}_{iahbjk}) and several related variables. The subscripts i and h identify the students who authored the texts (i.e., the individuals). The indices a and b represent the texts under comparison (i.e., the stimuli). The index j indicates the judge conducting the comparison, while the index k accounts for experimental conditions where a judge compares the same pair of stimuli multiple times, i.e., a *repeated measures designs* (Lawson, 2015, pp. 366-376). Thus, the indexing system supports comparisons between different texts written by the same student ($i = h$; $a \neq b$) and between texts written by distinct students ($i \neq h$; where $a = b$ is permitted), each compared once or repeatedly by all judges ($j = 1, \ldots, n_J$; $k = 1, \ldots, n_K$; where $n_J > 1$ and $n_K \geq 1$). However, it excludes cases where a judge compares a student's text to itself, whether once or multiple times ($i = h$; $a = b$; $j = 1, \ldots, n_J$; $k = 1, \ldots, n_K$; where $n_J > 1$ and $n_K \geq 1$), as such comparison lacks practical relevance within the CJ framework. Here, n_J indicates the total number of judges, and n_K denotes the number of repeated judgments each judge performs.

In line with Thurstone's theory, SCM 3a depicts the texts' discriminal processes (T_{ia}, T_{hb}) and their discriminal difference (D_{iahbjk}) (see Section 2). Additionally, the SCM incorporates a key design feature of CJ experiments: the judges' biases (B_{kj}). This extension builds on the arguments presented in Section 3.1.2, contending that the discriminal difference becomes an observable outcome only through judges' perceptions. Given that such perceptions may be imperfect–and that each judge may carry some degree of bias (see Pollitt and Elliott, 2003; van Daal et al., 2016)–it is reasonable that judges' perceptions (bias) should be treated as an integral component of the CJ system from the outset, as this leads to a more accurate representation of the data-generating process underlying the pairwise comparisons. This model defines the preliminary set of endogenous variables, $V = \{O_{iahbjk}, D_{iahbjk}, T_{ia}, T_{hb}, B_{kj}\}$, and the preliminary set of structural equations, $F = f_O, f_D$, which capture the non-parametric dependencies among these variables.

$$O^{cp}_{iahbjk} := f_O(D_{iahbjk})$$
$$D_{iahbjk} := f_D(T_{ia}, T_{hb}, B_{jk})$$

(a) SCM



(b) DAG

Figure 3: Conceptual-population model, scalar form.

Notably, every SCM has an associated DAG (Pearl et al., 2016; Cinelli et al., 2020). A DAG is a *graph* consisting of nodes connected by edges, where nodes represent random variables. The term *directed* indicates that edges or arrows extend from one node to another, indicating the direction of causal influence. The absence of an edge implies no direct relationship between the nodes. The term *acyclic* means that the causal influences do not form loops, ensuring the influences do not cycle back on themselves (McElreath, 2020). DAGs conventionally depict observed variables as solid black circles and unobserved (latent) variables as open circles (Morgan and Winship, 2014). Although DAGs conventionally omit exogenous variables for simplicity, the DAGs presented in this section includes exogenous variables to improve clarity and reveal potential issues related to conditioning and confounding (Cinelli et al., 2020).

Figure 3b displays the DAG corresponding to SCM 3a, illustrating the expected causal relationships outlined in Thurstone's theory. The graph shows that the discriminal processes of the texts (T_{ia}, T_{hb}) influence their discriminal difference (D_{iahbjk}), which in turn determines the outcome (O^{cp}_{iahbjk}). It also highlights the influence of judges' biases (B_{kj}) on the discriminal difference. Additionally, the DAG differentiates between observed endogenous variables, such as the outcome (solid black circle), and latent endogenous variables, including the texts' discriminal processes, their discriminal difference, and the judges' biases (open circles).

4.1.2. The conceptual-population *data structure*

Although specifying a data structure is not mandatory when using SCMs and DAGs, defining one in this case can improve clarity and facilitate the description of the system. Thus, to re-express the

scalar form of the CJ system shown in Figure 3 into an equivalent vectorized form, we first define the vectors I and J, along with the matrices IA and JK, as follows:

$$I = \begin{bmatrix} 1 \\ \vdots \\ i \\ \vdots \\ h \\ \vdots \\ n_I \end{bmatrix}; \; J = \begin{bmatrix} 1 \\ \vdots \\ j \\ \vdots \\ n_J \end{bmatrix}; \; IA = \begin{bmatrix} 1 & 1 \\ \vdots & \vdots \\ 1 & n_A \\ \vdots & \vdots \\ i & a \\ \vdots & \vdots \\ h & b \\ \vdots & \vdots \\ n_I & 1 \\ \vdots & \vdots \\ n_I & n_A \end{bmatrix}; \; JK = \begin{bmatrix} 1 & 1 \\ \vdots & \vdots \\ 1 & n_K \\ \vdots & \vdots \\ j & k \\ \vdots & \vdots \\ n_J & 1 \\ \vdots & \vdots \\ n_J & n_K \end{bmatrix} \tag{1}$$

Here, each element of I represents a unique individual i or h, where n_I denotes the total number of individuals. Similarly, each element of J corresponds to a unique judge j, with n_J indicating the total number of judges. Moreover, each row of IA represents a unique pairing of individuals i, h with stimuli a, b. As a result, the matrix IA contains $n_I \cdot n_A$ rows and 2 columns, where n_A specifies the number of stimuli available per individual. Likewise, each row of JK associates a judge j with a (repeated) judgment index k. Consequently, the matrix JK has $n_J \cdot n_K$ rows and 2 columns, where n_K indicates the number of repeated judgments each judge makes.

Additionally, we construct the matrix R to map each row of the IA matrix with a corresponding row from the JK matrix. This matrix has n rows and 6 columns, where $n = \binom{n_I \cdot n_A}{2} \cdot n_J \cdot n_K$. Here, the term $\binom{n_I \cdot n_A}{2}$ represents the binomial coefficient, which quantifies the total number of unique comparisons possible between every pair of stimuli generated by each pair of individuals in the population. Thus, we define the matrix as follows:

$$R = \begin{bmatrix} 1 & 1 & 1 & 2 & 1 & 1 \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\ 1 & 1 & 1 & 2 & 1 & n_K \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\ i & a & h & b & j & k \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\ n_I & n_A - 1 & n_I & n_A & n_J & 1 \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\ n_I & n_A - 1 & n_I & n_A & n_J & n_K \end{bmatrix} \tag{2}$$

It is easier to visualize the structure of these vectors and matrices by considering an example. Assuming $n_I = 5$, $n_A = 2$, $n_J = 3$, and $n_K = 3$, the vectors and matrices described in Equations (1) and (2) take the following form:

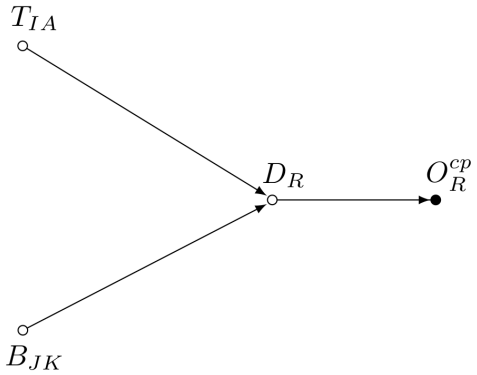
$$I = \begin{bmatrix} 1 \\ 2 \\ 3 \\ 4 \\ 5 \end{bmatrix} ; \; J = \begin{bmatrix} 1 \\ 2 \\ 3 \end{bmatrix} ; \; IA = \begin{bmatrix} 1 & 1 \\ 1 & 2 \\ 2 & 1 \\ 2 & 2 \\ 3 & 1 \\ 3 & 2 \\ 4 & 1 \\ 4 & 2 \\ 5 & 1 \\ 5 & 2 \end{bmatrix} ; \; JK = \begin{bmatrix} 1 & 1 \\ 1 & 2 \\ 1 & 3 \\ 2 & 1 \\ 2 & 2 \\ 2 & 3 \\ 3 & 1 \\ 3 & 2 \\ 3 & 3 \end{bmatrix} ; \; R = \begin{bmatrix} 1 & 1 & 1 & 2 & 1 & 1 \\ 1 & 1 & 1 & 2 & 1 & 2 \\ 1 & 1 & 1 & 2 & 1 & 3 \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\ 1 & 1 & 5 & 2 & 1 & 1 \\ 1 & 1 & 5 & 2 & 1 & 2 \\ 1 & 1 & 5 & 2 & 1 & 3 \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\ 4 & 2 & 5 & 2 & 3 & 1 \\ 4 & 2 & 5 & 2 & 3 & 2 \\ 4 & 2 & 5 & 2 & 3 & 3 \\ 5 & 1 & 5 & 2 & 3 & 1 \\ 5 & 1 & 5 & 2 & 3 & 2 \\ 5 & 1 & 5 & 2 & 3 & 3 \end{bmatrix} \tag{3}$$

Now, using Equations (1) and (2), we can re-express SCM 3a and DAG 3b in an equivalent vectorized form, as shown in Figure 4. In this depiction, the outcome O_R^{cp}, the texts' discriminal difference D_R, their discriminal processes T_{IA}, and the judges' biases B_{JK} are represented as vectors rather than scalar values. These vectors capture all the observations from the conceptual population. Specifically, O_R^{cp} and D_R are observed and latent vectors of length n, respectively. Moreover, T_{IA}

and B_{JK} are latent vectors of lengths $n_I \cdot n_A$ and $n_J \cdot n_K$, respectively.

$$O_R^{cp} := f_O(D_R)$$
$$D_R := f_D(T_{IA}, B_{JK})$$

(a) SCM



(b) DAG

Figure 4: Conceptual-population model, initial vectorized form.

4.1.3. Integrating hierarchical structural components

Building on the principles of Structural Equation Modeling (SEM) (Hoyle, 2023) and Item Response Theory (IRT) (Fox, 2010; van der Linden, 2017a), the conceptual-population model integrates two *hierarchical structural components* to examine how different teaching methods influence students' writing ability. Each structural component defines how observed or latent variables affect the primary latent variable of interest (Everitt and Skrondal, 2010). The model's hierarchical design allows researchers to formulate and test hypotheses that account for both the nested structure of stimuli and the uncertainties inherent in trait estimation (see Section 3.1.2 and Section 3.2 for a discussion of these considerations).

The top branch of DAG 5b illustrates the first component, where *relevant* [2] student-related variables X_I, such as teaching method, and students' idiosyncratic errors e_I causally influence the latent variable representing students' writing-quality trait T_I. The error term e_I captures

[2] *Relevant variables* are those that satisfy the *backdoor criterion* (Neal, 2020, pp 37), that is, they belong to a *sufficient adjustment set* (Pearl, 2009; Pearl et al., 2016; Morgan and Winship, 2014). A *sufficient* set (potentially empty) blocks all non-causal paths between a predictor and an outcome without opening new ones (Pearl, 2009). Refer also to footnote 1.

variations in students' traits unexplained by X_I. Here, X_I is an observed matrix with n_I rows and q_I independent columns (variables), and both e_I and T_I are latent vectors of length n_I. Additionally, this branch shows how T_I, along with *relevant* [3] text-related variables X_{IA} (e.g., text length), and texts' idiosyncratic errors e_{IA} causally influence the texts' written-quality trait T_{IA}, the first primary latent variable of interest. The error term e_{IA} captures variations in the texts' traits that remain unexplained by T_I or X_{IA}. Here, X_{IA} is an observed matrix with dimensions $n_I \cdot n_A$ rows and q_{IA} independent columns (variables), while e_{IA} and T_{IA} are latent matrices with n_I rows and n_A columns.

Similarly, the bottom branch of DAG 5b depicts the second component, where *relevant* [4] judge-related variables Z_J, such as judgment expertise, and judges' idiosyncratic errors e_J causally influence the latent variable representing judges' bias B_J. The error e_J captures variations in judges' bias unexplained by Z_J. Here, Z_J is an observed matrix with n_J rows and q_J independent columns (variables), and both e_J and B_J are latent vectors of length n_J. Furthermore, the branch shows how B_J, along with *relevant* [5] judgment-related variables Z_{JK} (e.g., the number of judgments a judge makes), and judgments' idiosyncratic errors e_{JK} causally influence the judges' biases associated with each text B_{JK}, the second primary latent variable of interest. The error e_{JK} captures variations in judgments unexplained by B_J or Z_{JK}. Here, Z_{JK} is an observed matrix with dimension $n_J \cdot n_K$ rows and q_{JK} independent columns (variables), while e_{JK} and B_{JK} are latent latent matrices with n_J rows and n_K columns

Notably, all variables and functions shown in SCM 5a and DAG 5b are part of the set of endogenous variables V, structural equations F, and exogenous variables E for the conceptual-population model. Additionally, the figures demonstrate that all exogenous variables are independent of one another, as indicated by the relationships $e_{IA} \perp \{e_I, e_{JK}, e_J\}$, $e_I \perp \{e_{JK}, e_J\}$ and $e_{JK} \perp e_J$ and the absence of connecting arrows.

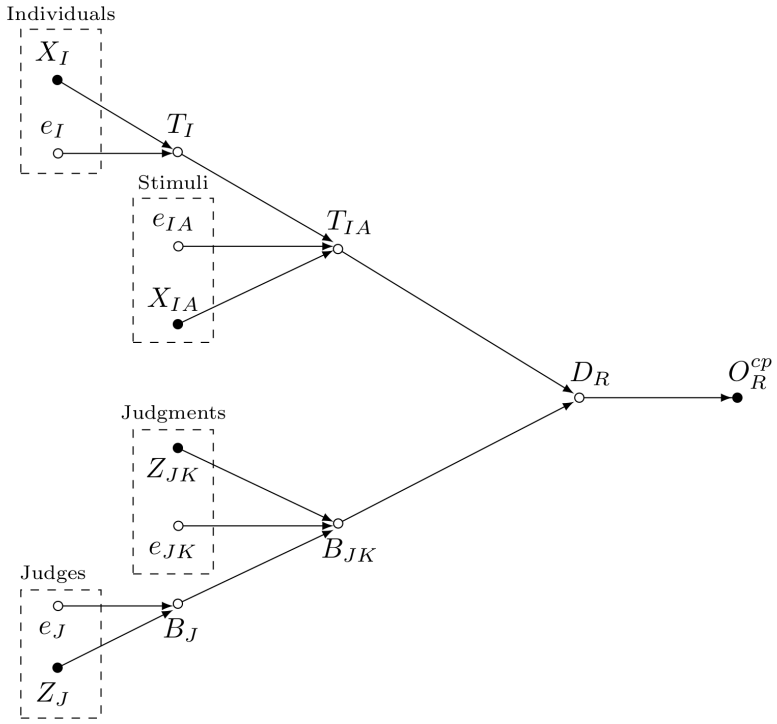
Overall, the conceptual-population model extends Thurstone's general form by introducing key innovations to address the limitations discussed in Section 3.1.2 and Section 3.2. These enhancements include accounting for judges' biases and integrating hierarchical structural components. Nevertheless, despite its promise of enhancing measurement accuracy and precision, the model still depends on the unrealistic assumption that researchers have access to data from the *conceptual population*. Since

[3] refer to footnote 2.
[4] refer to footnote 2.
[5] refer to footnote 2.

researchers rarely meet this assumption in practice, they must consider a more realistic scenario.

$$
\begin{aligned}
O_R^{cp} &:= f_O(D_R) \\
D_R &:= f_D(T_{IA}, B_{JK}) \\
T_{IA} &:= f_T(T_I, X_{IA}, e_{IA}) \\
T_I &:= f_T(X_I, e_I) \\
B_{JK} &:= f_B(B_J, Z_{JK}, e_{JK}) \\
B_J &:= f_B(Z_J, e_J) \\
e_I &\perp \{e_J, e_{IA}, e_{JK}\} \\
e_J &\perp \{e_{IA}, e_{JK}\} \\
e_{IA} &\perp e_{JK}
\end{aligned}
$$

(a) SCM



(b) DAG

Figure 5: Conceptual-population model, final vectorized form.

4.2. The sample-comparison model

The sample-comparison model presents a more realistic scenario than the conceptual-population model. First, in Section 4.2.1, it explicitly assumes researchers work with a data sample consisting of a limited number of repeated judgments (n_K^s) from a sample of judges (n_J^s) and a specific number of texts (n_A^s) from a sample of students (n_I^s), all drawn from the conceptual population. Second, in Section 4.2.2, the model assumes that judges do not perform *all repeated judgments* within the data sample. Instead, they conduct a sufficient number of stimuli comparisons, n_C, to ensure an accurate estimation of the proportion $P(B > A)$, as proposed by Thurstone (1927a).

4.2.1. The sample mechanism

To incorporate the sampling mechanism and facilitate the interpretation of the sample-comparison model, we first define the *data sampling process* using the binary vector variables S_I, S_J, S_{IA}, and S_{JK} as follows:

$$S_I = \begin{bmatrix} i_{(1)} \\ \vdots \\ i_{(i)} \\ \vdots \\ i_{(h)} \\ \vdots \\ i_{(nI)} \end{bmatrix} ; \; S_J = \begin{bmatrix} j_{(1)} \\ \vdots \\ j_{(j)} \\ \vdots \\ j_{(nJ)} \end{bmatrix} ; \; S_{IA} = \begin{bmatrix} ia_{(1,1)} \\ \vdots \\ ia_{(1,n_A)} \\ \vdots \\ ia_{(i,a)} \\ \vdots \\ ia_{(h,b)} \\ \vdots \\ ia_{(nI,1)} \\ \vdots \\ ia_{(nI,nA)} \end{bmatrix} ; \; S_{JK} = \begin{bmatrix} jk_{(1,1)} \\ \vdots \\ jk_{(1,n_K)} \\ \vdots \\ jk_{(j,k)} \\ \vdots \\ jk_{(nJ,1)} \\ \vdots \\ jk_{(nJ,nK)} \end{bmatrix} \tag{4}$$

Where each element of S_I is a binary value indicating the presence or absence of corresponding elements in the vector I, as in Equation (5). We apply the same logic to S_J using vector J (not shown). Thus, the vectors S_I and S_J contains n_I and n_J elements, respectively.

$$i_{(i)} = \begin{cases} 1 & \text{if data element } i \text{ from } I \text{ is sampled} \\ 0 & \text{if data element } i \text{ from } I \text{ is missing} \end{cases} \tag{5}$$

Similarly, each element of S_{IA} is a binary value indicating the presence or absence of data rows in the matrices IA, as defined in Equation (6). We apply the same logic to S_{JK} using the matrix JK

(not shown). Thus, the vectors S_{IA} and S_{JK} contains $n_I \cdot n_A$ and $n_J \cdot n_K$ elements, respectively.

$$ia_{(i,a)} = \begin{cases} 1 & \text{if data elements } i, a \text{ from } IA \text{ are sampled} \\ 0 & \text{if data elements } i, a \text{ from } IA \text{ are missing} \end{cases} \tag{6}$$

We can illustrate the structure of these vectors more clearly with an example. Suppose researchers exclude the second student, the second text from each student, and the third judge from the setup shown in Equation (3). Given $n_I = 5$, $n_A = 2$, $n_J = 3$, and $n_K = 3$, the resulting vectors would have the following structure:

$$S_I = \begin{bmatrix} 1 \\ 0 \\ 1 \\ 1 \\ 1 \end{bmatrix} ; \; S_J = \begin{bmatrix} 1 \\ 1 \\ 0 \end{bmatrix} ; \; S_{IA} = \begin{bmatrix} 1 \\ 0 \\ 0 \\ 0 \\ 1 \\ 0 \\ 1 \\ 0 \\ 1 \\ 0 \end{bmatrix} ; \; S_{JK} = \begin{bmatrix} 1 \\ 1 \\ 1 \\ 1 \\ 1 \\ 1 \\ 0 \\ 0 \\ 0 \end{bmatrix} \tag{7}$$

Notably, Equation (7) shows that missing observations in the vectors S_I and S_J–which represent unsampled students and judges–directly determine which observations are missing in S_{IA} and S_{JK}. In other words, researchers can only observe texts and judgments from students and judges initially included in the sample. The equation also shows that the sum of observed elements in S_I equals the number of sampled students (n_I^s) and that a similar sum in vector S_J equals the sampled judges (n_J^s). Conversely, the sum of observed elements in S_{IA} represents the total sampled texts across all sampled students ($n_I^s \cdot n_A^s$), while a similar sum in vector S_{JK} represents the total sampled repeated judgments across all sampled judges ($n_J^s \cdot n_K^s$). Notice that in this example, because the design systematically excludes every third repeated judgment, researchers can also express S_{JK} using $n_K = n_K^s = 2$.

Finally, we define the *sample mechanism* S in Equation (8), which maps each element of S_{IA} to every element of S_{JK}. Each element $s_{(i,a,h,b,j,k)}$ is a binary value indicating the presence or absence of data rows in the matrix R resulting from the sample mechanism, as in Equation (9). Thus,

the vector contains n elements, matching the number of rows in R, and the sum of its elements represents the total data sample: $n^s = \binom{n_I^s \cdot n_A^s}{2} \cdot n_J^s \cdot n_K^s$. Here, the term $\binom{n_I^s \cdot n_A^s}{2}$ represents the binomial coefficient, which quantifies the total number of unique comparisons possible between every pair of sampled stimuli generated by each pair of sampled individuals.

$$S = \begin{bmatrix} s_{(1,1,1,2,1,1)} \\ \vdots \\ s_{(1,1,1,2,1,n_K)} \\ \vdots \\ s_{(i,a,h,b,j,k)} \\ \vdots \\ s_{(n_I,n_A-1,n_I,n_A,n_J,1)} \\ \vdots \\ s_{(n_I,n_A-1,n_I,n_A,n_J,1)} \end{bmatrix} \tag{8}$$

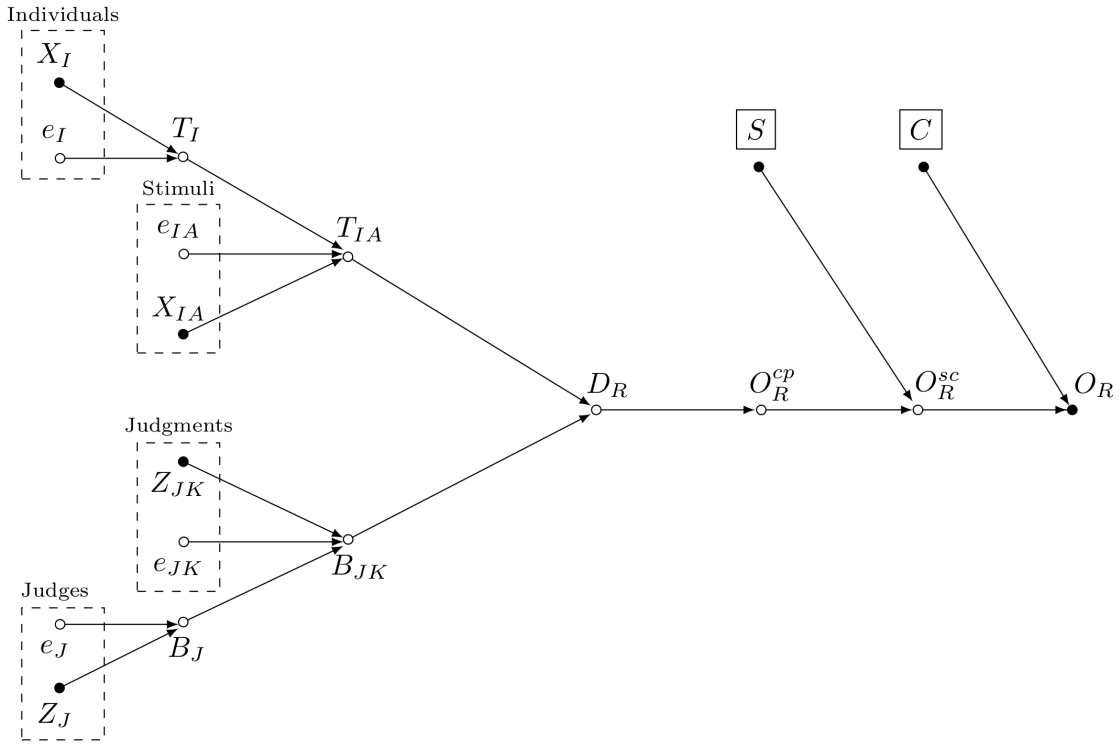
$$s_{(i,a,h,b,j,k)} = \begin{cases} 1 & \text{if data elements } i,a,h,b,j,k \text{ from } R \text{ are sampled} \\ 0 & \text{if data elements } h,i,a,b,j,k \text{ from } R \text{ are missing} \end{cases} \tag{9}$$

With the definition of S, we incorporate the sample mechanism into the conceptual-population model. Following the convention of McElreath (2020) and Deffner et al. (2022), DAG 6b represents the conceptual-population outcome O_R^{cp} as unobserved, emphasizing that researchers cannot directly access this outcome due to the sampling mechanism. The DAG also depicts the *sample design* vector S as a causal factor influencing the sample-comparison outcome O_R^{sc}. A square encloses S, indicating that it is a conditioned variable. In this context, *conditioning* means that researchers restrict their focus to the elements of O_R^{cp} that satisfy $s_{(i,a,h,b,j,k)} = 1$ (Neal, 2020; McElreath, 2020). In essence, S is a vector that selects *all repeated judgments made by a subset of judges for a subset of stimuli produced by the sampled individuals.*

Notably, the DAG shows that S is independent of all other variables in the model. This implies that DAG 6b applies exclusively to Simple Random Sampling (SRSg) designs. In these designs, each repeated judgment, judge, stimulus, and individual has the same probability of being included in the sample as any other observation within their respective groups (Lawson, 2015).

$$
\begin{aligned}
O_R &:= f_C(O_R^{sc}, C) \\
O_R^{sc} &:= f_S(O_R^{cp}, S) \\
O_R^{cp} &:= f_O(D_R) \\
D_R &:= f_D(T_{IA}, B_{JK}) \\
T_{IA} &:= f_T(T_I, X_{IA}, e_{IA}) \\
T_I &:= f_T(X_I, e_I) \\
B_{JK} &:= f_B(B_J, Z_{JK}, e_{JK}) \\
B_J &:= f_B(Z_J, e_J) \\
e_I &\perp \{e_J, e_{IA}, e_{JK}\} \\
e_J &\perp \{e_{IA}, e_{JK}\} \\
e_{IA} &\perp e_{JK}
\end{aligned}
$$

(a) SCM



(b) DAG

Figure 6: Sample-comparison model, final vectorized form

However, due to concerns about the practical feasibility of the comparison task [Boonen et al.

(2020);], CJ experiments rarely implement an exhaustive pairings of sampled judges, stimuli, and individuals. Thus, a realistic scenario must account for the fact that judges typically compare only a subset of stimuli authored by a sample of individuals.

4.2.2. The comparison mechanism

As in the previous section, we begin defining the *comparison mechanism* using the binary vector variable C to facilitate the interpretation of the sample-comparison model. Equation (10) shows that C contains n elements corresponding to the number of rows in the R matrix, with each element $c_{(i,a,h,b,j,k)}$ being a binary value indicating the presence or absence of data rows in R, a definition similar to that of $s_{(i,a,h,b,j,k)}$ in Equation (9).

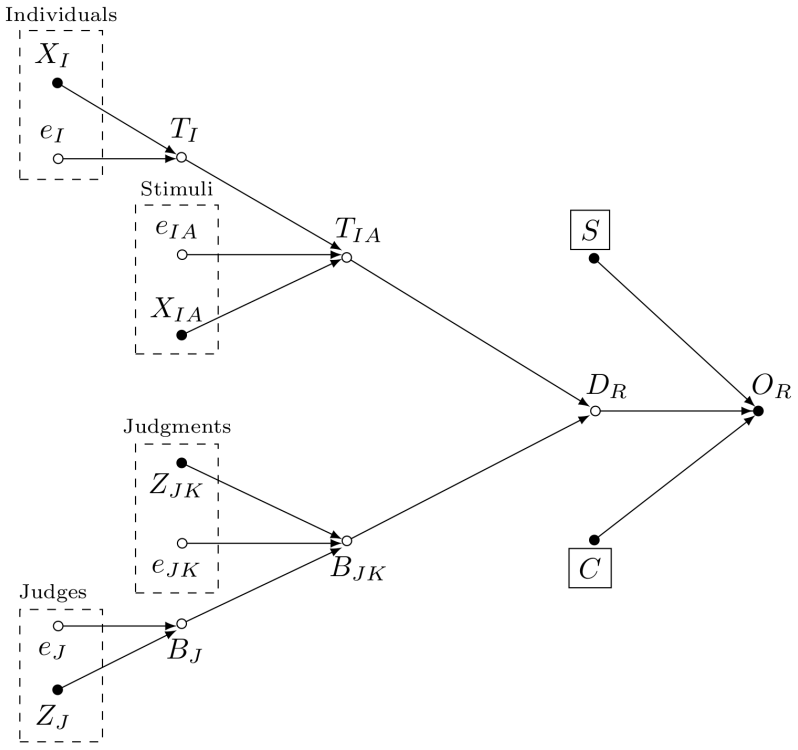
$$C = \begin{bmatrix} c_{(1,1,1,2,1,1)} \\ \vdots \\ c_{(1,1,1,2,1,n_K)} \\ \vdots \\ c_{(i,a,h,b,j,k)} \\ \vdots \\ c_{(n_I,n_A-1,n_I,n_A,n_J,1)} \\ \vdots \\ c_{(n_I,n_A-1,n_I,n_A,n_J,1)} \end{bmatrix} \tag{10}$$

The DAG 6b also incorporates the *comparison mechanism* C into the conceptual-population model. It shows the sample-comparison outcome O_R^{sc} as unobserved, emphasizing that researchers cannot directly access this variable because of the comparison mechanism. The DAG further shows C as a conditioned variable (enclosed in a square) that causally influences the observed outcome O_R. This structure implies that C determines *which repeated judgments judges make for the stimuli produced by the individuals*. In essence, C reflects the assumption that judges *do not* perform all possible repeated judgments but instead complete a sufficient number, n_C, to enable the accurate estimation of the proportion $P(B > A)$ for each stimulus pair (Thurstone, 1927a, pp. 267).

Notably, DAG 6b also shows that C is independent of all other variables in the model. This independence implies that the conceptual model represented by the DAG applies exclusively to Random Allocation Comparative Designs (Bramley, 2015), or Incomplete Block Designs (Lawson, 2015), where every repeated judgment has an equal probability of being included in the sample.

$$
\begin{aligned}
O_R &:= f_O(D_R, S, C) \\
D_R &:= f_D(T_{IA}, B_{JK}) \\
T_{IA} &:= f_T(T_I, X_{IA}, e_{IA}) \\
T_I &:= f_T(X_I, e_I) \\
B_{JK} &:= f_B(B_J, Z_{JK}, e_{JK}) \\
B_J &:= f_B(Z_J, e_J) \\
e_I &\perp \{e_J, e_{IA}, e_{JK}\} \\
e_J &\perp \{e_{IA}, e_{JK}\} \\
e_{IA} &\perp e_{JK}
\end{aligned}
$$

(a) SCM



(b) DAG

Figure 7: Comparative judgment model

Finally, since it is standard to assume that the distribution of the conceptual-population outcome O_R^{cp} also holds for O_R^{sc} and O_R, we can reformulate the sample-comparison model in Figure 6 into the equivalent form shown in Figure 7. This reformulation produces a model that applies directly to a

sample of comparative data. In this version, the unobserved outcomes O_R^{cp} and O_R^{sc} are omitted, and O_R inherits the structural equation f_O that originally defined O_R^{cp}. Moreover, the definition of O_R now reflects its direct dependence on the discriminal difference D_R and the sample and comparison mechanisms, S and C.

In summary, the SCM 7a and DAG 7b extend Thurstone's general form to address several limitations of the BTL model. These extensions account for judge biases (see Section 4.1.1), reflect the hierarchical structure of stimuli and incorporate measurement error in trait estimation and hypothesis testing (see Section 4.1.3), and clarify the role of the sample and comparison mechanisms in CJ experiments (see Section 4.2). However, they do not resolve concerns about the assumption of equal dispersions among stimuli discussed in Section 3.1.1. Since this concern relates to the statistical assumption underlying the distribution of the discriminal process, we develop a formal statistical model to address it in the next section.

5. From SCM to statistical model

Using the structural causal model (SCM) 7a, we can derive a statistical model that addresses violations of the equal dispersion assumption (see Section 3.1.1). This derivation is possible because a fully specified SCM encodes functional and probabilistic information, which we can replace with suitable functions and probabilistic assumptions (Pearl et al., 2016). Specifically, SCM 7a allows us to express the joint distribution of our complex CJ system as a product of simpler conditional probability distributions (CPDs)[6], as shown in Equation (11). For clarity, we treat expressions such as $Y := f_Y(X)$, $P(Y \mid X)$, and $Y \sim f(Y \mid X)$ as equivalent, where $P(Y \mid X)$ and $f(Y \mid X)$ represent the CPD of Y given X.

$$
\begin{aligned}
&P(O_R,S,C,D_R,T_{IA},X_{IA},e_{IA},T_I,X_I,e_I,B_{JK},Z_{JK},e_{JK},B_J,Z_J,e_J) \\
&\quad = P(O_R \mid D_R,S,C) \cdot P(S) \cdot P(C) \cdot P(D_R \mid T_{IA},B_{JK}) \\
&\qquad \cdot P(T_{IA} \mid T_I,X_{IA},e_{IA}) \cdot P(T_I \mid X_I,e_I) \\
&\qquad \cdot P(B_{JK} \mid B_J,Z_{JK},e_{JK}) \cdot P(B_J \mid Z_J,e_J) \\
&\qquad \cdot P(X_{IA}) \cdot P(X_I) \cdot P(Z_{JK}) \cdot P(Z_J) \\
&\qquad \cdot P(e_{IA}) \cdot P(e_I) \cdot P(e_{JK}) \cdot P(e_J)
\end{aligned}
\tag{11}
$$

[6]This re-expression is possible because the *chain rule* of probability and the *Bayesian Network Factorization (BNF)* property. For further details, see Pearl et al. (2016) and Neal (2020).

Each CPD in Equation (11) rests on specific assumptions, which we outline in the statistical model 8c. We begin by assuming that O_R follows a Bernoulli distribution[7], reflecting the binary nature of CJ outcomes. Furthermore, following the conventions of Generalized Linear Models (GLMs) (McCullagh and Nelder, 1983; Lee and Nelder, 1996; Agresti, 2015), the distribution links O_R to the latent discriminal difference vector D_R using an inverse-logit function: $\text{inv\_logit}(x) = 1/(1 + \exp(-x))$.

$$
\begin{array}{lll}
O_R := f_O(D_R, S, C) & P(O_R \mid D_R, S, C) & O_R \overset{iid}{\sim} \text{Bernoulli}\left[\text{inv\_logit}(D_R)\right] \\
D_R := f_D(T_{IA}, B_{JK}) & P(D_R \mid T_{IA}, B_{JK}) & D_R = (T_{IA}[i,a] - T_{IA}[h,b]) + B_{JK}[j,k] \\
T_{IA} := f_T(T_I, X_{IA}, e_{IA}) & P(T_{IA} \mid T_I, X_{IA}, e_{IA}) & T_{IA} = T_I + \beta_{XA} X_{IA} + e_{IA} \\
T_I := f_T(X_I, e_I) & P(T_I \mid X_I, e_I) & T_I = \beta_{XI} X_I + e_I \\
B_{JK} := f_B(B_J, Z_{JK}, e_{JK}) & P(B_{JK} \mid B_J, Z_{JK}, e_{JK}) & B_{JK} = B_J + \beta_{ZK} Z_{JK} + e_{JK} \\
B_J := f_B(Z_J, e_J) & P(B_J \mid Z_J, e_J) & B_J = \beta_{ZJ} Z_J + e_J
\end{array}
$$

$$
\begin{array}{lll}
e_I \perp \{e_J, e_{IA}, e_{JK}\} & P(e_I)P(e_{IA})P(e_J)P(e_{JK}) & e \sim \text{Multi-Normal}(\mu, \Sigma) \\
e_J \perp \{e_{IA}, e_{JK}\} & & \Sigma = VQV \\
e_{IA} \perp e_{JK} & &
\end{array}
$$

(a) SCM (b) Probabilistic model (c) Statistical model

Figure 8: Comparative judgment model, SCM, probabilistic and statistical model assuming different discriminal dispersions for the student's traits

Note that while the joint distribution in Equation (11) includes the probability distributions of the sampling and comparison mechanisms, $P(S)$ and $P(C)$, as well as those of the predictor variables–$P(X_{IA})$, $P(X_I)$, $P(Z_{JK})$, and $P(Z_J)$–we omit all of these probabilities from the statistical model 8c. This omission is justified because, while these distributions contribute to the overall joint distribution of the data, the variables S, C, X_{IA}, X_I, Z_{JK}, and Z_J are observed and independent of any other variable in the model. As observed variables, they do not require distributional assumptions in the same way the idiosyncratic errors do. Furthermore, their independence follows

[7]The binomial distribution–including its special case, the Bernoulli distribution–represent a maximum entropy distribution for binary events (McElreath, 2020, pp. 34). This means that the Bernoulli distribution is the most consistent alternative when only two un-ordered outcomes are possible and their expected frequencies are assumed to be constant (McElreath, 2020, pp. 310). For a detailed discussion of the binomial as a maximum entropy distribution, see McElreath (2020, sec. 10.1.2).

from the underlying random selection procedures that govern the variables[8].

Next we define D_R as the difference between the discriminal processes $T_{IA}[i,a]$ and $T_{IA}[h,b]$, representing the underlying written-quality trait of the compared texts, plus the corresponding repeated judge bias $B_{JK}[j,k]$. Note that if we assume that $B_{JK}[j,k]$ reflects the difference in stimulus-specific biases, i.e., $B_{JK}[j,k] = B_{JK}[i,a,j,k] - B_{JK}[h,b,j,k]$, we can re-write the discriminal difference as:

$$\begin{aligned} D_R &= (T_{IA}[i,a] - T_{IA}[h,b]) + B_{JK}[j,k] \\ &= (T_{IA}[i,a] + B_{JK}[i,a,j,k]) - (T_{IA}[h,b] + B_{JK}[h,b,j,k]) \\ &= T^*_{IA}[i,a] - T^*_{IA}[h,b] \end{aligned} \tag{12}$$

This formulation reveals that the discriminal difference captures a *pure interaction effect*, in which neither the texts' discriminal processes nor the judges' biases alone determine the outcome, but their interaction does (Attia et al., 2022). Put simply, this mathematical description captures the idea that the stimuli' discriminal processes become an observable outcome only through the lens of judges' perceptions (i.e., their biases). For clarity, the square brackets in D_R indicate the relevant indices for each trait vector; they do not imply any subsetting of the data.

We now specify the functional forms for T_{IA}, T_I, B_{JK}, and B_J. We model T_{IA} as a linear combination of the students' underlying writing-quality traits T_I, the effects of relevant text-related variables on quality assessment $\beta_{XA}X_{IA}$ (such as the influence of text length), and the text-specific idiosyncratic errors e_{IA}. Similarly, we express T_I as a linear combination of relevant student-related variables affecting the quality assessment $\beta_{XI}X_I$, and student-specific idiosyncratic errors e_I. For the judge-specific terms, we model B_{JK} as a linear combination of the judge's individual bias B_J, the influence of relevant judgment-related variables on quality assessment $\beta_{ZK}Z_{JK}$ (e.g., how the number of judgments affect the evaluation), and judgment-specific idiosyncratic errors e_{JK}. Finally, we define B_J as a linear combination of relevant judge-level variables influencing the quality assessment $\beta_{ZJ}Z_J$ (such as judgment expertise) and judge-specific idiosyncratic errors e_J.

[8]Randomization ensures that data–and, by extension, an estimator–satisfies several key identification properties, such as common support, no interference, and consistency. The most critical property, however, is the elimination of confounding. *Confounding* occurs when an external variable, such as X_I, simultaneously influences both the outcome (e.g., O_R) and a variable of interest (e.g., S), resulting in spurious associations between the latter two (Everitt and Skrondal, 2010). Randomization ensure the absence of confounding by effectively decoupling the association between the variable of interest and any other variable, except for the outcome itself. For a more detailed discussion on the benefits of randomization, see Pearl (2009), Morgan and Winship (2014), Neal (2020), and Hernán and Robins (2020).

Next, we specify the probabilistic assumptions for the idiosyncratic errors e_I, e_{IA}, e_J, and e_{JK}. Unlike other variables in the model, these error terms exhibit indeterminacies in their *location*, *orientation*, and *scale* due to the lack of an inherent scale in the associated latent variables T_I, T_{IA}, B_J, and B_{JK}. Thus, to identify the latent variable model, we must resolve these indeterminacies (Depaoli, 2021; de Ayala, 2009). Drawing on principles from SEM (Hoyle, 2023), we assume that the vector of idiosyncratic errors $e = [e_I, e_{IA}, e_J, e_{JK}]^T$, follows a Multivariate Normal distribution with mean vector μ and a covariance matrix $\Sigma = VQV$, with V denoting a diagonal matrix of standard deviations and Q a correlation matrix. To address the *location* indeterminacy, we set the errors' mean vector to zero:

$$\mu = [0, 0, 0, 0]^T \tag{13}$$

Following SCM 8a, we resolve the *orientation* indeterminacy by assuming that the errors are uncorrelated. This assumption leads us to define the error correlation matrix, Q, as the identity matrix:

$$Q = \begin{bmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{bmatrix} \tag{14}$$

To resolve the *scale* indeterminacy, we define the diagonal matrix V as:

$$V = \begin{bmatrix} s_{XI} & 0 & 0 & 0 \\ 0 & p_{IA} & 0 & 0 \\ 0 & 0 & s_{ZJ} & 0 \\ 0 & 0 & 0 & p_{JK} \end{bmatrix} \tag{15}$$

Here, s_{XI} represents the standard deviation for the individuals, p_{IA} for the stimuli, s_{ZJ} for the judges, and p_{JK} for the judgments. We assume s_{XI} varies depending on the teaching method group to which each student belongs. Using the example from Section 4, where the teaching method $X_I = \{1, 2\}$, the model sets the constraint according to Equation (16). This constraint anchors the scale of the individuals' latent trait while relaxing the assumption of equal dispersion for the stimuli, thereby addressing the concerns raised in Section 3.1.1.

$$\sum_{g=1}^{2} s_{XI}[g]/2 = 1 \tag{16}$$

Because the error vector e follows an uncorrelated Multivariate Normal distribution, the marginal distribution of e_{IA} is a univariate Normal distribution with mean zero and standard deviation p_{IA}.

We set P_{IA} as a proportion of 1 to establish the scale of the stimuli' latent trait relative to the scale of the individuals' trait. Note that as a result, T_{IA} is also normally distributed. This configuration effectively reinstates Thurstone's original assumption of Normal discriminal processes for the stimuli (see Table 1).

Similarly, we assume s_{ZJ} varies depending on the groups to which each judge belongs. For instance, if $Z_J = \{1, 2, 3\}$ represents three groups of judges with varying expertise, the model sets the constraint according to Equation (17). This constraint anchors the scale of the judges' latent trait and relaxes the assumption of equal dispersion for the judgments.

$$\sum_{g=1}^{2} s_{ZJ}[g]/3 = 1 \tag{17}$$

Conversely, p_{JK} is defined as a proportion of 1 to establish the scale of the judgments' latent trait relative to the scale of the judges' trait.

Finally, we use *Bayesian inference methods* to convert the statistical model 8c into a practical statistical tool for analyzing paired comparison data. Bayesian inference offers three main advantages in this context. First, it handles complex and overparameterized models, where the number of parameters exceeds the number of observations (Baker, 1998; Kim and Cohen, 1999). This capability is essential for our implementation, as the model is indeed overparameterized. Second, it incorporates prior information to constrain parameter estimates within plausible bounds, thereby mitigating estimation issues like non-convergence or improper solutions that often affect frequentist methods (Martin and McDonald, 1975; Seaman III et al., 2011). In our implementation, we use mainly use priors to define the error distribution, setting the scale of the latent variables (Depaoli, 2014). Third, Bayesian inference supports robust inferences from small samples, where the asymptotic properties underlying frequentist methods are less reliable (Baldwin and Fellingham, 2013; Lambert et al., 2006; Depaoli, 2014). This feature is particularly relevant in CJ experiments, as researchers often collect large volumes of paired comparisons but work with relatively small samples of judges, stimuli, and individuals to test hypotheses.

The **Declarations** section of this document provides a link to the model code, along with an alternative specification that assumes equal discriminal dispersions. We tested both versions of the model with success using `Stan` (Stan Development Team., 2021, version 2.26.1).

6. Discussion

Thurstone introduced the Law of Comparative Judgment to measure psychological traits of stimuli through pairwise comparisons (Thurstone, 1927b,a). In its general form, the theory models single-judge comparisons across multiple, potentially correlated stimuli. Each comparison produces a dichotomous outcome indicating which stimulus the judge perceives as having a higher trait level. However, Thurstone identified one key challenge with its measurement model: it required estimating more "unknown" parameters than the number of available pairwise comparisons (Thurstone, 1927a). To address this issue and facilitate the theory's practical applicability, he formulated five cases derived from this general form, each progressively incorporating several simplifying assumptions.

Of these five cases, the CJ literature has predominantly relied on Case V to measure various psychological traits, as shown in studies like Kimbell (2012), Jones and Inglis (2015), and Boonen et al. (2020). This preference largely stems from the widespread adoption of the BTL model, which offers a simplified statistical formulation of the case. The BTL model mirrors the assumptions of Case V–namely, equal discriminal dispersions and zero correlation for the stimuli' discriminal processes–with one notable distinction. While Case V assumes normally distributed discriminal processes, the BTL model uses the more mathematically tractable Logistic distribution (Andrich, 1978; Bramley, 2008). Although this substitution has a minimal impact on trait estimation or model interpretation (van der Linden, 2017a; McElreath, 2021), Thurstone acknowledged that assuming equal discriminal dispersions and zero correlation among stimuli' discriminal processes could introduce significant problems. In particular, he recognized that while these assumptions simplify the trait measurement model, they may not capture the complexity of some traits or account for heterogeneous stimuli, such as handwritten texts or English compositions (Thurstone, 1927b; Andrich, 1978; Bramley, 2008; Kelly et al., 2022). As a result, they can lead to unreliable and inaccurate trait estimates (Ackerman, 1989; Zimmerman, 1994; McElreath, 2020; Hoyle, 2023).

Furthermore, because Thurstone aimed primarily to produce a trait's "coarse scaling" and allocate the compared stimuli along this continuum (Thurstone, 1927b, pp. 269), his theory did not specify how to use the resulting trait estimates for statistical inference. The CJ tradition has attempted to address this gap by separating trait estimation from hypothesis testing. Specifically, CJ studies often rely on point estimates of the traits–typically the BTL scores or its transformations–to conduct statistical inference. Although this separation simplifies CJ data analysis, the statistical literature warns that this practice can introduce bias into the analysis and compromise the reliability of statistical inferences (McElreath, 2020; Kline, 2023; Hoyle, 2023).

Thus, to address the limitations of Thurstone's Case V and the BTL model–particularly their strong assumptions and the disconnect between trait measurement and hypothesis testing–this study extends Thurstone's general form using a systematic, integrated approach that combines Causal and Bayesian inference methods. The approach begins by formulating a conceptual model represented by a Structural Causal Model (SCM) and a Directed Acyclic Graph (DAG). This model integrates Thurstone's core theoretical principles–such as the discriminal processes of stimuli–alongside key CJ experimental design features, including judges' bias, sampling procedures, and comparison mechanisms, thereby disentangling the causal processes in the CJ system.

The study then translates the SCM into a bespoke statistical model that addresses key limitations of Case V. This model allows researchers to analyze CJ data when the assumptions of equal dispersion and zero correlation do not hold (see Section 3.1.1 and Section 3.1.2), and when they need to perform statistical inference (refer to Section 3.2). In particular, the model accounts for judge biases (see Section 4.1.1), captures the hierarchical structure of stimuli and incorporates measurement error in the hypothesis testing process (refer to Section 4.1.3), and accommodates heterogeneity in discriminal dispersions (see Section 5). These methodological innovations can potentially enhance the reliability and validity of trait measurement (Perron and Gillespie, 2015) and improve the accuracy of statistical inferences.

Beyond these potential benefits, the approach offers two additional advantages. First, it clarifies the roles and interactions of all actors and processes involved in CJ experiments. Second, it shifts the analytic paradigm from passively accepting the assumptions of the BTL model to actively testing their fit with observed data. Together, these advantages establish a principled framework for evaluating best practices in CJ experimental designs, one that better aligns with the demands of contemporary CJ assessment contexts (Kelly et al., 2022), offering new insights into existing research and opening promising avenues for future inquiry.

Five research avenues–current and new–deserve particular attention. The following sections outline these avenues and explain how our model provides a rigorous research framework to investigate them. Notably, while addressing these questions through formal derivations–following the tradition of classical statistical models like linear regression–may seem a natural step, we argue that the complexity of the CJ system makes this approach initially impractical. Instead, simulation-based methods, such as power analysis, offer a more feasible and flexible alternative, enabling researchers to investigate these questions without relying on cumbersome mathematical proofs. Nonetheless,

developing formal mathematical proofs remains an essential goal for future work.

6.1. The impact of sampling and comparison mechanisms on CJ outcomes and inference robustness

Sampling and comparison mechanisms play a central role in modern CJ experimental design. However, most of the CJ literature has examined these mechanisms within a limited scope. Researchers have primarily investigated the effects of adaptive comparative judgment (ACJ) designs on trait reliability (Pollitt, 2012a,b; Bramley, 2015; Verhavert et al., 2022; Mikhailiuk et al., 2021; Gray et al., 2024) or proposed practical guidelines for the number of comparisons judges should make (Verhavert et al., 2019; Crompvoets et al., 2022). Although these studies offer valuable insights, we argue that they may have overlooked the broader role these mechanisms play within the CJ system. Moreover, we suggest that this oversight largely stems from a lack of conceptual clarity about how the mechanisms function within the system. To address this gap, this study decided to integrate these mechanisms into CJ's conceptual model.

The explicit integration of the sampling and comparison mechanisms offers a new perspective on how these mechanisms shape the CJ process. In particular, it clarifies their role in the data-generating process by framing them as sources of missing data–that is, as mechanisms that determine which observations are missing from the final data sample. This new perspective encourages the application of Little and Rubin's principled missing data framework (2020), enabling a more rigorous evaluation of existing claims about these mechanisms, their influence on CJ outcomes, and their implications for designing and assessing more complex experimental setups.

Notably, this study circumvents the need to apply this missing data framework by deliberately structuring the sampling and comparison mechanisms to be independent of any observed or unobserved variables, including the outcome. In other words, we designed these mechanisms so they produce data that are *missing completely at random* (MCAR) (Little and Rubin, 2020). This design offers one key advantage: it generates simple random samples that satisfy the condition of *ignorability*, allowing researchers to legitimately *ignore* missing data during analysis without introducing bias (Everitt and Skrondal, 2010; Kohler et al., 2019; Neal, 2020).

However, many modern CJ applications rely on more complex experimental designs in which the sampling and comparison mechanisms introduce more intricate forms of missingness–such as *missing at random* (MAR) or *missing not at random* (MNAR) (Little and Rubin, 2020). A prominent example is the previously discussed ACJ designs, where prior judgment outcomes inform the selection of stimulus pairs for subsequent comparisons (Pollitt, 2012a,b; Bramley, 2015). This outcome-informed

selection suggests that the comparison mechanism may be outcome-dependent, potentially classifying ACJ as a generator of MNAR data. This classification may, in turn, facilitate a more nuanced understanding of the mixed evidence surrounding ACJ's capabilities. While some studies report improvements in trait reliability (Pollitt and Elliott, 2003; Pollitt, 2012a,b), others argue that these gains are artificially inflated by the method (Bramley, 2015; Bramley and Vitello, 2019; Crompvoets et al., 2020, 2022).

Nevertheless, regardless of the underlying missingness mechanisms, any CJ experimental design would benefit from explicitly defining its assumptions–an approach supported by our framework. This clarity enables researchers to evaluate, for instance, how sampling and comparison mechanisms affect trait estimation and statistical inference within each design. Such assessments are particularly relevant given the common misconception in the CJ literature that Thurstone's model can naturally handle even non-random missing data without compromising the reliability or validity of trait estimates (Bramley, 2008).

6.2. The effects of judges' bias on the reliability of traits

Despite the growing notion that various stimulus-related factors can influence judges' perceptions during pairwise comparisons–and that these influences may not always cancel each other out–few studies in the CJ literature provide empirical evidence for judges' biases (Pollitt and Elliott, 2003; van Daal et al., 2016; Bartholomew et al., 2020). We argue that this gap persists not due to a lack of interest or research but because researchers continue to rely on ad-hoc detection methods, such as 'misfit' statistics, that are poorly suited for the task (Kelly et al., 2022). To address this limitation, this study decided to treat judges' biases as an integral component of the CJ system from the outset. This approach offers one key advantage: it provides a more accurate representation of the data-generating process behind pairwise comparisons–one that acknowledges that the discriminal processes of stimuli become an observable outcome only through judges' perceptions, which may exhibit bias.

The explicit integration of judges' bias into CJ's conceptual model then paves the way for investigating several relevant research questions. For instance, can researchers validly analyze CJ data under the assumption of "sample-free" trait calibration–specifically, under the assumption that judges exhibit no systematic bias? This question is especially relevant because researchers often treat "sample-freeness" as an inherent property of the BTL model (Bramley, 2008; Andrich, 1978) despite growing evidence of persistent biases. Furthermore, if judges' biases reflect additional stimulus

characteristics that should not influence assessment, to what extent can training or expertise mitigate these biases? (Kelly et al., 2022) More specifically, as discussed in Section 3.1.2, can training or expertise help prevent judges from prioritizing irrelevant features–such as handwriting–over more central criteria like argumentative quality in writing assessments? Addressing these questions may also shed light on what it truly means to be an "expert" in the CJ context (Kelly et al., 2022).

Moreover, if we accept that judges may value different aspects of a stimulus, it follows that their judgments might also vary with individual characteristics such as gender, age, culture, income, education, training, or expertise (Kelly et al., 2022; Bartholomew et al., 2020), and even from one individual to another (Gill and Bramley, 2013; van Daal et al., 2016, 2017; van Daal, 2020). Building on this and the discussion in Section 6.1, researchers could investigate how the selection of judges influences the formation of a "shared consensus" and whether these individual characteristics introduce systematic biases or distortions in the observed trait distribution (Deffner et al., 2022). Furthermore, if such characteristics indeed undermine the "sample-freeness" assumption, then it becomes essential to explore strategies for controlling their influence. This includes investigating how many judges–and how many judgments per judge–are required to produce reliable trait estimates, as well as whether *repeated measures designs*, in which judges evaluate the same stimulus pairs multiple times (Lawson, 2015), can improve judgment consistency and accuracy.

6.3. The identification of 'misfitting' judges and stimuli

About the identification of 'misfitting' judges and stimuli

6.4. The effects of hierarchies on experimental design and inference

About the hierarchical structure of stimuli

6.5. The effects of non-dichotomous outcomes and multidimensional traits on CJ experiments

About non-dichotomous outcomes and multidimensional traits

6.6. Challenges

Declarations

Funding: The Research Fund (BOF) of the University of Antwerp funded this project.

Financial interests: The authors declare no relevant financial interests.

Non-financial interests: The authors declare no relevant non-financial interests.

Ethics approval: The University of Antwerp Research Ethics Committee confirmed that this study does not require ethical approval.

Consent to participate: Not applicable

Consent for publication: All authors have read and approved the final version of the manuscript for publication.

Data availability: This study did not use any data.

Materials and code availability: The `CODE LINK` section at the top of the digital document located at: https://jriveraespejo.github.io/paper2_manuscript/ provides access to all materials and code.

AI-assisted technologies in the writing process: The authors used various AI-based language tools to refine phrasing, optimize wording, and enhance clarity and coherence throughout the manuscript. They take full responsibility for the final content of the publication.

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

7. Appendix

7.1. Statistical and Causal inference

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).

7.1.1. 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 18, where $E[\cdot]$ denotes the expected value.

$$E[Y_i \mid T_i = 1] - E[Y_i \mid T_i = 2] \tag{18}$$

Researchers then proceed to identify the estimands. *Identification* determines whether an estimator can accurately compute the estimand based solely on its assumptions, regardless of random variability (Schuessler and Selb, 2023, pp. 4). An *estimator* refers to a method or function that transforms data into an estimate (Neal, 2020). *Estimates* are numerical values that approximate the estimand derived through the process of *estimation*, which integrates data with an estimator (Everitt and

Skrondal, 2010). The Identification-Estimation flowchart (McElreath, 2020; Neal, 2020), shown in Figure 9, visually represents the transition from estimands to estimates.

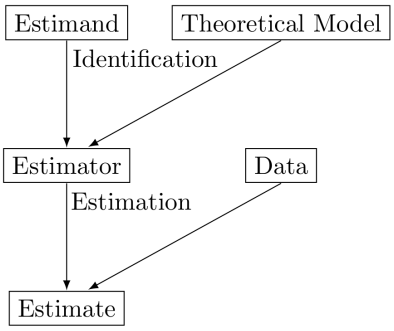


Figure 9: Identification-Estimation flowchart. Extracted and slightly modified from Neal (2020, pp. 32)

Identification is a necessary condition to ensure *consistent* estimators. An estimator achieves *consistency* when it converges to the "true" value of an estimand as the data size approaches infinity (Everitt and Skrondal, 2010). Without identification, researchers cannot achieve consistency, even with "infinite" and error-free data. As a result, deriving meaningful insights about an estimand from finite data becomes impossible (Schuessler and Selb, 2023, pp. 5). Therefore, to ensure accurate and reliable estimates, researchers prioritize estimators with desirable identification properties. For instance, the Z-test is a widely used estimator for comparing group proportions, yielding accurate estimates when its underlying assumptions are satisfied (Kanji, 2006). Furthermore, researchers can interpret estimates from the Z-test as causal, provided the data is 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 identification 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).

7.1.2. 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 7.1.1, the approach begins by defining the *individual causal effect* (ICE) as the difference between each student's potential outcomes, as in Equation 19.

$$\tau_i = Y_i \mid do(T_i = 1) - Y_i \mid do(T_i = 2) \tag{19}$$

where $do(T_i = t)$ represents the intervention operator, $Y_i \mid do(T_i = 1)$ represents the potential outcome under intervention $T_i = 1$, and $Y_i \mid do(T_i = 1)$ 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. 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 20), representing the average difference between the students' observed potential outcomes and their counterfactual counterparts.

$$\begin{aligned}\tau &= E[\tau_i] \\ &= E[Y_i \mid do(T_i = 1)] - E[Y_i \mid do(T_i = 2)]\end{aligned} \tag{20}$$

Even though counterfactuals are 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. When such a set exists, then T and Y are *d-separated* by X ($T \perp Y \mid X$) (Pearl, 2009), and X satisfies the *backdoor criterion* (Neal, 2020, pp 37). Here, *conditioning* describes the process of restricting the focus to the subset of the population defined by the conditioning variable (Neal, 2020, pp. 32) (see Equation 21).

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 7.1.1 and must instead rely on observational data, they can still identify the ACE as long as an observed 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 21)

$$CACE_t = E[Y_i \mid T_i = t, X] \tag{21}$$

From the CACE, researchers can identify the ACE from associational quantities as in Equation 22. This identification process is commonly known as the *backdoor adjustment*. Here, $E_X[\cdot]$ represents the marginal expected value over X (Morgan and Winship, 2014).

$$\begin{aligned}\tau &= E[Y_i \mid do(T_i = 1)] - E[Y_i \mid do(T_i = 2)] \\ &= E_X[CACE_1 - CACE_2] \\ &= E_X\left[E[Y_i \mid T_i = 1, X] - E[Y_i \mid T_i = 2, X]\right]\end{aligned} \tag{22}$$

Notably, the approach extends the ACE identification for a continuous variable T as in Equation 23, ensuring broad applicability across different causal scenarios (Neal, 2020, pp. 45)

$$\begin{aligned}\tau &= E[Y_i \mid do(T_i = t)] \\ &= dE_X\left[E[Y_i \mid T_i = t, X]\right]/dt\end{aligned} \tag{23}$$

7.1.3. Diving into the specifics

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, these tools serve as *conceptual (theoretical) models* on which identification analysis rests (Schuessler and Selb, 2023, pp. 4). Thus, using these tools, researchers can determine which statistical models can identify (ACE, CACE, or other), assuming the depicted causal structure is correct (McElreath, 2020), thus enabling valid causal inference. Figure 9 shows the role of theoretical models in the inference process.

SCMs and DAGs support identification analysis 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).

Thus, Section 7.1.3.1 and Section 7.1.3.2 elaborate on the first advantage, while Section 7.1.3.2 and Section 7.1.3.3 do so for the second. Finally, Section 7.1.3.4 explains how researchers use SCMs and DAGs alongside Bayesian inference methods in the estimation process.

7.1.3.1. The five fundamental block for SCMs and DAGs.

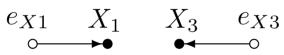
Figures 10, 11, 12, 13, and 14 display the five fundamental building blocks for SCMs and DAGs. The left panels of the figures show the formal mathematical models, represented by the SCMs,

defined in terms of a set of *endogenous* variables $V = \{X_1, X_2, X_3\}$, 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 the *standard representation* of DAGs typically omits exogenous variables for simplicity, the *magnified representation* depicted in the figures offers one key advantage: including exogenous variables can help researchers highlight potential issues related to conditioning and confounding (Cinelli et al., 2020).

$$X_1 := f_{X1}(e_{X1})$$
$$X_3 := f_{X3}(e_{X3})$$
$$e_{X1} \perp e_{X3}$$

(a) SCM



(b) DAG

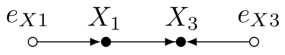
Figure 10: Two unconnected nodes

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

$$X_3 := f_{X3}(X_1, e_{X3})$$

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

(a) SCM



(b) DAG

Figure 11: Two connected nodes or descendant

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

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

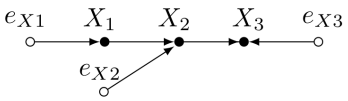
$$X_3 := f_{X3}(X_2, e_{X3})$$

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

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

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

(a) SCM



(b) DAG

Figure 12: Chain or mediator

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

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

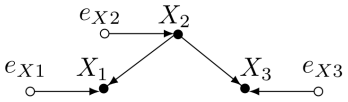
$$X_3 := f_{X3}(X_2, e_{X3})$$

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

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

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

(a) SCM



(b) DAG

Figure 13: Fork or confounder

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

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

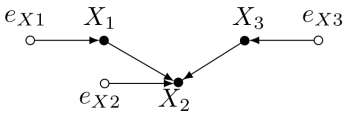
$$X_3 := f_{X3}(e_{X3})$$

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

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

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

(a) SCM



(b) DAG

Figure 14: Collider or inmorality

A careful examination of these building blocks highlights the theoretical assumptions underlying their observed variables. SCM 10a and DAG 10b depict two unconnected nodes, representing a scenario where variables X_1 and X_3 are independent or not causally related. SCM 11a and DAG 11b 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 them. SCM 12a and DAG 12b 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 13a and DAG 13b 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 14a and DAG 14b 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 *inmorality*). 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 7.1.2. SCM 15a and DAG 15b depict the plausible causal structure for this example. In this context, the variable X (socio-economic status of the school) is thought to be a confounder in the relationship between the teaching method T and the outcome Y. The figures display multiple descendant relationships such as $X \to T$, $X \to Y$, and $T \to Y$. They also highlight unconnected node pairs,

evident from the relationships $e_T \perp e_X$, $e_T \perp e_Y$, and $e_X \perp e_Y$. Additional, the figures show one fork, $X \to \{T, Y\}$, and two colliders: $\{X, e_T\} \to T$ and $\{X, T, e_Y\} \to Y$.

$$X := f_X(e_X)$$

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

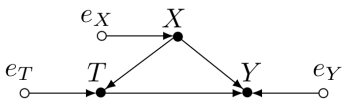
$$Y := f_Y(T, X, e_Y)$$

$$e_T \perp e_X$$

$$e_T \perp e_Y$$

$$e_X \perp e_Y$$

(a) SCM



(b) DAG

Figure 15: Plausible causal structure the scenario outlined in Section 7.1.2.

7.1.3.2. The probabilistic implications of these blocks.

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 as a direct cause of its children (Neal, 2020, pp. 22). Figure 16 illustrates how these assumptions influence the statistical and causal interpretations of graphs.

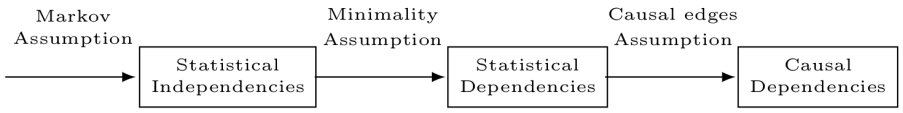


Figure 16: The flow of association and causation in graphs. Extracted and slightly modified from Neal (2020, pp. 31)

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 $P(child \mid parents)$. This property is formally known

as the *Bayesian Network factorization* (BNF, Equation 24) (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.

$$\begin{aligned} P(X_1, X_2, \dots, X_P) &= X_1 \cdot \prod_{p=2}^{P} P(X_i \mid X_{i-1}, \dots, X_1) \quad \text{(by chain rule)} \\ &= X_1 \cdot \prod_{p=2}^{P} P(X_i \mid pa(X_i)) \qquad\qquad \text{(by BNF)} \end{aligned} \tag{24}$$

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 the data can provide significant insights into the validity of the proposed conceptual model.

The five fundamental building blocks described in Section 7.1.3.1 clearly illustrate which (in)dependencies can SMCs and DAGs predict. For instance, applying the BNF to the causal structure shown in the SCM 10a and DAG 10b enables researchers to express the joint probability distribution of the observed variables as $P(X_1, X_3) = P(X_1)P(X_3)$, supporting the theoretical assumption that the observed variables X_1 and X_3 are unconditionally independent ($X_1 \perp X_3$) (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 11a and DAG 11b, the BNF express their joint probability distribution as $P(X_1, X_3) = P(X_3 \mid X_1)P(X_1)$. Notably, these descriptions demonstrate the clear correspondence between the structural equations illustrated in Section 7.1.3.1 and the CPDs.

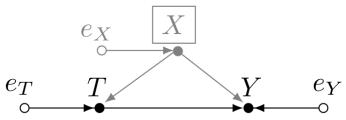
Beyond the insights gained from two-node structures, researchers can uncover more nuanced patterns of(in)dependencies from chains, forks, and colliders. These (in)dependencies apply to any data set generated by a causal model with those structures, regardless of the specific functions attached to the SCM (Pearl et al., 2016, pp. 36). For instance, applying the BNF to the chain structure depicted in the SCM 12a and DAG 12b allow researchers to represent the joint distribution for the observed variables as $P(X_1, X_2, X_3) = P(X_1)P(X_2 \mid X_1)P(X_3 \mid X_2)$. 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 ($X_1 \perp X_3 \mid X_2$). Moreover, in the fork structure shown in the SCM 13a and DAG 13b, researchers can express the joint distribution of the observed variables as $P(X_1, X_2, X_3) = P(X_1 \mid X_2)P(X_2)P(X_3 \mid X_2)$. Similar to the chain structure, 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 ($X_1 \perp X_3 \mid X_2$). Finally, researchers analyzing the collider structure illustrated in the SCM 14a and DAG 14b can express the joint distribution of the observed variables as $P(X_1, X_2, X_3) = P(X_1)P(X_2 \mid X_1, X_3)P(X_3)$. 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 ($X_1 \not\perp X_3 \mid X_2$). The authors Pearl et al. (2016, pp. 37, 40, 41) and Neal (2020, pp. 25–26) provide the mathematical proofs for these conclusions.

Using these additional probabilistic insights, researchers can revisit the scenario in Section 7.1.2. In this context, applying the BNF to the SCM 17a structure, enables the representation of the joint probability distribution of the observed variables as $P(Y, T, X) = P(Y \mid T, X)P(T \mid X)P(X)$. From this expression, researchers can infer that the outcome Y is unconditionally dependent on the teaching method T ($Y \not\perp T$). This dependency arises from two key structures: a direct causal path from the teaching method T to the outcome Y, represented by the two-connected-nodes structure $T \to Y$ (black path in DAG 17b), and a confounding non-causal path from the teaching method T to the outcome Y through the socio-economic status of the school X, represented by the fork structure $T \leftarrow X \to Y$ (gray path in DAG 17b).

$$
\begin{aligned}
X &:= x \\
T &:= f_T(x, e_T) \\
Y &:= f_Y(T, x, e_Y) \\
e_T &\perp e_X \\
e_T &\perp e_Y \\
e_X &\perp e_Y
\end{aligned}
$$

(a) SCM



(b) Conditioned DAG

Figure 17: Plausible causal structure the scenario outlined in Section 7.1.2.

7.1.3.3. From probability to causality.

The structural approach to causal inference translates probabilistic insights into actionable strategies seeking to identify the ACE from associational quantities. The approach achieves this by relying on the *modularity assumption*, which posits that intervening on a node alters only the causal mechanism of that node, leaving others unchanged (Neal, 2020, pp. 34).

The modularity assumption underpins the concepts of manipulated graphs and Truncated Fac-

torization, which are essential for representing interventions $P(Y_i \mid do(T_i = t))$ within SCMs and DAGs. *Manipulated graphs* simulate physical interventions by removing specific edges from a DAG, while preserving the remaining structure unchanged (Neal, 2020, pp. 34). In parallel, *Truncated Factorization* (TF) achieves a similar simulation by removing specific functions from the conceptual model and replacing them with constants, while keeping the rest of the structure unchanged (Pearl, 2010). The probabilistic implications of this factorization are formalized in Equation 25, where S represents the subset of variables X_p directly influenced by the intervention, while an example illustrating these concepts follows below.

$$P(X_1, X_2, \ldots, X_P \mid do(S)) = \begin{cases} \prod P(X_p \mid pa(X_p)) & \text{if } p \notin S \\ 1 & \text{otherwise} \end{cases} \tag{25}$$

Using the TF, researchers can define the *backdoor adjustment* to identify the ACE. This adjustment states 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 26. The sufficient adjustment set (potentially empty) must block all non-causal paths between X_a and X_b without introducing new paths. If such a set exists, then X_a and X_b are *d-separated* by X_p ($X_a \perp X_b \mid X_p$) (Pearl, 2009), and X_p satisfies the *backdoor criterion* (Neal, 2020, pp. 37).

$$P(X_a \mid do(X_b = x)) = \sum_{Xp} P(X_a \mid X_b = x, X_p) P(X_p) \tag{26}$$

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

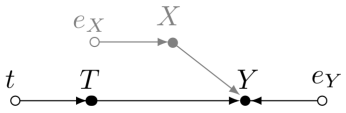
$$\begin{aligned} \tau &= E[X_a \mid do(X_b = 1)] - E[X_a \mid do(X_b = 2)] \\ &= E_{Xp}\left[E[X_a \mid do(X_b = 1), X_p] - E[X_a \mid do(X_b = 2), X_p]\right] \\ &= \sum_{Xp} X_a \cdot P(X_a \mid X_b = 1, X_p) \cdot P(X_p) - \sum_{Xp} X_a \cdot P(X_a \mid X_b = 2, X_p) \cdot P(X_p) \end{aligned} \tag{27}$$

With these new insights, researchers revisiting the scenario in Section 7.1.3.2 can infer that the socio-economic status of the school, X, satisfies the backdoor criterion, assuming the causal structure depicted by the SCM 17a and DAG 17b is correct. This means that X serves as a sufficient adjustment set, as it effectively blocks all confounding non-causal paths introduced by the fork structure. Nevertheless, since Y remains dependent on T even after conditioning ($Y \not\perp T \mid X$), this

dependency can only be attributed to the direct causal effect $T \to Y$. Notably, for the purpose of identification, the conditioned DAG 17b is equivalent to the manipulated DAG 18b, because X satisfies the backdoor criterion.

$$
\begin{aligned}
X &:= f_X(e_X) \\
T &:= t \\
Y &:= f_Y(t, X, e_Y) \\
e_T &\perp e_X \\
e_T &\perp e_Y \\
e_X &\perp e_Y
\end{aligned}
$$

(a) SCM



(b) Manipulated DAG

Figure 18: Plausible causal structure the scenario outlined in Section 7.1.3.2.

Researchers can then apply the *backdoor adjustment* to identify the ACE of T on Y. They achieve this by first identifying the CACE of T on Y by conditioning on X, and then marginalizing this effect over X to obtain the ACE. This process is expressed in Equation 28 (see Section 7.1.2).

$$
\begin{aligned}
\tau &= E[Y_i \mid do(T_i = 1)] - E[Y_i \mid do(T_i = 2)] \\
&= E_X\left[E[Y_i \mid T_i = 1, X] - E[Y_i \mid T_i = 2, X]\right] \\
&= \sum_X Y_i \cdot P(Y_i \mid T_i = 1, X) \cdot P(X) - \sum_X Y_i \cdot P(Y_i \mid T_i = 2, X) \cdot P(X)
\end{aligned} \tag{28}
$$

7.1.3.4. The estimation process.

Ultimately, researchers can use Bayesian inference methods to estimate the ACE. The approach begins by defining two probability distributions: the likelihood of the data, $P(X_1, X_2, \ldots, X_P \mid \theta)$, and the prior distribution, $P(\theta)$ (Everitt and Skrondal, 2010), where X_P represents a random variable, and θ represents a one-dimensional parameter space for simplicity. After observing empirical data, researchers can update the priors to posterior distributions using Bayes' rule in Equation 29:

$$
P(\theta \mid X_1, X_2, \ldots, X_P) = \frac{P(X_1, X_2, \ldots, X_P \mid \theta) \cdot P(\theta)}{P(X_1, X_2, \ldots, X_P)} \tag{29}
$$

Given that the denominator on the right-hand side of Equation 29 serves as a normalizing constant

independent of the parameter θ, researchers can simplify the posterior updating process into three steps. First, they integrate new empirical data through the likelihood. Second, they update the parameters' priors to a posterior distribution according to Equation 30. Ultimately, they normalize these results to obtain a valid probability distribution.

$$P(\theta \mid X_1, X_2, \dots, X_P) \propto P(X_1, X_2, \dots, X_P \mid \theta) \cdot P(\theta) \tag{30}$$

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 quickly becomes intractable (Neal, 2020). This intractability is evident from Equation 31, where the likelihood distribution is expressed by multiple chained CPDs.

$$P(X_1, X_2, \dots, X_P \mid \theta) = P(X_1 \mid \theta) \prod_{p=2}^{P} P(X_i \mid X_{i-1}, \dots, X_1, \theta) \tag{31}$$

Nevertheless, researchers can manage the complexity of the likelihood by assuming specific local (in)dependencies among variables. SCMs and DAGs provide a formal framework to represent these assumptions, as detailed in Section 7.1.3.2. These assumptions improve model tractability and simplify the estimation process by enabling the derivation of the BNF of the likelihood (Equation 32). With this simplified structure, any probabilistic programming language can model the system and compute the parameter's posterior distribution using Equation 29.

$$P(X_1, X_2, \dots, X_P \mid \theta) = P(X_1 \mid \theta) \prod_{p=2}^{P} P(X_i \mid pa(X_i), \theta) \tag{32}$$

References

Ackerman, T., 1989. Unidimensional irt calibration of compensatory and noncompensatory multidimensional items. Applied Psychological Measurement 13, 113–127. doi:10.1177/014662168901300201.

Agresti, A., 2015. Foundations of linear and generalized linear models. Wiley series in probability and statistics, John Wiley & Sons.

Andrich, D., 1978. Relationships between the thurstone and rasch approaches to item scaling. Applied Psychological Measurement 2, 451–462. doi:10.1177/014662167800200319.

Attia, J., Holliday, E., Oldmeadow, C., 2022. A proposal for capturing interaction and effect modification using dags. International Journal of Epidemiology 51, 1047–1053. doi:10.1093/ije/dyac126.

Baker, F., 1998. An investigation of the item parameter recovery characteristics of a gibbs sampling procedure. Applied Psychological Measurement 22, 153–169. doi:10.1177/01466216980222005.

Baldwin, S., Fellingham, G., 2013. Bayesian methods for the analysis of small sample multilevel data with a complex variance structure. Journal of Psychological Methods 18, 151–164. doi:10.1037/a0030642.

Bartholomew, S., Nadelson, L., Goodridge, W., Reeve, E., 2018. Adaptive comparative judgment as a tool for assessing open-ended design problems and model eliciting activities. Educational Assessment 23, 85–101. doi:10.1080/10627197.2018.1444986.

Bartholomew, S., Williams, P., 2020. Stem skill assessment: An application of adaptive comparative judgment, in: Anderson, J., Li, Y. (Eds.), Integrated Approaches to STEM Education. Advances in STEM Education. Springer, pp. 331–349. doi:10.1007/978-3-030-52229-2_18.

Bartholomew, S., Yoshikawa, E., Hartell, E., Strimel, G., 2020. Identifying design values across countries through adaptive comparative judgment. International Journal of Technology and Design Education doi:10.1007/s10798-019-09506-8.

Boonen, N., Kloots, H., Gillis, S., 2020. Rating the overall speech quality of hearing-impaired children by means of comparative judgements. Journal of Communication Disorders 83, 1675–1687. doi:10.1016/j.jcomdis.2019.105969.

Bouwer, R., Lesterhuis, M., De Smedt, F., Van Keer, H., De Maeyer, S., 2023. Comparative approaches to the assessment of writing: Reliability and validity of benchmark rating and comparative judgement. Journal of Writing Research 15, 497–518. doi:10.17239/jowr-2024.15.03.03.

Bradley, R., Terry, M., 1952. Rank analysis of incomplete block designs: I. the method of paired comparisons. Biometrika 39, 324–345. doi:10.2307/2334029.

Bramley, T., 2008. Paired comparison methods, in: Newton, P., Baird, J., Goldsteing, H., Patrick, H., Tymms, P. (Eds.), Techniques for monitoring the comparability of examination standards. GOV.UK., pp. 246—300. URL: https://assets.publishing.service.gov.uk/media/5a80d75940f0b62305b8d734/2007-comparability-exam-standards-i-chapter7.pdf.

Bramley, T., 2015. Investigating the reliability of adaptive comparative judgment. URL: http://www.cambridgeassessment.org.uk/Images/232694-investigating-the-reliability-of-adaptive-comparative-judgment.pdf. cambridge Assessment Research Report.

Bramley, T., Vitello, S., 2019. The effect of adaptivity on the reliability coefficient in adaptive comparative judgement. Assessment in Education: Principles, Policy & Practice 26, 43–58. doi:10.1080/0969594X.2017.1418734.

Casalicchio, G., Tutz, G., Schauberger, G., 2015. Subject-specific bradley–terry–luce models with implicit variable selection. Statistical Modelling 15, 526–547. doi:10.1177/1471082X15571817.

Chambers, L., Cunningham, E., 2022. Exploring the validity of comparative judgement: Do judges attend to

construct-irrelevant features? Frontiers in Education doi:10.3389/feduc.2022.802392.

Cinelli, C., Forney, A., Pearl, J., 2020. A crash course in good and bad controls. SSRN URL: https://ssrn.com/abstract=3689437, doi:10.2139/ssrn.3689437.

Coertjens, L., Lesterhuis, M., Verhavert, S., Van Gasse, R., De Maeyer, S., 2017. Teksten beoordelen met criterialijsten of via paarsgewijze vergelijking: een afweging van betrouwbaarheid en tijdsinvestering. Pedagogische Studien 94, 283–303. URL: https://repository.uantwerpen.be/docman/irua/e71ea9/147930.pdf.

Counterfactual, 2024. Merriam-webster.com dictionary. URL: https://www.merriam-webster.com/dictionary/hacker. retrieved July 23, 2024.

Crompvoets, E., Béguin, A., Sijtsma, K., 2020. Adaptive pairwise comparison for educational measurement. Journal of Educational and Behavioral Statistics 45, 316–338. doi:10.3102/1076998619890589.

Crompvoets, E., Béguin, A., Sijtsma, K., 2022. On the bias and stability of the results of comparative judgment. Frontiers in Education 6. doi:10.3389/feduc.2021.788202.

de Ayala, R., 2009. The Theory and Practice of Item Response Theory. Methodology in the Social Sciences, The Guilford Press.

Deffner, D., Rohrer, J., McElreath, R., 2022. A causal framework for cross-cultural generalizability. Advances in Methods and Practices in Psychological Science 5. doi:10.1177/25152459221106366.

Depaoli, S., 2014. The impact of inaccurate "informative" priors for growth parameters in bayesian growth mixture modeling. Journal of Structural Equation Modeling 21, 239–252. doi:10.1080/10705511.2014.882686.

Depaoli, S., 2021. Bayesian Structural Equation Modeling. Methodology in the social sciences, The Guilford Press.

Everitt, B., Skrondal, A., 2010. The Cambridge Dictionary of Statistics. Cambridge University Press.

Fox, J., 2010. Bayesian Item Response Modeling, Theory and Applications. Statistics for Social and Behavioral Sciences, Springer.

Gijsen, M., van Daal, T., Lesterhuis, M., Gijbels, D., De Maeyer, S., 2021. The complexity of comparative judgments in assessing argumentative writing: An eye tracking study. Frontiers in Education 5. doi:10.3389/feduc.2020.582800.

Gill, T., Bramley, T., 2013. How accurate are examiners' holistic judgements of script quality? Assessment in Education: Principles, Policy and Practice 20, 308—324. doi:10.1080/0969594X.2013.779229.

Goossens, M., De Maeyer, S., 2018. How to obtain efficient high reliabilities in assessing texts: Rubrics vs comparative judgement, in: Ras, E., Guerrero Roldán, A. (Eds.), Technology Enhanced Assessment, Springer International Publishing. pp. 13–25. doi:10.1007/978-3-319-97807-9_2.

Gray, A., Rahat, A., Crick, T., Lindsay, S., 2024. A bayesian active learning approach to comparative judgement within education assessment. Computers and Education: Artificial Intelligence 6, 100–245. URL: https://www.sciencedirect.com/science/article/pii/S2666920X24000481, doi:10.1016/j.caeai.2024.100245.

Gross, J., Yellen, J., Anderson, M., 2018. Graph Theory and Its Applications. Textbooks in Mathematics, Chapman and Hall/CRC. doi:https://doi.org/10.1201/9780429425134. 3rd edition.

Grubbs, F., 1969. Procedures for detecting outlying observations in samples. Technometrics 11, 1–21. URL: https://www.tandfonline.com/doi/abs/10.1080/00401706.1969.10490657, doi:10.1080/00401706.1969.10490657.

Hansson, S., 2014. Why and for what are clinical trials the gold standard? Scandinavian Journal of Public Health 42, 41–48. doi:10.1177/1403494813516712. pMID: 24553853.

Hariton, E., Locascio, J., 2018. Randomised controlled trials – the gold standard for effectiveness research. BJOG: An International Journal of Obstetrics & Gynaecology 125, 1716–1716. URL: https://obgyn.onlinelibrary.wiley.com/doi/abs/10.1111/1471-0528.15199, doi:10.1111/1471-0528.15199.

Hernán, M., Robins, J., 2020. Causal Inference: What If. 1 ed., Chapman and Hall/CRC. URL: https://www.hsph.harvard.edu/miguel-hernan/causal-inference-book. last accessed 31 July 2024.

Hoyle, R.e., 2023. Handbook of Structural Equation Modeling. Guilford Press.

Jones, I., Bisson, M., Gilmore, C., Inglis, M., 2019. Measuring conceptual understanding in randomised controlled trials: Can comparative judgement help? British Educational Research Journal 45, 662–680. doi:10.1002/berj.3519.

Jones, I., Inglis, M., 2015. The problem of assessing problem solving: can comparative judgement help? Educational Studies in Mathematics 89, 337–355. doi:10.1007/s10649-015-9607-1.

Kanji, G., 2006. 100 Statistical Tests. Introduction to statistics, SAGE Publications.

Kelly, K., Richardson, M., Isaacs, T., 2022. Critiquing the rationales for using comparative judgement: a call for clarity. Assessment in Education: Principles, Policy & Practice 29, 674–688. doi:10.1080/0969594X.2022.2147901.

Kim, S., Cohen, A., 1999. Accuracy of parameter estimation in gibbs sampling under the two-parameter logistic model. URL: https://eric.ed.gov/?id=ED430012. annual Meeting of the American Educational Research Association.

Kimbell, R., 2012. Evolving project e-scape for national assessment. International Journal of Technology and Design Education 22, 135–155. doi:10.1007/s10798-011-9190-4.

Kline, R., 2023. Principles and Practice of Structural Equation Modeling. Methodology in the Social Sciences, Guilford Press.

Kohler, U., Kreuter, F., Stuart, E., 2019. Nonprobability sampling and causal analysis. Annual Review of Statistics and Its Application 6, 149–172. URL: https://www.annualreviews.org/content/journals/10.1146/annurev-statistics-030718-104951, doi:https://doi.org/10.1146/annurev-statistics-030718-104951.

Lambert, P., Sutton, A., Burton, P., Abrams, K., Jones, D., 2006. How vague is vague? a simulation study of the impact of the use of vague prior distributions in mcmc using winbugs. Journal of Statistics in Medicine 24, 2401–2428. doi:10.1002/sim.2112.

Laming, D., 2004. Marking university examinations: Some lessons from psychophysics. Psychology Learning & Teaching 3, 89–96. doi:10.2304/plat.2003.3.2.89.

Lawson, J., 2015. Design and Analysis of Experiments with R. Chapman and Hall/CRC.

Lee, Y., Nelder, J.A., 1996. Hierarchical generalized linear models. Journal of the Royal Statistical Society: Series B (Methodological) 58, 619–656. URL: https://rss.onlinelibrary.wiley.com/doi/abs/10.1111/j.2517-6161.1996.tb02105.x, doi:10.1111/j.2517-6161.1996.tb02105.x.

Lesterhuis, M., 2018a. The validity of comparative judgement for assessing text quality: An assessor's perspective. Ph.D. thesis. University of Antwerp. URL: https://hdl.handle.net/10067/1548280151162165141.

Lesterhuis, M., 2018b. When teachers compare argumentative texts: Decisions informed by multiple complex aspects of text quality. L1-Educational Studies in Language and Literature 18, 1–22. doi:10.17239/L1ESLL-2018.18.01.02.

Little, R., Rubin, D., 2020. Statistical analysis with missing data. Wiley Series in Probability and Statistics, John Wiley & Sons. doi:10.1002/9781119482260. third Edition.

Luce, R., 1959. On the possible psychophysical laws. The Psychologcal Review 66, 482–499. doi:10.1037/h0043178.

Marshall, N., Shaw, K., Hunter, J., Jones, I., 2020. Assessment by comparative judgement: An application to secondary statistics and english in new zealand. New Zealand Journal of Educational Studies 55, 49–71. doi:10.1007/s40841-020-00163-3.

Martin, J., McDonald, R., 1975. Bayesian estimation in unrestricted factor analysis: A treatment for heywood cases. Psychometrika , 505–517doi:10.1007/BF02291552.

McCullagh, P., Nelder, J., 1983. Generalized Linear Models. Monographs on Statistics and Applied Probability,

Routledge. doi:10.1201/9780203753736.

McElreath, R., 2020. Statistical Rethinking: A Bayesian Course with Examples in R and STAN. Chapman and Hall/CRC. doi:https://doi.org/10.1201/9780429029608.

McElreath, R., 2021. Science before statistics: Causal inference. https://www.youtube.com/watch?v=KNPYUVmY3NM. Last accessed 30 April 2024.

McElreath, R., 2024. Statistical rethinking, 2024 course. URL: https://github.com/rmcelreath/stat_rethinking_2024. last accessed 15 March 2025.

Mikhailiuk, A., Wilmot, C., Perez-Ortiz, M., Yue, D., Mantiuk, R., 2021. Active sampling for pairwise comparisons via approximate message passing and information gain maximization, in: 2020 25th International Conference on Pattern Recognition (ICPR), pp. 2559–2566. doi:10.1109/ICPR48806.2021.9412676.

Morgan, S., Winship, C., 2014. Counterfactuals and Causal Inference: Methods and Principles for Social Research. Analytical Methods for Social Research. 2 ed., Cambridge University Press.

Morin, C., Black, B., Howard, E., Holmes, S., 2018. A study of hard-to-mark responses: Why is there low mark agreement on some responses? URL: https://assets.publishing.service.gov.uk/media/5bfc0058ed915d1199c8b206/HardtoMark_-_FINAL64495.pdf. ofqual, Marking Roundtable 2018.

Neal, B., 2020. Introduction to causal inference from a machine learning perspective. URL: https://www.bradyneal.com/Introduction_to_Causal_Inference-Dec17_2020-Neal.pdf. last accessed 30 April 2024.

Neyman, J., 1923. On the application of probability theory to agricultural experiments. essay on principles. section 9. Statistical Science 5, 465–472. URL: http://www.jstor.org/stable/2245382. translated by Dabrowska, D. and Speed, T. (1990).

O'Hagan, A., 2018. On outlier rejection phenomena in bayes inference. Journal of the Royal Statistical Society: Series B (Methodological) 41, 358–367. URL: https://academic.oup.com/jrsssb/article-pdf/41/3/358/49097051/jrsssb_41_3_358.pdf, doi:10.1111/j.2517-6161.1979.tb01090.x.

Pearl, J., 2009. Causality: Models, Reasoning and Inference. Cambrige University Press.

Pearl, J., 2010. An introduction to causal inference. The international journal of biostatistics 6, 855–859. URL: https://www.degruyter.com/document/doi/10.2202/1557-4679.1203/html, doi:10.2202/1557-4679.1203.

Pearl, J., 2019. The seven tools of causal inference, with reflections on machine learning. Communications of the ACM 62, 54–60. doi:10.1177/0962280215586010.

Pearl, J., Glymour, M., Jewell, N., 2016. Causal Inference in Statistics: A Primer. John Wiley & Sons, Inc.

Pearl, J., Mackenzie, D., 2018. The Book of Why: The New Science of Cause and Effect. 1st ed., Basic Books, Inc.

Perron, B., Gillespie, D., 2015. Reliability and Measurement Error, in: Key Concepts in Measurement. Oxford University Press. Pocket guides to social work research methods. chapter 4. doi:10.1093/acprof:oso/9780199855483.003.0004.

Pollitt, A., 2004. Let's stop marking exams, in: Proceedings of the IAEA Conference, University of Cambridge Local Examinations Syndicate, Philadelphia. URL: https://www.cambridgeassessment.org.uk/images/109719-let-s-stop-marking-exams.pdf.

Pollitt, A., 2012a. Comparative judgement for assessment. International Journal of Technology and Design Education 22, 157—170. doi:10.1007/s10798-011-9189-x.

Pollitt, A., 2012b. The method of adaptive comparative judgement. Assessment in Education: Principles, Policy and Practice 19, 281—300. doi:10.1080/0969594X.2012.665354.

Pollitt, A., Elliott, G., 2003. Finding a proper role for human judgement in the examination system.

URL: https://www.cambridgeassessment.org.uk/Images/109707-monitoring-and-investigating-comparability-a-proper-role-for-human-judgement.pdf. research & Evaluation Division.

Rohrer, J., 2018. Thinking clearly about correlations and causation: Graphical causal models for observational data. Advances in Methods and Practices in Psychological Science 1, 27–42. doi:10.1177/2515245917745629.

Rubin, D., 1974. Estimating causal effects of treatments in randomized and nonrandomized studies. Journal of Educational Psychology 66, 688–701. doi:10.1037/h0037350.

Schuessler, J., Selb, P., 2023. Graphical causal models for survey inference. Sociological Methods and Research 0. doi:10.1177/00491241231176851.

Seaman III, J., Seaman Jr., J., Stamey, J., 2011. Hidden dangers of specifying noninformative priors. The American Statistician 66, 77–84. doi:10.1080/00031305.2012.695938.

Sekhon, J., 2009. The neyman-rubin model of causal inference and estimation via matching methods, in: Box-Steffensmeier, J., Brady, H., Collier, D. (Eds.), The Oxford Handbook of Political Methodology. Oxford University Press, pp. 271–299. doi:10.1093/oxfordhb/9780199286546.003.0011.

Shaughnessy, J., Zechmeister, E., Zechmeister, J., 2010. Research Methods in Psychology. McGraw-Hill. URL: https://web.archive.org/web/20141015135541/http://www.mhhe.com/socscience/psychology/shaugh/ch01_concepts.html. retrieved July 23, 2024.

Spirtes, P., Glymour, C., Scheines, R., 1991. From probability to causality. Philosophical Studies 64, 1–36. URL: https://www.jstor.org/stable/4320244.

Stan Development Team., 2021. Stan Modeling Language Users Guide and Reference Manual, version 2.26. Vienna, Austria. URL: https://mc-stan.org.

Thurstone, L., 1927a. A law of comparative judgment. Psychological Review 34, 482–499. doi:10.1037/h0070288.

Thurstone, L., 1927b. Psychophysical analysis. American Journal of Psychology , 368–89URL: https://brocku.ca/MeadProject/Thurstone/Thurstone_1927g.html. last accessed 20 december 2024.

van Daal, T., 2020. Making a choice is not easy?!: Unravelling the task difficulty of comparative judgement to assess student work. Ph.D. thesis. University of Antwerp.

van Daal, T., Lesterhuis, M., Coertjens, L., Donche, V., De Maeyer, S., 2016. Validity of comparative judgement to assess academic writing: examining implications of its holistic character and building on a shared consensus. Assessment in Education: Principles, Policy & Practice 26, 59–74. doi:10.1080/0969594X.2016.1253542.

van Daal, T., Lesterhuis, M., Coertjens, L., van de Kamp, M., Donche, V., De Maeyer, S., 2017. The complexity of assessing student work using comparative judgment: The moderating role of decision accuracy. Frontiers in Education 2. doi:10.3389/feduc.2017.00044.

van der Linden, W. (Ed.), 2017a. Handbook of Item Response Theory: Models. volume 1 of *Statistics in the Social and Behavioral Sciences Series*. CRC Press.

van der Linden, W. (Ed.), 2017b. Handbook of Item Response Theory: Statistical Tools. volume 2 of *Statistics in the Social and Behavioral Sciences Series*. CRC Press.

Verhavert, S., Bouwer, R., Donche, V., De Maeyer, S., 2019. A meta-analysis on the reliability of comparative judgement. Assessment in Education: Principles, Policy and Practice 26, 541–562. doi:10.1080/0969594X.2019.1602027.

Verhavert, S., Furlong, A., Bouwer, R., 2022. The accuracy and efficiency of a reference-based adaptive selection algorithm for comparative judgment. Frontiers in Education 6. doi:10.3389/feduc.2021.785919.

Whitehouse, C., 2012. Testing the validity of judgements about geography essays using the adaptive comparative judgement method. URL: https://filestore.aqa.org.uk/content/research/CERP_RP_CW_24102012_0.pdf?download=1.

aQA Education.

Wu, W., Niezink, N., Junker, B., 2022. A diagnostic framework for the bradley–terry model. Journal of the Royal Statistical Society Series A: Statistics in Society 185, S461–S484. URL: https://academic.oup.com/jrsssa/article-pdf/185/Supplement_2/S461/49421054/jrsssa_185_supplement_2_s461.pdf, doi:10.1111/rssa.12959.

Zimmerman, D., 1994. A note on the influence of outliers on parametric and nonparametric tests. The Journal of General Psychology 121, 391–401. doi:10.1080/00221309.1994.9921213.