Juror Decision-Making in Rape Cases: The Influence of Victim Mental Health and Rape Myth Acceptance

Kiah Bouillaut

Candidate Registration Number: 100458844

Primary Supervisor: Dr Peter Beazley

Secondary Supervisor: Dr Lucy Fitton

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Thesis Portfolio Abstract

Background: Juror decisions in rape cases are shaped by extra-legal beliefs such as rape myth acceptance (RMA) and stigma towards people with severe mental illness. Although RMA is consistently linked with greater victim blame, up-to-date evidence on its association with juror decision-making has not been synthesised, and little is known about how victims with a serious mental health condition are evaluated, particularly within a contemporary cultural climate of public debate about sexual violence (for example, #MeToo).

Method: A systematic review and three-level meta-analysis updated Dinos et al. (2015), identifying post-2012 mock-juror studies that measured RMA in relation to a rape or sexual assault scenario and at least one legally relevant outcome (k = 20; N = 7,682). The empirical paper partially replicated Levi and Golding (2024) in an England and Wales context using a 3 (medication-cued victim mental health condition: borderline personality disorder, schizophrenia, no condition) × 2 (participant gender) design (N = 130). Participants read an acquaintance-rape vignette framed under the Sexual Offences Act 2003, returned a verdict and ratings of the victim and defendant, and completed a measure of mental health stigma and related attitudes.

Results: Across studies in the systematic review, higher RMA was associated with lower guilt judgements, and to a greater extent, higher victim blame. In the empirical study, a brief mental health cue increased perceived mental illness severity for the schizophrenia condition, but victim mental health condition did not reliably predict verdicts, guilt ratings, or most victim evaluations. Instead, more right-leaning political orientation, participant gender, and to a lesser extent, higher mental health stigma, were more strongly related to victim- and defendant-focused ratings.

Conclusions: Overall, the systematic review showed rape myth acceptance remains associated with more defendant-favourable outcomes and more victim-blaming judgements, while the empirical study suggested juror responses were more strongly organised around broader attitudinal frameworks (stigma, political orientation, gender) than around brief mental-health cues about the victim. Implications for clinical and legal practice, policy, and future research on managing rape myths and mental health information in rape trials are discussed.

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Table of Contents

Chapter One: General Introduction 9
Chapter Two: Systematic Review 12
Abstract 14
Introduction 15
Method 18
Results 24
Discussion 34
References 39
Chapter Three: Bridging Chapter 47
Chapter Four: Empirical Study 49
Abstract 51
Introduction 52
Method 59
Results 67
Discussion 73
References 82
Chapter Five: Discussion and Critical Evaluation 93
Thesis Portfolio References 105
Appendices 111

List of Tables

Chapter Two: Systematic Review

Table 1. Summary of reviewed studies and participant characteristics

Table 2. Output of Meta-Analyses of Guilt, Including Sample and Location Subgroup Analysis, and Blame

Chapter Four: Empirical Study

Table 3. Means (standard deviations) of dependent variables for each independent variable.

Table 4. Frequencies and percentages of coded reasons in not-guilty verdict explanations

List of Figures

Chapter Two: Systematic Review

Figure 1. PRISMA Study Selection Flowchart

Figure 2. Forest Plot of the Association Between Rape Myth Acceptance and Guilt by Study

Figure 3. Forest Plot of the Association Between Rape Myth Acceptance and Blame by Study

Figure 4. Funnel Plot of Guilt Meta-Analysis

List of Appendices

Appendix A: Internation Journal of Law, Crime and Justice

Appendix B: Psychology, Crime and Law

Appendix C: Final sample demographic characteristics

Appendix D: Trial Summary

Appendix E: Attitudes Towards Persons with Mental Illness (APWMI) scale

Appendix F: Participant Information Sheet

Appendix G: Participant Consent Form

Appendix H: Demographic Questions

Appendix I: Participant Debrief Sheet

Appendix J: UEA FMH Ethical Approval

Appendix K: Regression model outputs for victim and perpetrator outcomes

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CHAPTER ONE:
INTRODUCTION TO THESIS PORTFOLIO

General Introduction

Rape and sexual assault continue to present significant challenges for the criminal justice system. Despite legislative reforms and sustained policy attention, conviction rates for rape remain low, and there is ongoing concern about whether jury trials adequately protect complainants from unfairly biased evaluations (Gray & Horvath, 2018; Hudspith et al., 2024). While jurors are instructed to base their decisions solely on the evidence and legal directions provided in court, research in psychology and law indicates that verdicts are also shaped by extra-legal beliefs and assumptions brought into the courtroom, including attitudes towards gender, sexuality, and responsibility (Bornstein & Greene, 2011; Devine & Caughlin, 2014).

These issues are particularly salient in rape cases, where trials often hinge on contested accounts of consent and credibility rather than undisputed factual evidence. In this context, culturally shared beliefs about rape and victim behaviour can become central to how jurors interpret the same set of case facts. Rape myth acceptance (RMA) – endorsement of false, stereotyped beliefs about rape, victims, and perpetrators – has been linked with more defendant-favourable outcomes, higher victim blame, and lower perceived credibility of complainants in mock-juror research (Dinos et al., 2015; Leverick, 2020). Qualitative and observational work suggests that such beliefs can also be traced in the language of trials and deliberations, shaping how complainants' actions before and after an alleged assault are framed and evaluated (Ellison & Munro, 2010; Temkin et al., 2016).

Stigma towards people with severe mental illness (SMI) represents an additional attitudinal factor that may influence juror decision-making. Several hundred thousand adults in England are recorded on primary care registers as having an SMI diagnosis, such as schizophrenia or a personality disorder (Grigoroglou et al., 2020), and these groups experience elevated rates of victimisation, including domestic and sexual violence (Henderson et al., 2020; Khalifeh et al., 2015a). Diagnostic labels can activate stereotypes of dangerousness, unpredictability, or unreliability, with implications for how individuals are judged in legal settings (Angermeyer & Matschinger, 2003; Baker et al., 2022). However, empirical work on how jurors evaluate rape complainants with mental health conditions remains limited. Recent experimental evidence indicates that victim mental health status can affect credibility, sympathy, and verdicts in rape trials (Levi & Golding, 2024), but the extent and conditions of these effects, particularly in the context of an English and Welsh legal framework, are not well established.

The present thesis addresses these gaps by examining how rape myth acceptance and stigma towards people with SMI are associated with mock-juror decision-making in rape cases.

Clinical and Applied Relevance

These questions are of direct relevance to clinical psychology practice. People with SMI who have experienced sexual violence are likely to be in contact with mental health services, and may seek support around disclosure, reporting decisions, and involvement in criminal proceedings (Henderson et al., 2020; Khalifeh et al., 2015a, 2015b). Clinical psychologists may be involved in assessment, formulation, and therapy, as well as in preparing clients for court or contributing expert opinion about mental health conditions, trauma, and their interaction with offending or victimisation. Understanding how rape myths and mental health stigma might influence jurors' evaluations of a complainant can inform clinical formulations, psychoeducation, and collaborative decision-making about the potential risks and benefits of disclosing diagnostic labels in court.

At a systems level, clarifying the impact of these attitudinal constructs can contribute to debates about juror education, judicial directions, and the ethical use of diagnostic information in legal settings. It can also highlight areas where additional safeguards, guidance, or advocacy may be needed to reduce the risk that victims with SMI are unfairly disadvantaged.

Overview of the Thesis Portfolio

The present thesis examines these questions through two complementary projects. The systematic review and meta-analysis synthesise post-2012 mock-juror studies that have measured rape myth acceptance or related attitudes in the context of sexual violence cases. It quantifies the association between RMA and key juror decision-making outcomes (e.g., verdicts, guilt ratings, and blame judgments) and explores how effect sizes vary according to sample and study characteristics. In doing so, it updates and extends earlier work in this area (e.g. Dinos et al., 2015) and provides a contemporary evidence base on the role of rape myths in juror decision-making.

The empirical project focuses on mental health stigma and victim diagnosis in rape trials. It partially replicates and extends a recent United States mock-juror study on victim mental health and legal decision-making (Levi & Golding, 2024) by adapting the paradigm to the legal and cultural context of England and Wales. Using a jury-eligible sample and trial materials framed under the Sexual Offences Act 2003, the study manipulates a rape complainant's mental health status (schizophrenia, borderline personality disorder, or no mental health

condition) within a trial summary. It examines how mental health information can influence verdicts and perceptions of the complainant and defendant, and tests whether individual differences in mental health stigma help to explain these effects.

A bridging chapter links the two projects by drawing together insights from the rape myth literature and contextualising the empirical study within broader debates about stigma, credibility, and mental health in the courtroom. A final discussion chapter critically evaluates the portfolio as a whole, considers implications for clinical practice and legal policy, and identifies priorities for future research.

Overall Aims

The overarching aims of the thesis are to:

- clarify how rape myth acceptance is associated with mock-juror decision-making in contemporary sexual violence research;
- investigate whether, and in what ways, victim mental health information can influence juror evaluations in a rape trial; and
- consider the implications of these findings for clinical psychologists and other professionals working with rape victims with SMI who engage with the criminal justice system.

By integrating evidence on rape myths and mental health stigma, and by focusing on the specific context of rape cases, the thesis seeks to contribute to a more nuanced, empirically grounded understanding of extra-legal influences on juror decision-making and to inform efforts to reduce unfair bias in rape trials.

CHAPTER TWO:

SYSTEMATIC REVIEW

Prepared for submission to Internation Journal of Law, Crime and Justice.

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Rape Myths and Juror Decision-Making in Sexual Assault Cases: An Updated Systematic Review and Meta-Analysis

Kiah Bouillaut , William Hinton, Lucy Fitton & Peter Beazley

Department of Clinical Psychology and Psychological Therapies, Norwich Medical School, University of East Anglia, Norwich, United Kingdom

*Corresponding author: k.bouillaut@uea.ac.uk

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Abstract

Despite legal reforms and increased public attention to sexual violence, conviction rates for rape remain low and concerns persist about the role of rape myths in juror decision-making. An earlier meta-analysis by Dinos et al. (2015) suggested that higher rape myth acceptance (RMA) is associated with more defendant-favourable outcomes but was based largely on pre-2012 studies. This systematic review and meta-analysis updates that work in a contemporary context. Peer-reviewed studies published between 2012 and 2025 were eligible if adult mock jurors evaluated a rape or sexual assault case, completed a measure of RMA or closely related attitudes, and provided at least one legally relevant decision outcome (e.g. verdict, guilt rating, victim blame). Twenty studies (N = 7,682) met inclusion criteria. Three-level random-effects models pooled Pearson's r for guilt-related and victim-blame outcomes. Higher RMA was associated with lower guilt judgements (r = −.16) and higher victim blame (r = .52). Heterogeneity was substantial for both outcomes and was not explained by sample type or broad study location. These findings suggest that rape myths remain present in how complainants are evaluated, but that their impact on verdicts is smaller and more context-dependent than earlier work implied.

Key Words: rape myths; rape myth acceptance; juror decision-making; mock jurors; sexual assault; meta-analysis; systematic review

Introduction

Sexual violence remains a pervasive global public health and human rights problem. Estimates from the World Health Organization suggest that around one in three women worldwide experience physical and/or sexual violence in their lifetime (World Health Organization, 2025). In England and Wales, recent Crime Survey data indicated that approximately 2.4% of adults aged 16-59 experienced sexual assault in the year ending March 2025, an increase relative to a decade earlier (Office for National Statistics [ONS], 2025a).

Police-recorded sexual offences have reached record highs, with over 209,079 recorded in the year ending March 2025 (ONS, 2025b). Yet, only a small fraction of sexual offences progress through the criminal justice system: fewer than 3 in 100 rapes recorded by police resulted in a charge in the same year (Rape Crisis England & Wales, n.d.). This 'justice gap' has prompted extensive scrutiny of how rape is processed at each stage of the criminal justice system, including the role of jurors' attitudes and beliefs in shaping trial outcomes (Gray & Horvath, 2018; Hudspith et al., 2024).

Rape Myths and Rape Myth Acceptance (RMA)

A potential barrier to reporting sexual violence, and to decisions about charging and prosecution within the criminal justice system, is the influence of 'rape myths'. These were first discussed in early feminist work on sexual violence (e.g., Brownmiller, 1975) and later formally defined as "prejudicial, stereotyped, or false beliefs about rape, rape victims, and rapists" (Burt, 1980, p. 217). Subsequent definitions have emphasised their justificatory function as attitudes and beliefs that are generally false yet widely held, which serve to deny and justify male sexual aggression against women (Lonsway & Fitzgerald, 1994). Building on this work, Gerger et al. (2007) described rape myths as descriptive or prescriptive beliefs about the scope, causes, context, and consequences of sexual aggression that downplay or legitimise sexually aggressive behaviour, while Bohner et al. (2009) and Temkin and Krahé (2008) conceptualise them as cognitive schemas that structure how people understand rape and evaluate specific cases.

Rape myths commonly include ideas that 'real' rape is typically perpetrated by a stranger in a public place, involves physical violence and visible injuries, and is immediately reported; that women who drink alcohol, wear revealing clothing, or invite someone home are partly to blame; or that false allegations are common (Gray & Horvath, 2018; Hudspith et al., 2024; Krahé, 2025). These beliefs do not align with legal definitions of rape or the empirical reality of sexual offending; for instance, most rapes are committed by someone known to the

victim (Gray & Horvath, 2018; ONS, 2025), and of course the choice of dress or decision to drink alcohol is in no way a defence to rape in law. A large body of work has documented how RMA varies across demographic groups (e.g., higher among men, those with more traditional gender role attitudes, and people endorsing other prejudicial beliefs) and is associated with greater victim blame, lower perpetrator blame, and the minimisation of sexual violence (Hogge & Wang, 2022; Gray & Horvath, 2018; Hudspith et al., 2024; Suarez & Gadalla, 2010).

Rape Myths and Juror Decision-Making

Rape myths are not confined to the general public; they have been identified at multiple points in the criminal justice process, including among police officers, prosecutors, judges, jurors, and even victims themselves (Gray & Horvath, 2018; Hudspith et al., 2024; Temkin & Krahé, 2008). Qualitative and observational studies of rape trials in England and Wales show that rape myths continue to surface in courtroom narratives, shaping how complainants' behaviour is framed (e.g., alcohol use, delayed reporting, emotional presentation) and how 'reasonable belief in consent' – a key defence in law – is interpreted (Finch & Munro, 2005; Gray, 2015; Temkin et al., 2016). For example, intoxicated complainants are often judged more harshly and attributed greater responsibility, whereas intoxicated perpetrators are sometimes portrayed as having misread sexual signals (Gray & Horvath, 2018; Gunby et al., 2013).

Within this broader context, mock-juror research has been used to isolate how rape-relevant attitudes influence verdicts. Experimental vignette and mock-trial studies repeatedly find that higher RMA is associated with perceiving complainants as less credible and more responsible, perceiving defendants more favourably, and being less likely to convict (e.g., Dinos et al., 2015; Gray, 2006; Lilley et al., 2023). Rape myths also appear to function as interpretive schemas during deliberation; even participants who score low on standard RMA questionnaires may reproduce myth-based reasoning when discussing cases in a group (Ellison & Munro, 2010; Leverick, 2020).

Dinos et al.'s (2015) systematic review and meta-analysis provided early quantitative synthesis of this evidence base. Across nine correlational mock-juror studies using female complainants and Western samples, they reported a significant positive correlation between RMA scores and leniency towards defendants (i.e., lower guilt ratings or not-guilty verdicts), an effect that was robust to study quality and sample type (student vs. community). Since that review, the evidence base has grown substantially, including studies with more diverse samples, expanded outcome measures (e.g., credibility, sympathy, anger, memory), and more nuanced manipulations of case characteristics (e.g., relationship context, intoxication, delayed

reporting, complainant mental health). These developments raise the possibility that the relationship between RMA and juror decision-making may now be more complex than the pattern reported by Dinos et al. (2015).

Alongside empirical work, there is ongoing debate about how widespread rape myths are among jurors and how strongly they influence decisions in practice. Survey research with real jurors in England and Wales has been interpreted by some as suggesting relatively low explicit endorsement of traditional rape myths (Thomas, 2020). Others have challenged both this interpretation and the methods used, arguing that myth-based reasoning still surfaces in how complainants and defendants are discussed and evaluated (Chalmers et al., 2021; Daly et al., 2023; Leverick, 2023). Updating Dinos et al.'s (2015) meta-analysis in this newer context offers a way to test whether earlier conclusions about the impact of RMA on juror decision-making still hold.

Recent developments and the need for an updated review

Recent work has also examined whether targeted education can reduce rape-myth-driven bias. Hudspith et al. (2024) conducted a systematic review of mock-juror studies that explicitly evaluated rape myth education (e.g., expert testimony, judicial directions, complainant statements) and found mixed but promising evidence that such interventions can shift some attitudes and, in certain conditions, affect verdicts. However, they also noted significant limitations in internal and external validity and did not meta-analyse effect sizes. Legal and policy scholarship has similarly called for better evidence on how juror bias operates in contemporary rape trials and how it might be countered (Gray & Horvath, 2018; Graw Leary, 2020; Leverick, 2020). These debates have unfolded following the #MeToo movement, which has drawn unprecedented public attention to sexual violence, but whose impact on juror attitudes and decision-making remains uncertain (Krahé, 2025; Graw Leary, 2020).

Considered altogether, the landscape since Dinos et al. (2015) is marked by: (a) an expanded and more methodologically diverse mock-juror literature on rape myths and decision-making; (b) ongoing controversy about the extent of rape myth acceptance among jurors; and (c) emerging experimental work on rape myth education interventions. Yet there has been no recent quantitative synthesis of the post-2012 mock-juror literature specifically examining statistical associations between RMA and juror decisions in rape cases. Existing reviews either pre-date major social shifts (e.g., #MeToo), focus thematically on education without meta-analysis (Hudspith et al., 2024; Leverick, 2020), or combine rape myths with broader attitudinal constructs (Hudspith et al., 2023; Suarez & Gadalla, 2010).

Aims of the review:

The present meta-analytic review aimed to update Dinos et al. (2015) by systematically identifying empirical studies from 2012 onwards that examine rape myth acceptance and mock-juror decision-making in rape cases, quantifying the overall association between RMA and key decision outcomes, and exploring whether effect sizes vary as a function of study and sample characteristics. In doing so, it aims to provide an up-to-date evidence base to inform legal practice, juror education initiatives, and future research on rape myths in the courtroom.

Research questions:

1. To what extent do rape myth acceptance and related attitudes predict juror decision-making outcomes (e.g. verdicts, guilt ratings, victim culpability/blame) in sexual violence cases in studies published from 2012 onwards?
2. What are the key methodological and sample characteristics of post-2012 studies examining rape myth acceptance and juror decision-making in sexual violence cases?

Method

This systematic review and meta-analysis was prospectively registered on the Open Science Framework (OSF) on 23 February 2025 and subsequently updated on 21 February 2026 to reflect the final search, eligibility, and analysis procedures (registration ID: zj3wm).

Eligibility

Studies were included if they (a) investigated decision-making in the context of a rape or sexual assault case, where participants were required to reach a legally relevant judgement (e.g. a verdict of guilt, a continuous guilt rating, or ratings of victim or defendant blame, responsibility, or credibility); (b) employed a measure of rape myth acceptance, sexual assault myths, or closely related attitudes (for example, validated scales such as the RMAS or IRMA); (c) recruited adult participants (aged 18 years and over) in a lay or mock-juror role, including members of the general public, jury-eligible community samples, or university students asked to act as jurors; (d) reported primary quantitative data, or mixed-methods data where a quantitative component could be extracted, with sufficient information to derive an effect size for the association between rape myths (or related attitudes) and at least one decision-making outcome; and (e) were written in English, published in peer-reviewed academic journals between 1st January 2012 and 31st October 2025, and conducted in any geographical region.

Studies were excluded if they did not meet these criteria, or if they (a) did not involve a rape or sexual assault scenario (e.g. general criminal offences); (b) assessed attitudes to rape or sexual violence only, without asking participants to reach a verdict or make case-specific judgements; (c) did not include a rape myth, sexual assault myth, or closely related attitudinal measure; (d) reported qualitative data only, or did not provide sufficient quantitative information to calculate an effect size; (e) recruited children or adolescents, or used mixed samples in which an adult subsample could not be isolated; or (f) were not peer-reviewed or not available in full text (e.g. theses, dissertations, book chapters, conference abstracts, reports, or websites), or were not written in English. Studies that focused exclusively on professional groups such as judges, police officers, or lawyers were also excluded unless data for lay or mock-juror participants were reported separately.

Search strategy

An initial scoping search was undertaken in January 2025 to identify terminology commonly used to describe rape myths, sexual assault myths, and juror decision-making in the literature, and to refine the search concepts used in Dinos et al. (2015). The finalised search string was: (“rape myth*” OR “sexual assault myth*” OR “victim blaming” OR “false beliefs about rape”) AND (“juror*” OR “jury decision-making” OR “mock juror*”) AND (“verdict” OR “guilt attribution” OR “blame” OR “credibility”).

Electronic searches were conducted via the University of East Anglia’s EBSCOhost platform, using the database filter for APA PsycINFO, Science Direct and MEDLINE Ultimate. Searches were limited to publications from 1st January 2012 onwards to update the time period covered by Dinos et al. (2015) and to focus on contemporary work conducted in a changed social and legal context.

To identify additional relevant studies and reduce the risk of missing eligible articles due to variation in terminology, a supplementary search was conducted in Google Scholar in January 2025, restricted to 2012-2025 and sorted by relevance. The first 50 records were screened at title and abstract level, and forward citation tracking was used for key papers. An updated Google Scholar search was carried out in September 2025 (restricted to items published since 2025) to capture newly published studies, and any additional potentially eligible records were imported into Rayyan for full-text screening. Reference lists of all included studies, and of the Dinos et al. (2015) review, were also hand-searched to identify further eligible articles not retrieved through database searches.

Identification and selection of studies

The search strategy outlined above was used across the selected databases and Google Scholar. All records were exported to EndNote and transferred into Rayyan for screening. In total, 101 records were imported into Rayyan from database searches, Google Scholar, and citation searching. After removing 18 duplicates, 83 records were screened at title and abstract level against the eligibility criteria. Of these, 48 articles were retrieved in full and screened in detail. Screening was carried out by the primary researcher. To check consistency, a second reviewer independently screened a random 25% of titles/abstracts and 25% of full-text articles. Agreement was 98% at title/abstract stage; any disagreements were discussed and resolved. Agreement at full-text stage was 100%.

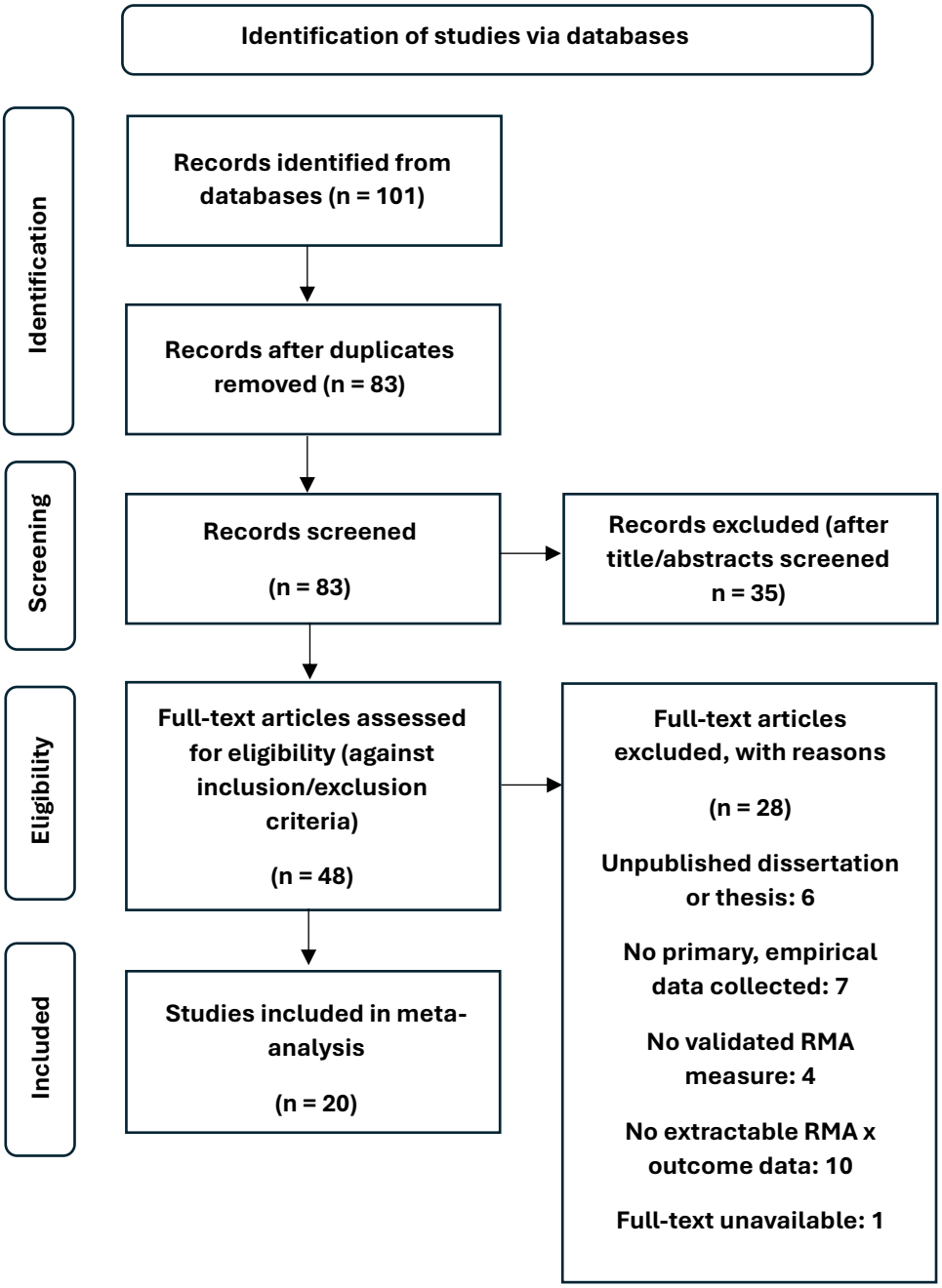
Studies included in the review

Of the 48 full-text articles screened, 28 met the inclusion criteria at full-text stage. During data extraction, a further eight studies were excluded because they did not report an analysable association between rape myth acceptance and a juror decision-making outcome or did not provide enough statistical information to calculate an effect size. This resulted in 20 studies being included in the meta-analysis.

The review initially aimed to include grey literature (e.g. theses and dissertations). However, once it became clear that there were sufficient eligible peer-reviewed journal articles, the scope was narrowed to published, peer-reviewed studies for feasibility and consistency. The OSF protocol was updated to reflect this refinement. A PRISMA flow diagram (Figure 1) summarises the identification, screening, and inclusion process.

Figure 1.

PRISMA Study Selection Flowchart



Data extraction

Data were extracted by the primary researcher using a standardised Excel form developed for this review. For each eligible study, information on study characteristics (authors, year, country, design, procedure, and any experimental manipulations), sample characteristics (total sample size, mock-juror status, student vs community samples, and available age and gender data), measures of rape myth acceptance or related attitudes, and juror decision-making outcomes was extracted.

Across the 20 included studies, decision-making outcomes included verdict-related judgements (e.g., guilty vs not guilty, continuous guilt ratings, confidence in guilt) and case-specific evaluations (e.g., victim blame, responsibility, or culpability; perceptions of victim credibility, sympathy and anger; and corresponding evaluations of the defendant). To maintain conceptual coherence and comparability with Dinos et al. (2015), the meta-analyses focused on two outcome domains: guilt and victim blame. The guilt outcome included binary verdicts and continuous guilt or confidence-in-guilt ratings, treated as indicators of the same underlying construct. The victim blame outcome included ratings of the complainant's blame, responsibility, or culpability for the assault. Other outcomes (e.g., credibility, sympathy, anger, defendant-focused ratings) were judged too heterogeneous in how they were measured and reported to pool meaningfully and were not meta-analysed (Lensen, 2023).

For the meta-analysis, the specific dependent variable selected for each study, the sample size contributing to that effect, and the statistics required to calculate an effect size were extracted. The extraction sheet also recorded whether effects supported authors' hypotheses and the quality rating assigned to each study. Extracted data were checked against the original articles to minimise transcription errors. The full data extraction table, including all variables and effect size calculations, is available in the online repository associated with this project (https://osf.io/qw79f/files/wygx8)

Methodological quality assessment

The quality of each included study was assessed using the Appraisal Tool for Cross-Sectional Studies (AXIS; Downes et al., 2016). The AXIS includes 20 items for appraising quantitative cross-sectional research, covering clarity of aims, sampling, measurement, bias, and analysis. For each study, items were rated as yes, no, or unclear.

Although AXIS does not prescribe a total score, previous reviews have summed the number of criteria met as a simple indicator of quality (e.g., Tremlin & Beazley, 2022; Wong et

al., 2018), and the same approach was used here. Three items (7, 13, and 14), which relate to the classification and analysis of non-responders, were not applicable to mock-juror vignette studies using self-selecting, opt-in samples. These items were therefore excluded from scoring, giving a maximum possible score of 17, with higher scores indicating better-reported methodological quality.

Analysis

Effect size calculation

The primary effect size was Pearson's r representing the association between rape myth acceptance (or closely related rape-supportive attitudes) and each eligible outcome. Where studies reported r directly, this value was extracted; otherwise, reported statistics (e.g., t, F, χ^2, odds ratios, regression coefficients) were converted to r using standard formulae. All effect sizes were transformed to Fisher's *z* for analysis and then converted back to *r* for interpretation, as is standard practice to stabilise variance and normalise the sampling distribution of correlation coefficients (Fisher, 1921; Hafdahl & Williams, 2009). All analyses were conducted in RStudio (version 2025.05.1+513) using the metafor package (Viechtbauer, 2010).

Meta-analytic models and heterogeneity

Because several studies contributed multiple effect sizes, a three-level random-effects meta-analysis was used to account for statistical dependence (Assink & Wibbelink, 2016; Van den Noortgate et al., 2013). Models were fitted using the rma.mv function in metafor, with sampling variance at level 1, effect sizes at level 2, and studies at level 3. Random-effects were specified at the within- and between-study levels to model expected heterogeneity in true effects across samples and study designs, rather than assuming a single common effect size.

Heterogeneity was summarised using the residual *QE* statistic, which represents the remaining heterogeneity in the three-level models after any moderators are included and is appropriate when effect sizes are nested within studies. This was preferred to the traditional Cochran's *Q* statistic, which in standard single-level meta-analyses assumes independent effect sizes and yields only a single test of total heterogeneity across studies (Higgins & Thompson, 2002).

For the guilt outcome, where there were sufficient studies, planned moderator analyses examined sample type (student vs community/jury-eligible) and location (North America, Europe, Australia) using three-level mixed-effects meta-regression. These analyses were only conducted where at least 10 studies contributed to an outcome, given concerns about low

statistical power and unstable estimates in smaller meta-analyses (Mathur & VanderWeele, 2020). Pooled estimates are reported as *r*, with forest plots displaying effects on the Fisher's *z* scale.

Results

Study Characteristics

Location and samples

The 20 included studies were conducted across five countries: Canada (*k* = 8), the United Kingdom (*k* = 5), the United States (*k* = 4), Australia (*k* = 2), and Spain (*k* = 1). Most studies recruited student samples (*k* = 14), typically undergraduate psychology students, while six studies used community or jury-eligible samples recruited through panels, online platforms or local advertising. Across all studies, the combined sample size was *N* = 7,682. Mean ages, where reported, ranged from late teens to mid-40s, and samples were generally majority female, with the proportion of women usually between 50% and 75%. All studies used rape or sexual assault scenarios involving a female complainant and a male defendant, although the relationship between the parties and other case details (e.g. acquaintance vs stranger, alcohol use, delayed reporting) varied across studies.

Quality assessment

Total AXIS scores ranged from 13 to 17 (*M* = 15), indicating generally moderate-to-high study quality. Only one study achieved the maximum score of 17 (Belyea & Blais, 2023). Most studies had clear aims, appropriate designs, and adequately described measures and analyses. However, sample sizes were rarely justified, and many relied on convenience samples that did not closely reflect jury-eligible populations, leading to frequent "no" ratings on the items concerning sample frame and selection process. Ethical approval was reported in most studies, with only three failing to provide this information.

Table 1.

Summary of reviewed studies and participant characteristics

Authors	Study ID	Study location	Sample size used in analysis	Sample type	RMA measure used	Case type/procedure	Guilt outcome	Blame outcome	Participant characteristics		Quality of study
									Mean age (SD)	Gender (% female)	
Anderson et al., 2018 https://doi.org/10.1177/0886260515618214		USA	541	Student	Burt's RMA Scale	Vignette	✓	✓	19.8 (3.67)	70%	14
Angelone et al., 2018 https://doi.org/10.1177/0886260516635318		USA	504	Student	IRMA	Vignette		✓	19.5 (2.6)	50.6%	16
Belyea & Blais, 2023 https://doi.org/10.1080/1068316X.2021.2018440		Canada	156	Community	uIRMA	PTP + Trial transcript		✓	44.5 (17)	50.3%	17
Curley et al., 2024 https://doi.org/10.3390/bs14070619		UK	180	Community	AMMSA	Video	✓		42.70 (13.56)	47%	16
Dawtry et al., 2019 https://doi.org/10.1177/0146167218818475	1	UK	255	Community	IRMA	Vignette		✓	36.89 (11.66)	43.5%	14
	2		176	Community	IRMA	Vignette + Crime scene photo		✓	37.69 (13.23)	55%	
Ellingwood et al., 2023 https://doi.org/10.1007/s11896-022-09533-7		Canada	375	Student	IRMA	Vignette	✓		21.88 (5.45)	73.8%	14
Fraser et al., 2025 https://doi.org/10.1002/bsl.2725	1	Canada	436	Student	AMMSA	Trial transcript	✓		20.55 (5.13)	73.3%	16
	2		322	Student	AMMSA	Trial transcript	✓		19 (5.67)	67.1%	
Fraser & Pozzulo, 2025 https://doi.org/10.1177/08862605251381848		Canada	736	Student	uIRMA	Vignette	✓		19.98 (4.06)	68.5%	16

Authors	Study ID	Study location	Sample size used in analysis	Sample type	RMA measure used	Case type/procedure	Guilt outcome	Blame outcome	Participant characteristics		Quality of study
									Mean age (SD)	Gender (% female)	
Grella & Burd, 2025 https://doi.org/10.1111/asap.70017	2	USA	545	Community	uIRMA	Mock trial materials	✓	✓	41.76 (13.51)	53.4%	14
Klement et al., 2019 https://doi.org/10.1007/s11199-018-0950-4	1	USA	97	Student	uIRMA	Vignette	✓	✓	22.43 (3.95)	65%	15
	2		84	Student	uIRMA	Vignette	✓	✓	22.43 (3.14)	73%	
	3		98	Student	MRMS	Vignette		✓	20.25 (2.19)	58%	
	4		116	Student	MRMS	Vignette		✓	19.91 (1.73)	66%	
Lilley et al., 2023 https://doi.org/10.3389/fpsyt.2022.1086026		UK	108	Student	AMMSA	Mock trial video	✓		23.90 (13.83)	59.3%	14
McIntosh & Davis, 2022 https://doi.org/10.1177/0886260520966679		UK	449	Community	AMMSA	Video testimony	✓		18-75	63.5%	14
Nitschke et al., 2021 https://doi.org/10.1177/0886260518790601		Australia	212	Student	uIRMA	Trial synopsis + transcript	✓	✓	20.80 (4.20)	51.9%	14
Parsons & Mojtahedi, 2022 https://doi.org/10.1016/j.ijlp.2022.101837		UK	196	Student	AMMSA	Vignette	✓		20.73 (5.15)	70%	14
Pica et al., 2020 https://doi.org/10.1177/0886260517713715		Canada	489	Student	IRMAS	Trial transcript	✓		20.29 (4.61)	72.8%	13
Pica et al., 2021 https://doi.org/10.1177/0886260518800316		Canada	503	Student	IRMAS	Trial transcript	✓		20 (4.15)	73%	14
Pica et al., 2024 https://doi.org/10.1007/s11896-024-09717-3	1	Canada	278	Student	IRMAS	Vignette	✓		20.14 (4.38)	76.6%	14

Authors	Study ID	Study location	Sample size used in analysis	Sample type	RMA measure used	Case type/procedure	Guilt outcome	Blame outcome	Participant characteristics		Quality of study
									Mean age (SD)	Gender (% female)	
	2		553	Student	AMMSA	Trial transcript	✓		20.73 (5.15)	70%	
Romero-Sanchez et al., 2018 https://doi.org/10.1177/1077801217727372	1	Spain	137	Student	Spanish AMMSA	Vignette		✓	21.1	47.4%	15
	2		133	Student	Spanish AMMSA	Vignette		✓	21.22 (2.52)	53.4%	
Thompson & Pozzulo, 2024 https://doi.org/10.1007/s11896-024-09664-z		Canada	709	Student	AMMSA	Trial transcript	✓		20.36 (4.40)	71.7%	15
Westera et al., 2015 https://doi.org/10.1002/acp.3146	1	Australia	96	Community	Costin	Vignette + mock trial summary	✓		42.76 (14.70)	48%	13
	2		96	Community	Costin	Vignette + mock trial summary	✓		44.02 (15.74)	51%	
	3		102	Community	Costin	Vignette + mock trial summary	✓		44.3 (15.45)	53%	
	4		102	Community	Costin	Vignette + mock trial summary	✓		45.85 (13.55)	46%	

Note. PTP = pre-trial publicity; RMA = rape myth acceptance; AMMSA = Acceptance of Modern Myths about Sexual Aggression; IRMA / IRMAS / uIRMA = Illinois Rape Myth Acceptance Scale and updated versions; MRMS = Male Rape Myth Scale. "Study ID" refers to the specific study within multi-study articles that contributed at least one eligible effect size to the meta-analysis. For example, if an article reported four studies but only Studies 2 and 4 met inclusion criteria, these are listed separately with Study IDs 2 and 4. A tick (✓) in the "Included in guilt meta-analysis" or "Included in victim blame meta-analysis" column indicates that the study contributed at least one effect size to that model. Where an article reported multiple studies, the quality rating refers to the article as a whole. Quality ratings reflect methodological quality scores on a 0-17 scale (higher scores = better quality).

Measures of rape myth acceptance and related attitudes

All studies used a self-report measure of rape myth acceptance or closely related attitudes. The most frequently used were the Illinois Rape Myth Acceptance Scale (IRMA/IRMAS; Payne et al., 1999) (k = 6) and the Acceptance of Modern Myths about Sexual Aggression scale (AMMSA; Gerger et al., 2007), including a Spanish adaptation (k = 7). Five studies used the Updated Illinois Rape Myth Acceptance Scale (uIRMA) (McIntosh & Davis, 2022; McMahon & Farmer, 2011). Two studies used older or less commonly employed measures: Costin's Rape Myth Acceptance Scale (Costin, 1985) and Burt's Rape Myth Acceptance Scale (Burt, 1980). One study used the Male Rape Myths Scale (MRMS) (Melanson, 1999).

In most studies, rape myth measures were scored as a total scale, although a subset also reported subscale scores (e.g., "she asked for it", "it wasn't really rape"). Several studies additionally assessed victim-specific blame or responsibility items, attitudes toward victims of sexual assault, or broader gender-related attitudes, but these were not entered separately into the meta-analyses.

Meta-analytic findings

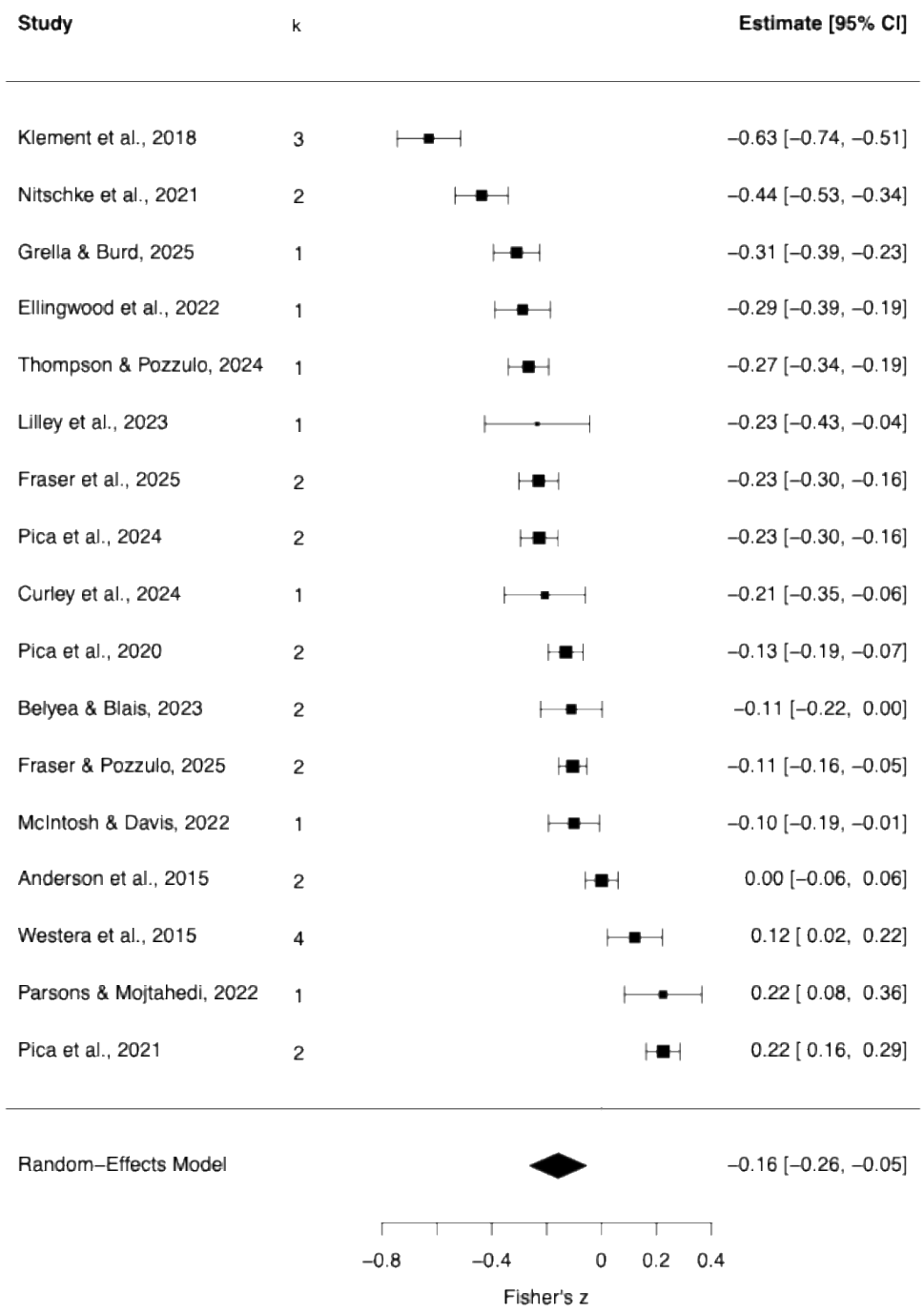
Association between rape myth acceptance and guilt

A three-level random-effects meta-analysis examined the association between rape myth acceptance and guilt judgments, pooling 30 effect sizes from 17 independent studies. The overall association was small but statistically significant, r = −.16, 95% CI [−0.26, −0.05], p = .006. Higher rape myth acceptance was therefore reliably associated with lower guilt (i.e. greater leniency towards the defendant) across studies. A forest plot of individual and pooled effects is presented in Figure 2.

There was, however, substantial residual heterogeneity in effect sizes, indicating considerable variability in the strength of associations across studies. The residual heterogeneity statistic was $QE(29) = 388.23$, $p < .001$. Variance decomposition suggested that most heterogeneity operated at the between-study level, with 91.8% of the variance attributable to differences between studies ($\sigma^2 = 0.043$) and 8.2% to within-study variability among multiple effect sizes from the same study ($\sigma^2 = 0.004$).

Figure 2.

Forest Plot of the Association Between Rape Myth Acceptance and Guilt by Study



Note. The horizontal axis is Fisher's z. For reference, z = 0.10, 0.25, 0.50, 0.75 and 0.90 correspond approximately to r = .10, .24, .46, .64 and .72. Negative z values correspond to negative r values of the same magnitude.

Subgroup analyses for guilt

Two potential moderators were examined where there were sufficient studies: sample type (student vs community) and study location (North America, Europe, Australia). All moderator analyses used three-level mixed-effects meta-regression models.

Sample type did not significantly moderate the association between rape myth acceptance and guilt, $F(1, 28) = 3.29$, $p = .081$. The pooled effect for student samples was small but statistically significant ($z = -0.21$, 95% CI [−0.32, −0.09], $p = .001$), and there was no statistically reliable evidence that community samples differed meaningfully from student samples ($\Delta z = 0.20$, 95% CI [−0.03, 0.40], $p = .08$). Substantial residual heterogeneity remained after including sample type as a moderator, $QE(28) = 366.09$, $p < .001$, indicating that differences between student and community samples did not explain the observed variability in effect sizes.

Study location also did not emerge as a clear moderator of guilt outcomes. The omnibus test comparing North America, Europe and Australia was non-significant, $F(2, 27) = 0.31$, $p = .74$, and substantial residual heterogeneity remained, $QE(27) = 383.63$, $p < .001$. These findings suggest neither sample type nor broad study location accounts for the considerable between-study variability in the association between rape myth acceptance and guilt.

Association between rape myth acceptance and victim blame

A separate three-level random-effects meta-analysis examined the association between rape myth acceptance and victim blame, pooling 13 effect sizes from 7 studies. The pooled effect was moderately strong and statistically significant, $r = .52$, 95% CI [.38, .63], $p < .001$. Thus, higher rape myth acceptance was reliably associated with greater victim blame across studies. A forest plot of individual and pooled effects is presented in Figure 3.

Residual heterogeneity for victim blame was again substantial, $QE(12) = 216.19$, $p < .001$. Variance partitioning indicated that heterogeneity was almost entirely attributable to variability among multiple effect sizes within studies ($\sigma^2 = 0.069$), with negligible between-study variance. Given the relatively small number of studies, no further subgroup or meta-regression analyses were conducted for blame outcomes. Full model estimates for the pooled guilt and blame effects, and for the sample-type and location moderator models for guilt, are summarised in Table 2.

Table 2.

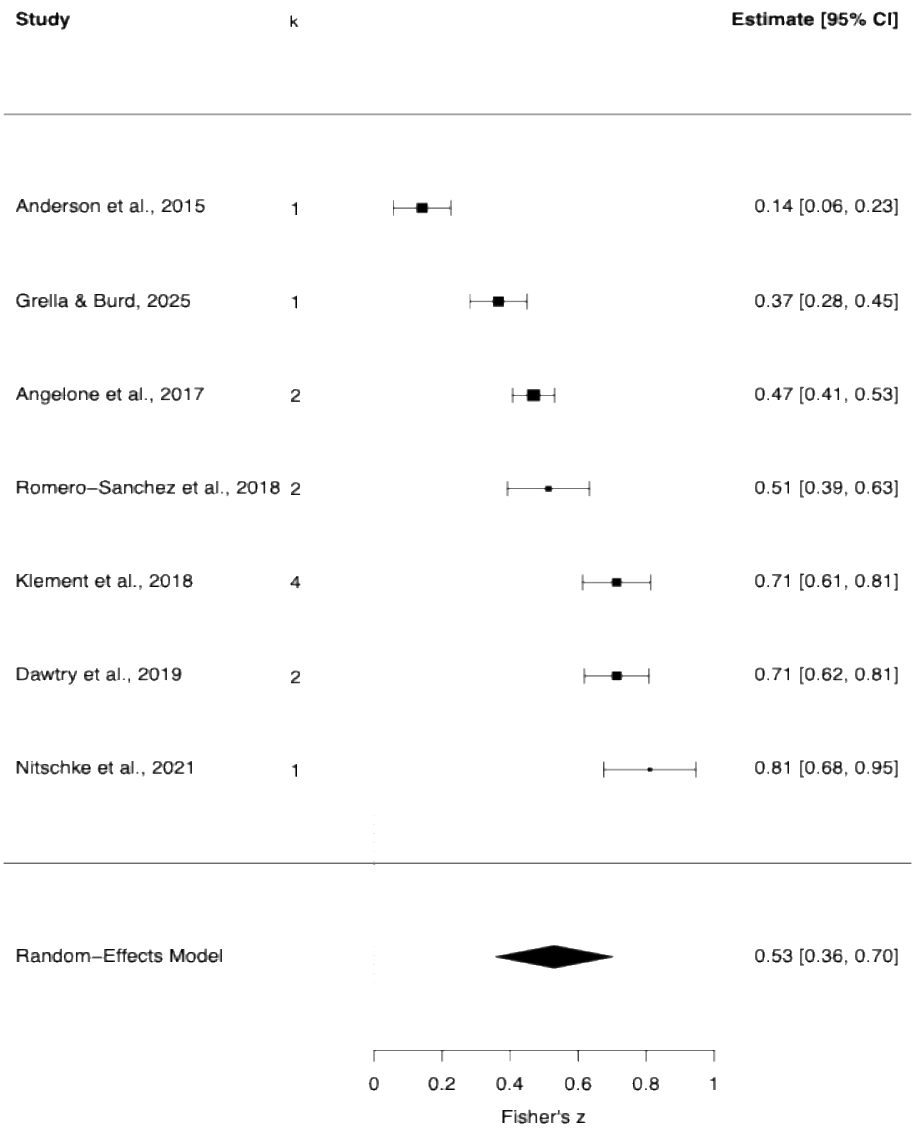
Output of Meta-Analyses of Guilt, Including Sample and Location Subgroup Analysis, and Blame

Predictor	Estimate	*SE*	*t*	*df*	*p*	95% CI
Guilt	-0.16	0.05	-2.95	29	0.006**	[-0.27, -0.049]
Sample type						
Intercept (student)	-0.22	0.06	-3.62	28	.001**	[-0.34, -0.09]
Community	0.20	0.11	1.81	28	.08	[-0.26, 0.43]
Location						
Intercept (North America)	-0.19	0.07	-2.69	27	.012*	[-0.33, -0.04]
Europe	0.11	0.14	0.79	27	.439	[-0.17, 0.39]
Australia	0.30	0.18	0.17	27	.864	[-0.33, 0.39]
Blame	0.57	0.08	7.50	12	< .001***	[0.4, 0.74]

Note. * $p < .05$. ** $p < .01$. *** $p < .001$. Estimates are Fisher's *z* values; corresponding *r* values are reported in the text.

Figure 3.

Forest Plot of the Association Between Rape Myth Acceptance and Blame by Study



Note. The horizontal axis is Fisher's z. For reference, z = 0.10, 0.25, 0.50, 0.75 and 0.90 correspond approximately to r = .10, .24, .46, .64 and .72.

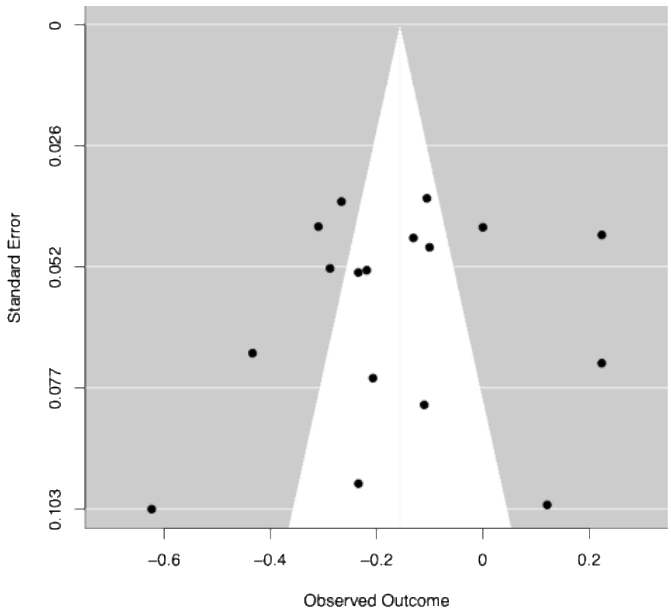
Publication bias and sensitivity analyses

Because sensitivity analyses and tests for small-study effects are underpowered when fewer than 10 studies are available (Mathur & VanderWeele, 2020), publication bias assessments were restricted to the guilt meta-analysis. Visual inspection of the funnel plot did not suggest marked asymmetry (Figure 4), and Egger's regression test was non-significant (r = −0.62, p = .54), indicating no evidence of small-study effects for guilt outcomes. Leave-one-out analyses showed that the pooled guilt effect was robust to the omission of individual studies; no single study exerted a disproportionate influence on the overall estimate, with pooled effects ranging from r = −0.13 to r = −0.18 across iterations.

For the blame meta-analysis, the number of available studies (k = 7) was below the recommended threshold for meaningful funnel plot asymmetry tests. Formal publication bias analyses were therefore not conducted for blame, and results should be interpreted with appropriate caution.

Figure 4.

Funnel Plot of Guilt Meta-Analysis



Discussion

This review set out to update Dinos et al.'s (2015) meta-analysis of rape myth acceptance (RMA) and juror decision-making, focusing on studies published since 2012. Across 20 eligible studies, the pooled association between RMA and guilt judgements was small but statistically significant, such that higher RMA was reliably associated with lower ratings of guilt and a reduced likelihood of guilty verdicts. The pooled association between RMA and victim blame was moderate and statistically significant, indicating that higher RMA was associated with substantially greater blame of the complainant. In both outcomes, heterogeneity was very high, with guilt effects largely driven by between-study differences and blame effects dominated by variability among multiple effect sizes within studies. This pattern is broadly consistent with Dinos et al.'s finding of a medium-sized correlation between RMA and more defendant-favourable outcomes but suggests that in more recent work RMA exerts a smaller, more context-dependent influence on guilt decisions alongside a more robust association with victim blame, indicating rape myths remain relevant to how complainants are evaluated.

One interpretation is that the field is now operating in a different attitudinal context. Over the last decade, public and legal attention to sexual violence has increased and direct questioning suggests relatively low explicit endorsement of some 'classic' rape myths among real jurors in England and Wales (Thomas, 2020). Changes in statutory frameworks and guidance (for example, the codification of consent and 'reasonable belief' in the Sexual Offences Act 2003 in England and Wales, and subsequent judicial directions) may also have shaped both the case materials used in mock-juror research and the expectations that participants bring to the task. If fewer people now openly agree with classic myth items, variance on standard RMA scales may have shrunk, naturally weakening simple correlations with verdicts even when underlying schemas remain influential. However, Daly et al. (2023) argue Thomas's work does not show that rape myths are unimportant; rather, they highlight that her studies were not designed to test the impact of myths on verdicts and that her own data show ambivalence and uncertainty on key items. The present review fits between these positions. It does not simply reproduce the earlier medium-sized effects reported by Dinos et al., but neither does it justify a strong claim that rape myths are now negligible in juror decision-making. Instead, RMA continues to predict guilt and blame, with a particularly strong association for blame.

A second important explanation lies in how the post-2012 evidence base differs from the studies synthesised by Dinos et al. More recent work is methodologically more varied; for example, using updated RMA measures (e.g. AMMSA, revised IRMA scales), manipulating more complex case features (such as relationship context, intoxication, delayed reporting, and complainant mental health), and often giving at least as much weight to intermediate outcomes (credibility, sympathy, anger, memory) as to verdicts and blame. When considering the story-model of juror decision-making (Pennington & Hastie, 1993), jurors build narrative accounts of what happened by combining case evidence with pre-existing schemas. Rape myths in this framework shape how information is interpreted (for example, what counts as 'normal' victim behaviour or reasonable belief in consent), rather than operating only as a direct lever on guilty and not-guilty decisions. In this review, guilt and victim blame were meta-analysed to preserve continuity with Dinos et al., but the stronger pooled association with blame is consistent with RMA exerting its clearest influence on intermediate evaluative judgements about the complainant, with guilt perhaps emerging at a later stage from those judgements in combination with other evidential and normative considerations.

The substantial heterogeneity observed in both meta-analyses underlines the importance of context. Moderator analyses based on broad study characteristics (student versus community samples; North America, Europe, Australia) did not explain this variability for guilt, and the smaller blame dataset did not permit further tests. It is likely that key sources of variation lie in case-level features that are difficult to capture with simple codes. Qualitative work has repeatedly shown that relationship status, intoxication, the presence or absence of physical force or injury, complainant demeanour, and delays in reporting act as focal points for myth-based reasoning in rape trials (Finch & Munro, 2005; Ellison & Munro, 2009; Temkin et al., 2016). When scenarios closely match 'real rape' stereotypes, most participants may favour conviction regardless of their RMA scores. In more ambiguous cases, RMA appears to play a larger role in how the incident is interpreted and how the complainant and defendant are evaluated (Nyúl et al., 2018). Averaging across both ambiguous and very clear cases in a meta-analysis is likely to dilute these context-specific effects, helping to explain why the overall association with guilt is modest even though rape myths clearly matter in some situations.

Measurement decisions are also relevant. RMA scales vary in the myth themes they emphasise (e.g. victim blame, denial of harm, disbelief, excusing the perpetrator) and some themes will be more salient than others in any given scenario. The relatively strong pooled association with victim blame may partly reflect conceptual overlap between blame-focused myth items and blame outcomes, whereas verdicts are shaped by a broader mix of evidential

and attitudinal inputs. In addition, several post-2012 studies use RMA primarily as a covariate or moderator and do not report simple bivariate associations with guilt or blame, making them difficult to include in a traditional correlation-based meta-analysis. The current synthesis therefore mainly reflects studies that reported direct RMA-outcome links and may under-represent work where the proposed role of RMA is conditional (for example, stronger effects in particular subsamples or case types) or indirect (for example, via credibility).

These findings also need to be considered alongside wider empirical and policy debates. Observational studies and legal work continue to identify the presence of rape myths in courtroom language and juror deliberations, especially around resistance, injury, intoxication, and reporting delay (Leverick, 2023). A recent systematic review of rape myth education with mock jurors concluded that providing myth-countering information can reduce RMA and, in some designs, shift decisions, although effects are often modest and sensitive to delivery format and study quality (Hudspith et al., 2024). Taken together, the literature suggests rape-supportive beliefs remain part of how complainants and defendants are evaluated, even if the direct correlation between global RMA scores and verdicts is smaller and more variable than earlier work implied.

For debates around juror reform and education, the current findings argue against simple, one-directional narratives. On the one hand, they do not support treating rape myths as a single, dominant explanation for low conviction rates in rape cases. On the other hand, they do not support the view that rape myths have been largely eradicated or can safely be ignored. Instead, the picture that emerges is one in which RMA is best understood as one element within a wider set of attitudes and structural factors that influence how rape cases progress and how evidence is evaluated. Its impact appears to be mediated through intermediate judgments (such as credibility and perceived reasonableness of complainant behaviour) and moderated by case features and other juror characteristics, rather than operating as a uniform, main-effect predictor.

Overall, the review suggests the relationship between rape myths and juror decision-making in rape cases is now more heterogeneous and context-dependent than the earlier meta-analysis by Dinos et al. indicated. RMA remains relevant, and its influence on guilt and blame is neither inevitable nor uniform. Future work needs to move beyond treating RMA as a single predictor of a single outcome and instead model how specific myth themes interact with specific kinds of cases and with other attitudes.

Strengths and Limitations

A key strength of this review is that it directly updates earlier work by Dinos et al. (2015), using similar eligibility criteria and outcomes while extending the timeframe into a period marked by substantial social and legal change around sexual violence. Data extraction followed a standardised template and study quality was appraised using the AXIS tool. Three-level random-effects models were used to account for dependence between multiple effect sizes from the same study and to partition within- and between-study variance, which is important in a literature where individual studies often report several relevant outcomes. Sensitivity analyses indicated that the pooled effects were robust to reasonable alternative decisions about effect-size selection and model specification.

At the same time, there are important limitations in the evidence base that affect how these results can be understood. Several studies reported insufficient quantitative information to derive an effect size linking RMA (or closely related attitudes) to at least one decision-making outcome, reporting of basic descriptive and correlational statistics was often incomplete, and outcome measures varied considerably. To keep conceptual consistency with Dinos et al. (2015) the meta-analysis was limited to guilt-related and victim blame outcomes. Outcomes such as credibility, sympathy, anger, and memory were not combined, as they were operationalised and reported too inconsistently across studies, even though they may be closer to the processes through which RMA exerts its effects. In addition, all the included studies were conducted in high-income, largely Western countries that share broadly similar adversarial legal systems. The review therefore cannot speak to how rape-supportive attitudes operate in lower- and middle-income settings, or in cultural and legal contexts where gender norms, evidential rules, or jury structures look very different.

The ability to examine moderators was also limited. Sample type and broad geographical region were tested in the guilt meta-analysis, but did not account for the high residual heterogeneity, and the smaller victim-blame dataset precluded moderator analyses. Important contextual variables such as relationship between the parties, presence and type of intoxication, use of force, complainant demeanour, and the presence or absence of educational materials were often described narratively but could not be coded reliably for quantitative analysis. Participant gender and age also place constraints on interpretation. Most samples were majority female and skewed towards younger adults, and although several primary studies reported gender differences, data were not presented in a way that allowed formal meta-

analytic comparison by gender or age bracket. As a result, the review cannot determine whether the pooled effects differ systematically across demographic subgroups of jurors.

As in most mock-juror research, there are also concerns about ecological validity and representativeness. Many included studies recruited student or online panel samples, used individual decision-making rather than group deliberation, and presented written vignettes rather than live or filmed evidence. These designs have clear practical advantages but limit the extent to which the findings can be generalised to real juries. Further to this, it may make it more difficult to quantify the real-world impact of rape myth acceptance, because the simplified, individual decision-making tasks used in experiments may not capture how attitudes are influenced in group deliberations and under legal directions.

Conclusions and Recommendations

Altogether, the post-2012 mock-juror literature suggests a more complex picture than earlier work. In contrast to Dinos et al. (2015), the pooled association between rape myth acceptance and guilt in this review was small rather than moderate, and the association with victim blame, although moderate in size, sat within a highly heterogeneous set of findings. This pattern indicates that RMA still plays a role in shaping both guilt judgements and victim blame, but perhaps not as a single, consistent driver of decision-making across all case types and study designs.

Future research should move away from treating global RMA scores as a single predictor of verdicts and instead focus on the pathways through which rape myths shape decisions. This includes examining how specific myth themes affect intermediate judgements (for example, credibility, perceived reasonableness of the complainant's behaviour, or assessments of consent), how those judgements feed into verdicts, and how these patterns differ across case types. Routine reporting of basic descriptive and correlational statistics would also make future synthesis easier. Where possible, studies should improve ecological validity by using more diverse jury-eligible samples, incorporating deliberation, and presenting more realistic case materials.

For policy and practice, these findings point to a middle ground. They do not justify abandoning concern about rape myths, but they also do not support placing primary responsibility for low conviction rates on juror attitudes alone. Juror education and judicial directions that challenge common rape myths remain a reasonable measure, especially when they target specific, well-documented misconceptions about resistance, injury, intoxication and

delayed reporting. However, expectations about large or direct effects on conviction rates should be modest. Meaningful progress on the justice gap will require addressing rape myths as one factor among many, alongside police and prosecutorial practices, evidential rules, and the practical realities and resource constraints of investigating and prosecuting sexual offences.

Beyond juror decision-making, the findings also have implications for legal professionals and expert witnesses. Barristers and advocates may, consciously or otherwise, draw on rape-supportive assumptions when framing questions or constructing alternative narratives; for example, by placing emphasis on delayed reporting, lack of resistance or injury, alcohol use, or ongoing contact between complainant and defendant in ways that encourage myth-consistent interpretations. Thematic-linguistic analyses of cross-examination in U.S. rape trials suggest that strategy-based questioning frequently organises itself around such themes and can reproduce familiar rape myths within courtroom discourse (Zidan, 2023). Awareness of how these questioning strategies may interact with juror attitudes is relevant for the development of guidance on appropriate cross-examination. For clinicians and other expert witnesses, the persistence of myth-consistent reasoning underlines the value of explicitly explaining the variability of trauma responses, including non-resistance, fragmented recall, and delayed disclosure, so that these are less likely to be treated as indications of fabrication rather than as common responses to sexual assault.

In summary, rape myths continue to form part of the interpretive framework through which rape cases are understood, but their influence on juror decision-making appears to be conditional and context dependent rather than uniform. Recognising this complexity should help to focus future research and practice on when and how rape-supportive attitudes matter most, rather than treating them as a single explanation or conversely, as if they are no longer relevant.

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CHAPTER THREE:

BRIDGING CHAPTER

Bridging Chapter

The two projects presented in this thesis portfolio examine how pre-existing belief systems shape juror responses in rape cases, but they each focus on different attitudinal constructs. The systematic review and meta-analysis centres on rape myth acceptance and related victim-focused attitudes, asking how these beliefs are associated with guilty verdicts and victim blame in sexual violence trials. Across the literature, higher rape myth acceptance is reliably linked with more defendant-favourable judgments and less favourable views of complainants (e.g., Dinos et al., 2015; Gray & Horvath, 2018; Hudspith et al., 2024). More broadly, work on jury decision-making indicates individual characteristics and pre-trial attitudes can exert small-to-moderate effects on guilt judgments across case types, rather than jurors approaching trials as blank slates (Devine & Caughlin, 2014; Walters et al., 2025). Together, these findings underline that cognitive schemas about rape and victims are an important part of the context in which any complainant is evaluated.

However, the review has a narrow scope. It focuses on rape-specific attitudes and does not systematically address mental illness stigma, diagnosis-specific stereotypes, or the situation of complainants with a SMI. This is a notable gap given the evidence people with SMI experience markedly elevated rates of physical and sexual victimisation, including rape, compared with the general population (Goodman et al., 2001; Khalifeh et al., 2015). Previous work on mental health stigma also suggests individuals with diagnoses of SMI often encounter assumptions of dangerousness, instability, or unreliability, and that these beliefs can influence responses in institutional settings (Baker et al., 2022; Klein et al., 2022; Livingston, 2013). For victims with SMI, rape myths and mental health stigma are therefore likely to intersect, but to the author's knowledge, there is very limited experimental work examining how jurors respond when a rape complainant has a stigmatised mental health condition, particularly in England and Wales.

The empirical paper addresses this intersection by testing how jurors respond to a rape complainant with a SMI, and whether these responses are associated with mental-health-related stigma. In doing so, it builds on the systematic review's synthesis of rape myth related bias while addressing the relative neglect of complainants with SMI and the lack of diagnosis-specific and jurisdiction-specific evidence. Together, the two projects move from examining how rape myths shape juror decision-making in sexual violence cases to considering how those decisions may be further influenced when the complainant is also described as having a stigmatised mental health condition.

CHAPTER FOUR:

EMPIRICAL STUDY

Prepared for submission to Psychology, Crime and Law.

(See Appendix B for author guidelines)

Word count: 10,694

How victim mental health status impacts juror decision-making in a rape case: A partial replication study with an English and Welsh sample

Kiah Bouillaut, Lucy Fitton & Peter Beazley

Department of Clinical Psychology and Psychological Therapies, Norwich Medical School, University of East Anglia, Norwich, United Kingdom

*Corresponding author: k.bouillaut@uea.ac.uk

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Abstract

People with severe mental illness (SMI) are at increased risk of sexual victimisation and contact with the criminal justice system, yet little is known about how rape victims with SMI are evaluated by jurors. This study partially replicated and extended Levi and Golding's (2024) work by examining how victim mental health status influences mock-juror decision-making in an adult rape trial framed under the Sexual Offences Act 2003. Jury-eligible adults (N = 130) from England and Wales read an acquaintance-rape trial summary in which the victim's mental health condition was cued via medication (borderline personality disorder, schizophrenia, or no mental illness). Participants returned a verdict, gave victim and defendant evaluations, and completed a mental health stigma measure and demographic questions.

The manipulation increased perceived mental illness severity for the schizophrenia condition relative to the control condition, but in contrast to Levi and Golding's (2024) findings, medication-cued victim mental health condition did not significantly predict guilty verdicts, guilt ratings, or most victim and perpetrator evaluations, and proposed indirect effects via credibility and perceived severity were not supported. More right-leaning political orientation was associated with greater victim blame and less favourable victim evaluations, and male participants were unexpectedly more pro-victim than female participants. Mental illness was rarely mentioned explicitly in not-guilty verdict explanations.

Briefly cueing victim SMI via medication description influenced perceived severity but did not meaningfully shift verdicts. Juror characteristics, particularly self-rated broad political orientation and gender, showed clearer associations with victim blame and credibility, highlighting the importance of broader attitudinal frameworks for clinical and legal practice.

Keywords: severe mental illness; schizophrenia; borderline personality disorder; rape; stigma; juror decision-making; mock juror.

Introduction

People with severe mental illness (SMI) comprise a sizeable minority of adults in England, with primary care registers indicating around half a million people with SMI (Office for Health Improvement and Disparities, 2025). People with SMI are frequently exposed to mental health stigma, which has been conceptualised as multi-dimensional and comprising three main components: stereotypes (negative beliefs), prejudice/attitudes (evaluative, often negative responses), and discrimination (behavioural responses) (Corrigan & Watson, 2002). Stereotypes are what people know or believe about mental illness, while attitudes are the cognitive and affective responses leading to discriminatory behaviour (Görzig & Ryan, 2022). Mental health stigma is a complex construct resulting from misperception and inaccurate public knowledge about mental health (Picco et al., 2019); people with mental illnesses can be seen as dangerous, incompetent, unpredictable, less human, and irresponsible (Corrigan et al., 2019; Njaka et al., 2023). These beliefs are particularly pronounced for people with SMI, where diagnoses such as schizophrenia and borderline personality disorder (BPD), are among the most stigmatised diagnoses across general-public and professional samples (Baker et al., 2022; Catthoor et al., 2015; Klein et al., 2022; Wood et al., 2014).

There are multiple tangible consequences arising from these beliefs. People with SMI are more likely to experience social exclusion, poor access to timely and appropriate physical and mental healthcare, and disproportionate contact with the criminal justice system (Henderson et al., 2020). They also face elevated risks of violent and sexual victimisation, including rape and sexual assault, with multiple studies documenting higher rates of assault and victimisation among people with SMI than in the general population (Goodman et al., 2001; Maniglio, 2009; Monahan et al., 2017; Rossa-Roccor et al., 2020). In the UK, nearly half of people with SMI report being victims of crime in a given year, with rates of domestic and sexual violence several times higher than in the general population (Khalifeh et al., 2015a, 2015b; Pettitt et al., 2013). These patterns of disadvantage make it especially important to understand how people with SMI are perceived in high-stakes settings, including the criminal justice system.

Diagnosis-Specific Stigma: Schizophrenia and BPD

Although mental illness is often discussed as a single category, stigma is not evenly distributed across diagnoses. For example, in relation to schizophrenia, public and professional attitudes are consistently more negative than those towards common mental health problems, such as depression (Angermeyer & Matschinger, 2003; Pescosolido et al., 2013) and anxiety (Wood et al., 2014). People with schizophrenia are frequently viewed as dangerous and unpredictable, eliciting fear and a desire for social distance (Angermeyer & Matschinger, 2003; Crisp et al., 2000). In population-based surveys, the label 'schizophrenia' has been associated with reduced perceived competence in treatment decision-making, greater endorsement of coercive measures such as compulsory hospitalisation, and pessimism regarding prognosis relative to labels such as depression or 'distress' (Pescosolido et al., 1999). These findings suggest diagnostic labels can activate stereotypes over and above the behavioural information presented, raising concern about how such labels may function in contexts such as the courtroom.

BPD is similarly associated with pronounced stigma but has received less empirical attention in public-attitudes research. BPD is characterised by instability in self-image, relationships, affect, and impulse control, alongside high rates of self-harm and suicidality (Gunderson et al., 2018). Limited but emerging work indicates that members of the public and some clinicians frequently describe individuals with BPD as manipulative, attention-seeking, or dangerous, and report feelings of frustration, fear, or hopelessness when working with them (Baker et al., 2022; Furnham et al., 2015; Ociskova et al., 2023; Weight & Kendal, 2013). Stigma towards personality disorders may be exacerbated by media portrayals emphasising volatility and harm to others (Crisp et al., 2000), and by ongoing debates about the validity and utility of personality disorder diagnoses more broadly (Lewis & Appleby, 1988; McCartan et al., 2020).

In combination, the literature suggests that schizophrenia and BPD carry particularly heavy diagnostic baggage. When such labels appear in legal contexts, whether in police reports, clinical records, or expert testimony, they may plausibly shape how lay decision-makers evaluate the credibility, responsibility, and deservingness of the person to whom the label is applied. However, compared to schizophrenia, BPD has been largely overlooked in empirical work on mental illness stigma in legal decision-making, despite its high public salience and the likelihood that people with BPD will come into contact with the criminal justice system as both defendants and victims.

Extra-legal Factors in Jury Decision-Making

One key area in which these diagnostic labels may carry particular weight is the courtroom. In England and Wales, most adults can be called for jury service, with jurors selected at random from the electoral roll and instructed to base their verdict solely on the evidence presented in court (Juries Act 1974; HM Courts & Tribunals Service, n.d.). In practice, jurors bring their own experiences, beliefs, and biases into the courtroom, and jury decision-making is subject to the same heuristics and cognitive shortcuts that affect decisions in everyday life (Bornstein & Greene, 2011; Tversky & Kahneman, 1974). Mock-jury and trial-simulation studies, developed in response to the legal and ethical barriers to studying real juries, consistently show that legally irrelevant 'extra-legal' factors, such as defendant race, attractiveness (Maeder et al., 2015), socio-economic status (Mazzella & Feingold, 1994), gender, and occupation (Maeder et al., 2012), can influence verdicts and sentencing recommendations.

Despite extensive research on extra-legal factors, mental health stigma remains relatively under-examined within juror decision-making, and much of the existing work has focused on defendants rather than victims. A recent systematic review of experimental mock-juror studies involving defendants with mental health conditions found that the bulk of this literature comes from North America and concentrates on violent offences and diagnoses such as psychosis, psychopathy, and personality disorders (Holmes & Beazley, 2025). Across these studies, diagnostic labels can shape perceptions of culpability and risk, but the direction and size of effects vary considerably. For example, mock jurors have sometimes judged defendants described as having schizophrenia or 'severe personality disorder, borderline pattern' as more dangerous and more in need of coercive control than comparison conditions, even when case facts are held constant (Baker et al., 2022; Mossiere & Maeder, 2015). In other designs, mental illness type has influenced support for insanity or diminished-responsibility outcomes, with schizophrenia or psychotic-spectrum diagnoses in particular sometimes attracting less leniency than depression or more generic mental health problems (Shapter, 2023).

However, these effects are not uniformly aggravating. Several experimental studies suggest that providing fuller mental health information, especially when it emphasises symptoms, treatment needs, or reduced control, can reduce guilty verdicts or lower guilt ratings, and that higher mental health literacy is associated with more lenient decisions (O'Leary, 2021; Tremlin, 2021). At the same time, baseline stigma appears to be a powerful moderator in that jurors with more negative attitudes towards mental illness are more likely to

convict, even when identical diagnostic information is presented (Holmes & Beazley, 2025; Termeer & Szeto A, 2021; Tremlin, 2021). Related work on witnesses indicates that mental health information can alter credibility assessments without necessarily shifting verdicts, again pointing to nuanced, diagnosis-specific effects rather than simply a 'mental illness = bias' pattern (Gous et al., 2024). Overall, recent reviews conclude that findings are methodologically heterogeneous and often inconsistent, with relatively few studies using jurors from England and Wales and a broader lack of replications in line with ongoing concerns about reproducibility in psychological science (Holmes & Beazley, 2025; Open Science Collaboration, 2015; Tackett et al., 2019).

Juror Decision-Making in Rape and Sexual Assault Trials

Within this broader literature on extra-legal influences, rape and sexual assault trials raise particular concerns. Rape and sexual assault trials are particularly vulnerable to the influence of extra-legal factors because they often involve contested questions of consent, credibility, and character rather than undisputed physical evidence. Large-scale victimisation surveys indicate that many sexual assaults are committed by current or former partners or other known perpetrators, meaning the key issue in court is frequently whether sexual contact was consensual rather than whether it occurred at all (Black et al., 2011; Breiding et al., 2014). In such cases, assaults typically occur in private and may leave limited forensic evidence, so jurors must make sense of competing narratives by drawing on their perceptions of the complainant's demeanour, consistency, and conformity with lay expectations of 'typical' victim behaviour before and after the assault (Coundouriotis, 2013; Golding et al., 2022; Leverick, 2020). These contextual features create space for jurors' pre-existing beliefs and cultural scripts about rape to shape their decisions.

Substantial literature has examined the concept of rape myths. These are culturally shared, false beliefs about rape, victims, and perpetrators that serve to excuse or minimise sexual violence, blame victims, and frame only a narrow subset of assaults as 'real rape' (Gray & Horvath, 2018; Graw Leary, 2020; Leverick, 2023). Across experimental and correlational studies, higher rape myth acceptance (RMA) has been linked to more defendant-favourable judgments, greater victim blame, lower perceived credibility, and reduced likelihood of guilty verdicts, particularly in cases involving acquaintance relationships, alcohol use, or delayed reporting (Dinos et al., 2015; Golding et al., 2022; Leverick, 2020). Recent reviews and meta-analyses conclude there is consistent evidence RMA can affect how mock jurors evaluate the

same evidence, shaping interpretations of consent, resistance, and victim responsibility (Dinos et al., 2015; Hudspith et al., 2024; Leverick, 2020).

At the same time, recent critiques caution against treating rape myths as a catch-all explanation for outcomes in real jury trials. Waiton (2023), for example, argues that claims of 'overwhelming evidence' of juror prejudice rely heavily on attitudinal surveys and mock-jury simulations that may not capture the safeguards, deliberation, and judicial directions present in actual trials. Leverick (2023) similarly highlights methodological limitations, such as convenience sampling, limited ecological validity, and variable operationalisations of RMA, while still concluding there is meaningful evidence of rape-myth-linked bias in decision-making. Thomas's analysis of real jurors in England and Wales similarly questions generalised claims about uniformly prejudiced juries (Thomas, 2020). Collectively, these accounts suggest rape myths remain an important construct for understanding juror decision-making, but that careful, context-specific and jurisdiction-specific research is required rather than assuming a universally biased jury.

Victim Mental Health as an Extra-Legal Factor in Rape Cases

Given the high rates of victimisation among people with SMI, it is striking that comparatively few studies have examined how jurors respond when a rape complainant is described as having a mental health condition. Existing work instead comes largely from adjacent areas, where diagnostic labels have been shown to shape perceptions of witness or victim credibility. For example, Bottoms et al. (2003) found that a 16-year-old sexual assault victim with mild intellectual disability was seen as more credible and less likely to have fabricated the allegation than an equivalent victim described as having average intelligence. In contrast, other studies have indicated witnesses with intellectual disabilities are often judged to be fundamentally honest but less accurate, competent, or reliable than non-disabled witnesses, with jurors more reluctant to rely on their evidence (Henry et al., 2011; Stobbs & Kebbell, 2003). Research on autism spectrum disorder suggests these effects can sometimes be mitigated; providing mock jurors with clear, non-stigmatising information about an autistic witness's diagnosis and associated behaviours has been shown to improve credibility ratings and, in some cases, bring them into line with those for non-autistic witnesses (Crane et al., 2020; Maras et al., 2019). More recently, Gous et al. (2024) demonstrated that mental health labels can also influence perceptions of witness competency, with witnesses described as having depression rated as more competent than those described as having schizophrenia or no mental illness. Taken together, these studies suggest diagnostic labels can act as powerful

cues in juror decision-making about witnesses and victims, but the direction and magnitude of their impact depend on the specific mental health condition and the way information is framed.

In one of the only studies to consider this issue, Levi and Golding (2024) directly examined the impact of victim mental illness on legal decision-making in a hypothetical adult rape case. Using a 3 (victim mental health status: depression, schizophrenia, no mental illness) × 2 (participant gender) between-subjects design, they presented participants with trial summaries in which the only experimental manipulation was the victim's diagnostic status; the factual details of the case were otherwise identical. Participants then rendered a verdict and rated the victim and defendant on a range of perceptual and emotional variables. Consistent with broader stigma research, participants were generally less pro-victim in the schizophrenia condition than in the depression and control conditions, including lower guilty verdict rates and less favourable ratings of victim credibility and sympathy. Mediation analyses indicated that victim mental health status indirectly influenced verdicts through perceptions of victim credibility, sympathy towards the victim, and perceived severity of the victim's mental illness (Levi & Golding, 2024).

Levi and Golding's study provides an important initial demonstration that a rape victim having a particular mental illness (cued via medication description), without changing the behaviour described, can alter juror perceptions and trial outcomes. However, several limitations constrain the conclusions that can be drawn and highlight the need for further work. First, the study used a United States sample and legal context, which limits generalisability to jurors in England and Wales, where legal directions, trial structure, and public discourse around both mental illness and sexual violence differ. Second, the diagnostic manipulation contrasted schizophrenia with depression and found little difference between the depression and no-diagnosis conditions, leaving open the question of how jurors might respond to other highly stigmatised diagnoses such as borderline personality disorder (BPD). Third, Levi and Golding did not include a direct measure of participants' mental-health-related stigma or attitudes, making it difficult to determine whether the observed effects reflected general negativity towards mental illness, diagnosis-specific stereotypes, or other individual-difference factors (e.g., rape myth acceptance, political orientation). Addressing these gaps, including by examining different diagnoses, collecting stigma measures, and testing samples drawn from England and Wales, forms a key rationale for the present study.

The Present Study

The present study aims to partially replicate and extend Levi and Golding's (2024) work in several ways. First, it adopts a similar design, but with a sample drawn from England and Wales and materials adapted to reflect the language, procedures, and legal expectations of criminal trials in this cultural context. This helps address the current lack of empirical data on how jurors in England and Wales evaluate rape complainants with mental health conditions and responds to broader calls for more context-sensitive jury research that goes beyond North American student samples (Horan & Israel, 2016; Ross, 2023).

Second, the study replaces Levi and Golding's depression condition with BPD. This modification is theoretically motivated by evidence that BPD attracts particularly negative public and professional attitudes (Adebowale, 2010; Baker et al., 2022; Ociskova et al., 2023). Levi and Golding's finding that depression did not meaningfully differ from the no-diagnosis condition suggests including BPD may provide a more stringent test of whether diagnosis-specific stigma shapes juror decision-making. Comparing schizophrenia-cued, BPD-cued, and no mental illness conditions also allows examination of whether different forms of SMI are associated with different patterns of perceived credibility, sympathy, and blame in the context of a rape trial.

Finally, the present study includes a validated self-report measure of participants' attitudes towards people with mental illness, providing a direct index of mental-health-related stigma that was absent in Levi and Golding's original design. Incorporating this measure makes it possible to examine whether baseline stigma moderates or helps explain the impact of victim diagnosis on verdicts and perceptions; for example, whether participants with higher levels of public stigma are particularly likely to discount the credibility of a victim with schizophrenia or BPD. This addition is consistent with prior mock-jury work showing that individual differences in stigma and mental health literacy can alter the extent to which diagnostic information influences legal judgments (Metcalfe-Hume et al., 2025; Tremlin, 2021).

In line with concerns about the replication crisis in psychology and the relative scarcity of direct replications in mock-juror research (Open Science Collaboration, 2015; Tackett et al., 2019), this project is explicitly framed as a partial replication and extension in a different legal and cultural setting.

Aims and Hypotheses:

The present study explored the impact of the presence and type of victim mental health condition on trial outcomes and juror perceptions, as well as the role of participant gender in shaping these judgments in a hypothetical rape case. Based on Levi and Golding's (2024) experimental paradigm, the present study tested four hypotheses:

1. Medication-cued victim mental health condition will have a main effect on juror judgments, including trial outcomes (guilty verdicts) and perceptions of the victim (e.g., sympathy, anger, and credibility), such that judgments will differ depending on whether the victim is cued as having a mental health condition (e.g., schizophrenia or borderline personality disorder) or no mental health condition.
2. Participant gender will have a main effect on juror judgments, with female participants expected to demonstrate more pro-victim judgments (e.g., higher rates of guilty verdicts and higher ratings of victim credibility) than male participants.
3. Victim mental health status will indirectly affect verdicts through perceptions of victim credibility and perceived severity of the victim's mental illness, such that differences in verdicts will be mediated by these perceptual variables.
4. Participants will reference the victim's mental illness as a reason for not guilty verdicts when providing open-ended explanations of their decisions.

Method

Design

The study used a between-subjects design with three levels of medication-cued victim mental health condition and participant gender measured as a between-participant factor. Participants were presented with a summary of a rape trial, where the mental health status of the victim was manipulated through the type of medication the victim was described as receiving (mood stabiliser for BPD condition, antipsychotic for schizophrenia condition, allergy medication for the control condition). No diagnosis was given at any point; a modification of Levi and Golding's (2024) paradigm. Rather than stating a diagnosis explicitly, the present study used medication as a more indirect cue to inferred mental health, with the aim of approximating how such information may arise in real legal contexts through treatment history or background material rather than through a direct diagnostic label. This approach was intended to increase

external validity, although it also made the manipulation more subtle than an explicitly stated diagnosis. Whilst there are no medications for BPD recommended within current guidance, mood stabiliser medication may be used 'off label' for management of mood instability and was chosen as the closest medication-based cue for this condition within the adapted paradigm.

The victim's medication was only mentioned during their recounting of the crime and there was no integration of their mental health status within either the prosecution or defence arguments. Following the trial summary, participants were asked to return a verdict (dependent variable) and respond to questions regarding their perceptions of the case.

Participants and Recruitment

A priori power analysis was conducted using G*Power 3.1 (Faul et al., 2009) to determine the required sample size. Based on a medium effect size (f^2 = .15), α = .05, and desired power of .80 for detecting main effects, the analysis indicated a minimum of 68 participants. To ensure adequate power for interaction effects and to align with comparable studies (e.g., Devine & Mojtahedi, 2021; Gous et al., 2024), the target sample size was set at 130 participants. Of 142 eligible participants who began the survey, 130 completed all measures and correctly answered the manipulation and comprehension checks; 12 who discontinued early were excluded. Due to missing data on some variables, analytic sample sizes for individual models ranged from 116 to 130.

Participants were recruited using two methods: Prolific (Palan & Schitter, 2018), an online platform suitable for psychological research, and snowball sampling (Johnson, 2005). Stratified recruitment based on England and Wales (E&W) census data (Office for National Statistics, 2022) was implemented within the Prolific sample using pre-screening and quotas for gender, age, and ethnicity; snowball sampling was then used as a supplementary recruitment method. Inclusion criteria aligned with the Juries Act 1974, requiring participants to be English-speaking adults aged 18-76 who had resided in England and Wales for at least five years and had no criminal record. Prolific participants received £3 on completion, and participants recruited via other methods were entered into a £20 Amazon voucher prize draw. Summary demographic characteristics of the final sample are provided in Appendix C.

Materials and Measures

Trial Summary

Participants were presented with an approximately 1500-word written summary (Appendix D) of a fictional criminal trial of a rape case (Levi & Golding, 2024). The summary comprised a description of an acquaintance rape scenario and was a modified version of a trial summary used in previous jury decision-making research (Lynch et al., 2020). Acquaintance rape was chosen by the original study because it reflects many real-life rape cases (Levi & Golding, 2024; Office for National Statistics, 2025c). In the summary, the victim (female) claimed she met the defendant (male) while standing in line at a pharmacy while waiting on her prescription for either mood stabiliser medication (BPD condition), antipsychotic medication (schizophrenia condition), or allergy medication (no mental illness control). The victim stated the defendant started a conversation with her, and that the two continued to talk while at the pharmacy. She claimed the defendant asked to walk her home, and she accepted. She then stated that after they arrived at her home and she said goodbye, the defendant held a knife to her back and forced her to let him inside. The victim stated the defendant raped her, even after she tried to stop him. The defendant claimed the victim invited him back to her home and asked him to come inside. He stated the sexual act was rough but consensual.

Participants were also presented with two additional witnesses in the trial summary. The victim was the first witness for the prosecution's case, providing the testimony described earlier. The second witness was a detective, who testified for the prosecution. He stated he was dispatched to the victim's home after the alleged rape occurred and observed the victim visibly distressed and bruised. He further testified that he took her to the hospital for an examination. The defendant was the first witness for the defence's case, providing the testimony previously outlined. A neighbour of the victim was the second witness for the defence, testifying he saw the victim and the defendant having a friendly conversation at the pharmacy when he stopped in to buy an item. The summary included both direct examination and cross-examination of all witnesses. At the end of the trial summary, participants were informed about the legal requirements for returning a guilty verdict in accordance with E&W law, specifically under the Sexual Offences Act 2003 (Sexual Offences Act, 2003), and were given a description of the standard of proof required, which is 'beyond reasonable doubt.'

Manipulation-and Comprehension-check Questions

As part of the trial summary, participants were asked to complete a manipulation-check question assessing awareness of the manipulated variable by prompting them to recall specific details (e.g., "What medication did the victim receive at the pharmacy?"). Participants were then asked three multiple-choice questions to confirm their comprehension of the material (e.g., "How did the defendant describe the sexual interaction?"). Participants who answered any of these questions incorrectly were informed and asked to review the trial summary carefully (Oppenheimer et al., 2009, as cited in Levi & Golding, 2024). Levi and Golding (2024) used more frequent section-by-section checks; here a shorter set was adopted, given their very high pass rates and to reduce participant burden.

Trial Questionnaire

Participants were asked to complete a trial questionnaire in the form of a survey. This included being asked to return a verdict of the trial (guilty or not guilty); provide ratings of the defendant's guilt (1= not at all guilty, 10= completely guilty); and answer an open-ended question giving the reason for their chosen verdict. To ensure ecological validity and representation of jury decision-making processes in E&W, returning a guilty or not guilty verdict was the primary variable of interest and was asked first in the temporal order of questions within the survey, however the continuous version was also asked to provide more statistical power and support sensitivity analyses. This approach mirrored that in the original study.

Participants were also asked to provide judgments regarding both the victim and defendant using a scale ranging from 1 (not at all) to 10 (completely/a lot). Specifically, they rated the victim on credibility, believability, honesty, perceived responsibility for the crime, and blame for the crime. Additionally, participants indicated the extent of sympathy and anger they felt towards the victim, as well as their perception of the victim's memory of the event. Similarly, participants rated the defendant on these same dimensions, apart from ratings of memory and perceived mental illness. This is because the primary focus of the study was perceptions of the victim and impact of victim mental health status. These measures are adapted from previous studies on juror decision-making and perceptions (e.g., Golding et al., 2016; Lippert et al., 2017; Lynch et al., 2019; Lynch et al., 2013, as cited in Levi & Golding, 2024).

Attitudes Towards Persons with Mental Illness (APWMI) scale

The APWMI (Kobau et al., 2010; Appendix E) was used to assess participants' attitudes towards mental illness. Based on the British Omnibus National Survey (Crisp et al., 2000) and used in similar mock-juror studies (Gous et al., 2024; Mossière & Maeder, 2015), the scale comprises 11 items rated on a 5-point Likert scale ('strongly disagree' to 'strongly agree'). Example items include 'Treatment can help people with mental illness lead normal lives' and 'A person with mental illness is unpredictable.' Total scores range from 11 to 55, with higher scores indicating more negative attitudes; five items are reverse-scored. Previous work has reported acceptable internal consistency (α = .57-.67), and internal consistency in the present sample was α = .63. The APWMI was included as a covariate indexing general mental-health stigma.

Procedure

Participants were presented with a participant information sheet (Appendix F) and a consent form (Appendix G). For participants who did not consent to proceed with the study, the survey ended. Those who consented to participate began with completing the APWMI and then were instructed to carefully read the trial summary. Once they finished reading the trial summary, participants were asked complete the manipulation and comprehension check questions, deliver a verdict (guilty or not guilty), complete the trial questionnaire, and provide demographic information (e.g., gender, age, ethnicity, and political orientation; see Appendix H). All participants were asked the questions in the same order. The process took approximately 30 minutes to complete. At the end, participants were debriefed (see below and Appendix I) and provided with the researchers' contact information for the opportunity to ask questions or share concerns.

Ethical Considerations

Confidentiality was ensured by obtaining a non-identifiable dataset; only collecting non-identifiable data. Participants were provided with a participant information sheet outlining the purpose of the study, procedure, and potential risks and benefits, along with the consent form. Participants were informed participation in the study was voluntary. However, due to having a non-identifiable dataset, participants did not have a right to withdraw once they clicked 'submit'; this was clearly explained in the participant information sheet. No deception was

used, as the purpose of the study was broadly explained without the specific hypotheses being outlined. Due to the sensitive topic of the study, steps were taken to minimise participant harm by including signposting information in the debrief for anyone experiencing distress. Ethical approval (Appendix J) was granted by the Faculty of Medicine and Health Sciences Research at the University of East Anglia (ETH2425-0020).

Analytic Plan

Participant responses were only analysed if they met criteria for inclusion (i.e. correctly answered all manipulation-check and comprehension-check questions). All analyses were conducted using IBM SPSS Statistics (Version 31), and mediation analyses were conducted using Hayes' PROCESS macro (Hayes, 2022).

Independent and Dependent Variables

The primary independent variable was victim mental health condition. This variable was dummy coded into two planned comparison variables: BPD versus control and schizophrenia versus control. Each dummy variable was entered in separate models to allow clear interpretation of each contrast. Dependent variables included the binary guilty verdict, the continuous guilt rating, composite scores for victim credibility and victim blame, composite scores for perpetrator credibility and perpetrator blame, sympathy and anger ratings towards both the victim and perpetrator, and perceived severity of the victim's mental illness. Stigma-related variables consisted of the Attitudes towards Persons with Mental Illness (APWMI) total score and perceived mental illness severity (when not treated as the dependent variable). Demographic variables included participant gender, age, and political orientation.

Composite Variables

Following Levi and Golding (2024), several questionnaire items were combined into composite scores reflecting core constructs. Victim credibility was calculated by averaging ratings of credibility, believability, and truthfulness (α = .95; Levi & Golding α = .95). Victim blame was calculated by averaging blame and responsibility ratings (α = .84; Levi & Golding α = .93). Perpetrator credibility (α = .95; Levi & Golding α = .97) and perpetrator blame (α = .98; Levi & Golding α = .96) were computed from parallel items. All composites demonstrated excellent internal consistency and were retained for analysis.

Categorical Covariates

To ensure adequate cell sizes and stable estimates in regression models, ethnicity was collapsed into two categories (White; ethnic minority), with 'Prefer not to say' coded as missing. Political orientation was collapsed into three categories (Left; Centre; Right), and responses of 'Other' or 'Prefer not to say' were treated as missing. These decisions balanced statistical stability with preservation of the conceptual structure of the variables.

Manipulation Check

A manipulation check was conducted to assess whether the victim mental health manipulation influenced participants' perceptions of the victim's mental illness. A one-way ANOVA compared perceived mental illness severity across the three victim conditions. As Levene's test indicated unequal variances ($p = .003$), the Games-Howell procedure was used for pairwise comparisons (Games & Howell, 1976). Holm's sequential Bonferroni correction was applied to the set of three contrasts to control the familywise error rate while retaining reasonable statistical power (Holm, 1979).

Analysis for Hypothesis 1

To address Research Question 1 and Hypothesis 1, hierarchical regression models examined the effect of victim mental health status on juror judgments. For the binary guilty verdict, hierarchical logistic regression was used. Victim mental health dummy codes were entered at Block 1, stigma-related variables were entered at Block 2, and demographic variables were entered at Block 3. This structure was also used for continuous outcomes (e.g., guilt rating, victim credibility, sympathy, anger, and memory ratings) using hierarchical linear regression. Each dependent variable was analysed separately, and models were run for each of the dummy-coded contrasts to test all planned comparisons.

Analysis for Hypothesis 2

Hypothesis 2 was examined using separate regression models in which participant gender was entered as the sole predictor. Logistic regression was used when the dependent variable was the binary guilty verdict, and standard linear regression was used for all continuous dependent variables (e.g., credibility, sympathy, anger, blame, memory, perceived mental illness severity). Each outcome variable was analysed in an independent regression model to

estimate the direct effect of participant gender on juror judgments. No covariates or additional blocks were included in these analyses, reflecting the analytic approach used by Levi and Golding (2024) for examining gender differences in juror decision-making.

Analysis for Hypothesis 3

To test Hypothesis 3, mediation analyses were conducted using the PROCESS macro for SPSS (Model 4; Hayes, 2022), which estimates indirect effects in a regression framework using bootstrap confidence intervals. For each planned contrast, the relevant victim mental health dummy code was entered as the independent variable, guilty verdict (0 = not guilty, 1 = guilty) was specified as the outcome, and victim credibility and perceived severity of the victim's mental illness were entered as mediators. Gender, age, ethnicity, and political orientation were included as covariates. Indirect effects were evaluated using 10,000 bias-corrected bootstrap samples, as recommended for mediation analysis (Hayes, 2022). These models were also repeated with continuous guilt ratings as the outcome to provide a sensitivity analysis.

Analysis for Hypothesis 4

Hypothesis 4 was examined using a directed quantitative content analysis of participants' open-ended explanations for their verdict. A deductive coding framework was developed by the author, informed by the study hypotheses and previous work on juror decision-making and rape myths. The author first read all responses to familiarise themselves with the data and to identify segments where participants explained why they reached a guilty or not-guilty verdict. An initial set of coding categories was then drafted to capture: (a) explicit mentions of the victim's mental illness or medication ('mental illness mentioned'), (b) use of the victim's mental illness as part of the justification for the verdict ('mental illness used in reasoning'), (c) stigmatising references to mental illness (e.g., implying the mental health information or symptoms made the victim less credible or more to blame; 'mental illness stigma'), (d) credibility-based reasoning more broadly (e.g., whether the victim or defendant was believable, reliable, or likely to be lying), and (e) factual or evidential reasoning (e.g., references to bruises, the knife, witnesses, or the standard of 'beyond reasonable doubt'). These categories were refined through trial coding of a subset of responses until the definitions were clear and non-overlapping.

All responses were first coded by the author for the presence (1) or absence (0) of each category. A second coder then independently applied the same coding frame to 128 responses. Coders agreed on 127 of 128 responses (99% agreement), and the single discrepancy was

discussed and resolved before analysis. A single response could be assigned to multiple categories if several types of reasoning were present. Frequencies and proportions were calculated for each category. Although all verdict explanations were coded, analyses for Hypothesis 4 focused on not-guilty verdicts (n = 64), in order to examine whether participants explicitly cited the victim's mental illness as part of their justification for acquittal.

Results

Descriptive Statistics

Descriptive statistics (means and standard deviations) for verdicts and continuous study variables, broken down by each independent variable, are presented in Table 3. Inspection of histograms, skewness and kurtosis values, and Shapiro-Wilk tests indicated that all continuous variables were approximately normally distributed, with no substantial deviations from normality. Distributions showed adequate variability and no floor or ceiling effects, supporting the use of parametric analyses.

Table 3.

Means (standard deviations) of dependent variables for each independent variable.

	Age (years)	Stigma (APWMI total)	Verdict	Guilt ratings	Victim credibility (composite)	Victim blame (composite)	Victim sympathy	Victim anger	Victim memory	Victim MIS
Participant gender										
Male (*n* = 85)	37.05 (13.30)	31.21 (5.08)	0.56 (0.50)	64.67 (27.94)	74.69 (23.48)	9.68 (18.14)	84.21 (23.91)	7.52 (20.66)	66.18 (29.28)	40.60 (30.78)
Female (*n* = 40)	39.73 (15.70)	32.09 (4.86)	0.38 (0.49)	53.82 (28.42)	64.40 (25.04)	21.14 (22.15)	73.00 (23.99)	7.27 (14.60)	59.16 (25.99)	51.58 (30.22)
Victim MH status										
BPD	36.69 (14.70)	31.88 (4.72)	0.55 (0.50)	68.21 (22.68)	73.50 (18.13)	9.94 (17.63)	85.02 (20.56)	3.43 (10.58)	66.90 (26.69)	45.24 (24.86)
Schizophrenia	39.85 (14.72)	30.46 (4.88)	0.48 (0.51)	55.28 (31.40)	68.72 (27.16)	14.65 (19.92)	81.00 (21.87)	10.59 (23.16)	60.80 (28.11)	55.85 (27.55)
Control	37.21 (13.12)	32.31 (5.30)	0.48 (0.51)	59.79 (29.37)	71.40 (26.98)	16.25 (22.94)	74.90 (29.64)	7.98 (19.35)	63.81 (30.29)	31.02 (35.02)
Total	37.98 (14.17)	31.52 (5.00)	0.50 (0.50)	60.92 (28.47)	71.13 (24.43)	13.65 (20.29)	80.33 (24.44)	7.43 (18.73)	63.75 (28.28)	44.40 (30.92)

	Perpetrator credibility (composite)	Perpetrator blame (composite)	Perpetrator sympathy	Perpetrator anger
Participant gender				
Male (*n* =85)	36.01 (26.46)	73.25 (31.50)	18.32 (23.74)	55.27 (36.93)
Female (*n* =40)	43.88 (24.11)	68.61 (26.38)	22.33 (23.56)	41.47 (36.52)
Victim MH status				
BPD	36.02 (25.26)	76.49 (24.17)	16.24 (20.50)	61.33 (35.43)
Schizophrenia	43.57 (28.35)	67.65 (35.46)	25.17 (26.36)	44.09 (39.24)
Control	36.14 (23.29)	71.17 (28.08)	17.19 (22.61)	46.67 (35.13)
Total	38.73 (25.86)	71.64 (29.80)	19.71 (23.56)	50.49 (37.23)

Note. APWMI = Attitudes Towards Persons with Mental Illness; MIS = perceived mental illness severity. Verdict coded 0 = not guilty, 1 = guilty; higher scores on all other variables indicate more of the named construct (e.g., greater victim blame, sympathy, or anger).

Manipulation Check

A one-way ANOVA examined whether perceived mental illness severity differed across the three victim conditions (control, BPD, schizophrenia). The effect of condition was significant, $F(2, 127) = 7.85$, $p < .001$, $\eta^2 = .11$, indicating a medium effect.

Games-Howell comparisons showed that the schizophrenia condition ($M = 55.85$, $SD = 27.55$) was rated as significantly more severe than the control condition ($M = 31.02$, $SD = 35.02$), $p = .001$. Holm-adjusted post-hoc tests confirmed this was the only significant pairwise difference. Ratings for the BPD condition ($M = 45.24$, $SD = 24.86$) did not differ significantly from either the control condition ($p = .088$) or the schizophrenia condition ($p = .145$).

These results indicate that the schizophrenia label increased perceived mental illness severity as intended, whereas the BPD label showed more moderate, non-significant changes. Overall, this suggests the schizophrenia cue was effective in shaping perceived severity, while the BPD cue produced weaker differentiation.

Hypothesis 1: Victim Mental Health Status

To test whether juror judgments varied based on the victim's mental health diagnosis (BPD, schizophrenia, or no condition [control]), hierarchical logistic and linear regressions were conducted, with victim condition entered at Step 1, stigma towards mental illness (APWMI) at Step 2, and demographic covariates (age, gender, ethnicity, and political orientation) at Step 3.

A hierarchical logistic regression examined whether victim mental health status predicted the likelihood of returning a guilty verdict (0 = not guilty, 1 = guilty), while controlling for stigma towards mental illness and participant demographics. Entering the two victim condition contrasts at Step 1 did not significantly improve model fit relative to the null model, $\chi^2(2) = 0.90$, $p = .64$, Nagelkerke $R^2 = .01$. Adding stigma towards mental illness (APWMI total) at Step 2 did not account for additional variance in verdicts, $\chi^2\Delta(1) = 0.13$, $p = .71$ (Nagelkerke $R^2 = .01$). At Step 3, the inclusion of demographic covariates (gender, age, ethnicity, and political orientation) produced a non-significant improvement in model fit, $\chi^2\Delta(5) = 9.55$, $p = .09$, and the final model was not significant overall, $\chi^2(8) = 10.58$, $p = .23$, Nagelkerke $R^2 = .12$.

In the final model, neither schizophrenia vs. control nor BPD vs. control significantly predicted guilty verdicts (schizophrenia vs. control: $B = 0.27$, $SE = 0.49$, Wald = 0.31, $p = .58$, $OR = 1.31$, 95% CI [0.50, 3.41]; BPD vs. control: $B = 0.46$, $SE = 0.50$, Wald = 0.84, $p = .36$, $OR = 1.58$, 95% CI [0.59, 4.21]). Stigma towards mental illness and ethnicity and political orientation covariates were also non-significant predictors (all ps ≥ .30). There was weak evidence that male and younger participants were more likely to return guilty verdicts (gender: $B = 0.77$, $SE =$

0.42, Wald = 3.36, p = .07, OR = 2.15, 95% CI [0.95, 4.89]; age: B = −0.03, SE = 0.02, Wald = 3.15, p = .08, OR = 0.97, 95% CI [0.95, 1.00]), although these effects did not reach the conventional α = .05 threshold. Overall, victim mental health status did not significantly influence the likelihood of a guilty verdict.

Hierarchical linear regressions were conducted for guilt ratings, victim credibility, blame, sympathy, anger, and memory; full model coefficients are reported in Appendix K (Table K1). Victim mental health status did not significantly predict any of these outcomes; in the final models, neither the BPD nor schizophrenia conditions differed from the control condition on any victim rating (all ps ≥ .16).

The only outcome with a clearly significant overall model was victim blame, where the full model accounted for around a quarter of the variance, R^2 = .25, F(7,108) = 5.07, p < .001. Higher stigma towards people with mental illness and more right-leaning political views were associated with greater victim blame (APWMI β =.20, p = .027; political orientation β =.27, p = .003), whereas male participants tended to blame the victim less than female participants (β = −.21, p = .013). A similar political pattern emerged across other victim evaluations where more right-leaning participants perceived the victim as less credible and less deserving of sympathy (credibility β =−.29, p = .003; sympathy β = −.26, p = .007). All other effects were non-significant.

Hierarchical linear regressions were also conducted for perpetrator credibility, blame, sympathy, and anger; detailed coefficients are provided in Appendix K (Table K2). Victim mental health status did not significantly predict any perpetrator ratings in the final models (all ps ≥ .12). The only significant predictor across these outcomes was political orientation in the perpetrator-blame model: participants with more right-leaning views attributed less blame to the perpetrator (β = −.20, t = −2.06, p = .042). Although the full model for perpetrator anger was statistically significant, R^2 = .13, F(7,108) = 2.34, p = .029, no individual predictors reached the conventional α = .05 threshold.

Hypothesis 2: Participant Gender

Hypothesis 2 was partially supported. Participant gender significantly predicted several juror decision-making outcomes, although some effects were in the opposite direction to predictions. In contrast with the hypothesis, male participants were more likely than female participants to render a guilty verdict (b = 0.76, SE = 0.38, Wald = 4.05, p = .044, OR = 2.14, 95% CI [1.02, 4.48]), and gave significantly higher ratings of victim credibility (b = 10.29, SE = 4.43, β = .20, p = .022), victim sympathy (b = 11.21, SE = 4.41, β = .22, p = .012), and anger toward the perpetrator (b = 13.80, SE = 6.78, β = .18, p = .044). However, male participants also assigned

significantly less blame to the victim compared to female participants (b = -11.47, *SE* = 3.62, β = -.27, *p* = .002).

Participant gender did not significantly predict anger toward the victim (b = 0.25, *SE* = 3.47, β = .01, *p* = .942), perceptions of the victim's memory (b = 7.02, *SE* = 5.20, β = .12, *p* = .179), or perceived severity of the victim's mental illness (b = -10.98, *SE* = 5.64, β = -.17, *p* = .054). Similarly, participant gender did not significantly predict ratings of perpetrator credibility (b = -7.87, *SE* = 4.73, β = -.15, *p* = .099), perpetrator blame (b = 4.64, *SE* = 5.50, β = .07, *p* = .401), or perpetrator sympathy (b = -4.02, *SE* = 4.35, β = -.08, *p* = .357).

Hypothesis 3: Indirect effects of victim mental health status on guilt verdicts and guilt ratings via victim credibility and perceived mental illness severity

Guilt Verdicts

Parallel mediation models (PROCESS Model 4) tested whether victim mental health status exerted indirect effects on guilty verdicts via victim credibility and perceived mental illness severity. For each planned contrast (BPD vs control; schizophrenia vs control), guilty verdict (0 = not guilty, 1 = guilty) was modelled using logistic regression, with coefficients expressed on the log-odds scale, and age, gender, ethnicity, and political orientation entered as covariates (*N* = 116). Victim condition did not directly predict the likelihood of a guilty verdict (BPD vs control: *b* = 0.44, *SE* = 0.58, *Z* = 0.75, *p* = .45; schizophrenia vs control: *b* = 0.56, *SE* = 0.66, *Z* = 0.84, *p* = .40). Schizophrenia, relative to the control condition, was associated with higher perceived mental illness severity (*b* = 18.30, *SE* = 5.70, *p* = .002), but neither contrast significantly affected victim credibility (*p*s ≥ .29). Across both models, higher victim credibility was associated with increased odds of a guilty verdict (bs ≈ 0.12, *p*s < .001), and younger age predicted greater likelihood of conviction (*b*s = −0.05, *p*s ≤ .03). However, there was no evidence of mediation: for both contrasts, the bootstrapped total indirect effect and the specific indirect effects via credibility and perceived severity were non-significant, with all 95% confidence intervals including zero (e.g., total indirect effect for BPD vs control: *b* = 0.27, 95% CI [−0.87, 1.61]; schizophrenia vs control: *b* = −0.32, 95% CI [−1.89, 1.04]). Thus, Hypothesis 3 was not supported when guilty verdicts were used as the outcome.

Guilt Ratings

As a sensitivity analysis, the same mediation models were repeated with continuous guilt ratings as the outcome, using the same contrasts (BPD vs control; schizophrenia vs control), mediators (victim credibility and perceived mental illness severity), covariates, and 10,000 bootstrap samples. Victim condition did not significantly predict guilt ratings in either model (*p*s = .08 and .37). Victim credibility again showed a strong positive association with guilt ratings (*p*s < .001), whereas perceived severity did not reach significance (*p*s ≥ .08). Across both contrasts, the total indirect effect and the specific indirect effects via credibility and perceived severity were non-significant, with all bootstrapped 95% confidence intervals including zero (e.g., total indirect effect for BPD vs control: *b* = 2.06, 95% CI [−5.79, 9.51]; schizophrenia vs control: *b* = −2.71, 95% CI [−11.31, 6.12]). This sensitivity analysis therefore supported the primary verdict models, suggesting no evidence for the proposed mediation when guilt was modelled continuously.

Hypothesis 4: Content analysis of open-ended verdict explanations

To address Hypothesis 4, frequencies and percentages of coded reasons for not-guilty verdicts (n = 64) are presented in Table 4.

Table 4.

Frequencies and percentages of coded reasons in not-guilty verdict explanations

Code	N	%
Mental illness mentioned	5	7.8
Mental illness used in reasoning	4	6.2
Mental illness stigma	3	4.7
Credibility reasoning	32	50
Factual/evidential reasoning	57	89

Note. Percentages are calculated out of 64 not-guilty verdict explanations. Each code was scored 0 (absent) or 1 (present).

As shown in Table 4, explicit references to the victim's mental illness were rare when participants explained not-guilty verdicts, providing little support for Hypothesis 4.

Discussion

The present study set out to replicate and extend Levi and Golding's (2024) work on victim mental health and juror decision-making in rape trials, using a jury-eligible sample from England and Wales, a modified diagnostic manipulation in which the victim was described as collecting medication intended to cue BPD (mood stabiliser), schizophrenia (antipsychotic), or no mental illness (allergy medication), and an added stigma measure. Overall, the pattern of findings differed from the original study in several important respects. Medication-cued victim mental health condition did not significantly predict guilty verdicts, guilt ratings, or most evaluations of the victim and perpetrator, and there was no evidence victim condition exerted indirect effects on verdicts via credibility or perceived mental illness severity. By contrast, participant characteristics – particularly self-rated broad political orientation and gender, and with smaller and less consistent contributions, mental illness stigma – were more reliably associated with variation in how participants evaluated the victim and perpetrator.

The content analysis suggested that participants rarely cited the victim's mental health explicitly when explaining not-guilty verdicts; instead, explanations tended to centre on available evidence and broader credibility concerns rather than diagnosis. Taken together, these findings suggest the brief medication cues to mental health, as presented in this paradigm, were not determinative of verdicts or most evaluative outcomes. Juror worldviews and broader socio-political attitudes, alongside gender, appeared to shape responses in more modest and selective ways, with some outcomes more affected than others. In this sense, the study showed the cued mental health information was noticed and associated with perceived severity, but it did not, in itself, drive verdict outcomes in an obviously prejudicial direction.

Victim Mental Health Status

In contrast to Levi and Golding (2024), who found that victims described as having schizophrenia were generally evaluated less favourably and were less likely to secure guilty verdicts than those described as having depression or no diagnosis, the present study found no significant main effects of victim condition on verdicts or most outcomes. In this sample, schizophrenia was rated as more severe than the control condition, and BPD was rated at an intermediate level, but these differences in perceived severity did not translate into systematic shifts in credibility, sympathy, blame, or guilt ratings. Similarly, the mediation analyses further indicated that, although higher victim credibility was strongly associated with a greater likelihood of conviction, victim condition did not reliably alter credibility or perceived severity in ways that produced indirect effects on verdicts. The failure to reproduce Levi and Golding's

pattern of victim mental health effects in this England and Wales sample suggests these effects may be more context-dependent than originally implied.

There are several potential interpretations. One is that the diagnostic manipulation, as implemented here, was relatively subtle. Mental health information was signalled only through the type of medication the victim was described as collecting at the pharmacy and was not mentioned again by either advocate. The manipulation check showed that participants did differentiate schizophrenia from the control condition in terms of perceived severity, but the BPD condition did not differ significantly from either. This pattern suggests that, in this sample, 'antipsychotic medication' may have more reliably evoked a sense of serious mental illness than 'mood stabiliser', and that some participants may have noticed or weighted the BPD cue less strongly. For example, studies in which diagnostic labels are named directly, or where symptoms are more vividly depicted, have sometimes found small but detectable effects of mental health information on legal judgements (e.g., Baker, 2022). More broadly, labels that map clearly onto existing stereotypes or 'serious mental illness' categories (e.g., schizophrenia) may be more likely to shape judgments than labels that are less familiar or less salient (Angermeyer & Matschinger, 2003). If participants varied in how clearly they registered or interpreted the medication information, any genuine effect of victim condition would have been diluted when averaged across the sample.

A second, more substantive explanation is that the legal and cultural context of England and Wales may have shaped how diagnostic cues were interpreted. The case was explicitly framed under the Sexual Offences Act 2003, and participants were given information about the standard of proof in this jurisdiction. England has also been the focus of national anti-stigma initiatives, particularly the Time to Change campaign, with large-scale surveys suggesting gradual improvements in public knowledge, attitudes, and desired social distance regarding mental illness over time (e.g. Henderson et al., 2020). In this context, a single, relatively low-salience reference to medication may be less likely to trigger overtly discriminatory decision-making, particularly when the prosecution case itself includes visible injuries, prompt reporting, and corroborative evidence.

A further interpretive point concerns the extent to which any credibility-related response to mental illness cues should be understood as stigma, rather than as a potentially case-relevant inference. This is particularly important for schizophrenia and other psychotic-spectrum conditions, where jurors may regard questions about perception, interpretation, or reality orientation as more directly relevant to witness reliability than they would for other

diagnoses. In that sense, not every credibility-related inference is necessarily irrational or overtly prejudicial. However, the present vignette did not describe the complainant as actively psychotic, confused, or experiencing symptoms directly relevant to the alleged assault; participants were only given a brief medication cue. In that context, stronger negative judgements about credibility may be difficult to justify from the case facts alone and are more likely to reflect stereotype-based assumptions associated with schizophrenia, such as unreliability or unpredictability (Angermeyer & Matschinger, 2003; Crisp et al., 2000).

Political Orientation and Broader Beliefs

Levi and Golding (2024) focused primarily on diagnosis and gender and did not examine political orientation or broader ideological variables. In the present study, by contrast, self-reported political orientation showed clearer and more consistent associations with victim evaluations than victim condition did. Participants who identified as more right-leaning tended to attribute greater blame to the victim, rate her as less credible, express less sympathy, and assign less blame to the perpetrator. Higher scores on the mental health stigma measure (indicating more negative attitudes towards people with mental illness) were also associated with increased victim blame, although these effects were smaller, accounted for only modest incremental variance, and were not consistently statistically significant across models. In other words, participants' self-rated broad political orientation appeared to be more consistently associated with these judgements than the brief mental health cue embedded in the vignette, with stigma showing a weaker and less consistent pattern.

This pattern is consistent with wider work linking conservative political ideology and related constructs (e.g., right-wing authoritarianism, social dominance orientation) with stronger mental health stigma and a greater emphasis on personal responsibility (Schomerus & Angermeyer, 2021; Szabó et al., 2024). It also echoes research on rape myth acceptance (RMA), which shows that people who endorse more rape-supportive beliefs tend to blame victims more, blame perpetrators less, and interpret the same case facts in more defendant-favourable ways (Murray et al., 2023; Rosewood & Hammond, 2023). Meta-analytic work on juror decision-making similarly identifies authoritarianism and related attitudinal constructs as robust predictors of guilt judgements across a range of case types (Devine & Caughlin, 2014). Although the present study did not measure RMA, right-wing authoritarianism, or system-justifying beliefs directly, the finding that more right-leaning participants blamed the victim more and the

perpetrator less fits with these accounts of how broader ideological worldviews can shape responses to sexual violence, regardless of whether mental health is salient in the case.

Overall, our findings suggest that, even when a victim's mental health status does not produce a clear main effect on verdicts, there can still be meaningful variation in how jurors interpret the same case as a function of their wider beliefs about responsibility, risk, and social order. In practice, this implies that adjusting how mental health information is presented in court may not be sufficient on its own; jurors bring their own political and attitudinal frameworks to the task, and these frameworks appear to shape how they read and weigh the evidence they are given.

Gender Effects

Hypothesis 2 was only partially supported and, importantly, in the opposite direction to both the original prediction and Levi and Golding's (2024) findings. In the present study, male participants were more likely than female participants to return guilty verdicts, rated the victim as more credible and more deserving of sympathy, expressed greater anger towards the perpetrator, and attributed less blame to the victim. Female participants, by contrast, tended to be more cautious or critical in their evaluations. This is an opposite pattern to that reported by Levi and Golding, who found that women were more pro-victim than men across multiple outcomes and runs against the general trend in the wider rape-attitudes literature, where women typically endorse fewer rape myths and show lower levels of victim blame (e.g., Suarez & Gadalla, 2010).

One possible explanation is sample composition and potential selection bias. This study recruited jury-eligible adults who were willing to complete a relatively long, sensitive survey about rape and mental illness. Men who choose to take part in this type of research may be more likely to hold explicitly pro-victim or egalitarian views than men in the general population. In contrast, some women may approach the material with more ambivalence or scepticism, particularly if they have personal experiences of sexual violence or not being believed. Theoretical work on just-world beliefs suggests that, because sexual assault poses a particular threat to women, some women may sometimes engage in victim blaming as a self-protective strategy – using harsh judgments of other women's behaviour to distance themselves from the possibility of their own victimisation (i.e. to maintain the belief that 'this would not happen to me; Gravelin et al., 2019; Pinciotti & Orcutt, 2020). The present study did not collect data on trauma history or more detailed gender identity, so these interpretations remain

speculative rather than tested explanations, but they are consistent with accounts of intra-gender variation in responses to sexual violence (Person et al., 2025).

Gender effects are also increasingly intertwined with political and cultural context. Large-scale survey data from Scotland, for example, indicate that between 2014 and 2019 victim-blaming attitudes decreased and perceptions of perpetrator culpability increased, alongside shifts towards more liberal political views and less traditional gender-role attitudes (Abrams, 2025). In that study, being a woman was associated with higher victim blaming in 2014 once attitudes were controlled for, but this effect was no longer significant by 2019, underscoring that gender gaps in responses to sexual violence are not fixed. The present finding that men were more pro-victim than women may therefore reflect a subgroup of participants who self-identified with broader left-leaning political views and who were highly critical of sexual violence. Future work should examine gender together with political orientation, rape myth acceptance, and mental-health stigma to clarify whether gender continues to explain unique variance once these related constructs are considered.

Qualitative Reasoning for Not-Guilty Verdicts

Hypothesis 4 focused on whether participants would explicitly draw on the victim's mental health when explaining not-guilty verdicts. In line with the quantitative findings, this was rare. Only a small minority mentioned the victim's mental health at all, and even fewer used it as a clear reason for acquittal or in an openly stigmatising way. Most explanations centred on the strength of the evidence (for example, whether the charge was proved 'beyond reasonable doubt') and on broader questions of credibility.

This contrasts with Levi and Golding's (2024) cognitive network modelling, where 'victim has schizophrenia' emerged as a central reason for not-guilty verdicts in that condition. The difference may reflect several factors, including analytical approach (network modelling versus relatively simple content coding), cultural and legal context, or the brevity of written responses in an online survey. Future work using richer qualitative methods (for example, recorded deliberations or interviews), and potentially network approaches in a UK sample, would be better placed to examine how, if at all, victim mental health is woven into jurors' reasoning in practice.

Strengths, Limitations, and Directions for Future Research

The present study has several important strengths. First, it provides a partial replication and extension of Levi and Golding's (2024) work in a different legal setting, using jury-eligible adults from England and Wales and situating the case under the Sexual Offences Act 2003. Amid wider concerns about the replicability of psychological research (e.g., Shrout & Rodgers, 2018), systematically reproducing key elements of the original paradigm in a new context represents a meaningful contribution. By retaining core features of the design (such as the acquaintance-rape scenario and the manipulation of victim mental health status) while adding additional variables and analytic approaches, the study both tests the robustness of earlier findings and develops them further.

Another strength is the inclusion of an explicit measure of stigma towards people with mental illness (APWMI; Kobau et al., 2010). Levi and Golding (2024) highlighted the absence of such a measure as a limitation in their study. By incorporating the APWMI, the present study could test whether individual differences in attitudes towards mental illness accounted for variation in victim and perpetrator evaluations. This helps distinguish decision-making driven by the experimental manipulation (victim diagnosis) from decision-making shaped by pre-existing stigma or mental health literacy. Finally, the directed quantitative content analysis of open-ended verdict explanations provided additional insight into how participants articulated their reasoning. Coding not-guilty verdict explanations for references to mental illness, credibility, and evidential reasoning added depth to the interpretation of the quantitative findings and directly addressed Hypothesis 4.

Nonetheless, several limitations should also be considered. As with most mock-juror research, participants made decisions as individuals and did not engage in jury deliberation. Group deliberation can change both the process and outcome of decision-making, and individual judgments may not fully capture how juries arrive at a final verdict (Diamond, 1997; Thomas, 2020). In addition, the study relied on a written trial summary rather than filmed testimony or live presentation. Written materials facilitate experimental control and comparability with Levi and Golding (2024), but they may encourage participants to rely more on pre-existing beliefs and cannot reproduce non-verbal cues, emotional tone, or the interpersonal dynamics of a courtroom. Future research could extend this work by incorporating video-based stimuli and small-group deliberations to approximate real-world jury processes more closely.

The online survey format also brings constraints. Although manipulation and comprehension checks were included, it is not possible to be certain that all participants read

the trial summary carefully, attended to every detail, or were free from distractions. Participants were self-selecting, compensated for taking part, and aware their decisions had no real-world consequences. These factors may differentiate them from jurors who are randomly summoned and required to decide a real case. Moreover, while the sample was designed to be broadly representative on key demographics, collapsing ethnicity into a binary variable (White vs. ethnic minority) and political orientation into three categories inevitably sacrificed nuance. These decisions were necessary to maintain adequate cell sizes and model stability, but they limit the ability to explore more fine-grained patterns (e.g., distinctions between specific minority groups or between different forms of left- and right-leaning political ideology).

Interpretation of the experimental manipulation also warrants caution. The manipulation check indicated that the schizophrenia condition was rated as more severe than the control condition, but ratings for the BPD condition were intermediate and did not differ significantly from either control or schizophrenia. This suggests that the BPD label may have produced a weaker or more ambiguous manipulation of perceived severity than intended, potentially contributing to the absence of clear condition effects on juror judgments. In addition, the APWMI is a generic measure of attitudes towards 'people with mental illness' rather than a diagnosis-specific scale. Previous work has reported only acceptable internal consistency for this measure (e.g., Gous et al., 2024; Mossière & Maeder, 2015), and stigma towards schizophrenia and BPD may operate differently from the broader construct captured by the APWMI; it is therefore difficult to determine whether the small and inconsistent stigma effects reflect genuinely weak relationships or measurement limitations. Future studies could incorporate diagnosis-specific stigma measures or behavioural indicators of bias alongside self-report scales.

There are also limitations relating to design and statistical power. Although the a priori power analysis suggested that a sample of 130 participants was adequate to detect medium-sized main effects, the study may have been underpowered to detect smaller effects, interactions, or complex indirect pathways. The largely null findings for victim mental health condition should therefore be interpreted as an absence of evidence rather than firm evidence of no effect. The mediation analyses for Hypothesis 3 were also based on cross-sectional data, with verdicts and ratings collected in a single session. While temporal ordering was modelled conceptually (victim condition – perceptions – verdict), the procedure required participants to return a verdict before completing the rating scales. As Levi and Golding (2024) note, this limits the extent to which causal inferences can be drawn about perceptual mediators and verdict

outcomes. Longitudinal or multi-stage designs would be needed to establish temporal precedence more clearly.

Finally, the content analysis for Hypothesis 4 was based on relatively brief open-ended responses, and only a small proportion of not-guilty verdicts explicitly referenced the victim's mental illness. Even though two coders applied a shared coding framework, the finding that participants rarely mentioned the victim's mental illness should be interpreted cautiously. It may reflect how short the responses were, the wording of the prompt, or social desirability concerns, rather than an unequivocal absence of mental health information-related reasoning. Future studies could address this by using more open-ended qualitative methods, inviting participants to comment more directly on the role of the victim's mental health in their decision-making.

Overall, these strengths and limitations suggest the present study provides a rigorous and ecologically informed test of how victim mental health status and juror characteristics shape decision-making in a rape trial, while highlighting the need for further studies using different methods to build a clearer evidence base in this area.

Conclusions and Clinical Implications

This study set out to examine how victim mental health status, juror characteristics, and stigma relate to decision-making in a rape trial, using a jury-eligible sample from England and Wales. In contrast to Levi and Golding's (2024) findings, simply cueing the victim's schizophrenia or BPD via medication information, compared to no mental illness, did not significantly alter guilty verdicts or most victim and perpetrator ratings. Instead, patterns emerged around juror characteristics, with more right-leaning political views and higher stigma linked to greater victim blame and less favourable victim evaluations, while male participants were, unexpectedly, more pro-victim across several outcomes. Victim credibility was strongly associated with verdicts and guilt ratings, but victim condition did not reliably shift credibility scores. Open-ended verdict explanations suggested that participants rarely stated the victim's mental illness as a central reason for acquittal, with most emphasising credibility and evidential reasoning.

These findings have several implications for clinical practice. First, they suggest that for this sample, diagnostic labels alone may not be as determinative of juror decision-making as often feared, which may offer some reassurance to victims with mental health conditions who worry that disclosure will automatically undermine their credibility. At the same time, the

association between stigma, political orientation, and victim blame highlights that broader attitudes towards mental illness still matter. Clinicians working with victims involved in the criminal justice system may therefore need to attend both to the impact of past invalidation and to internalised stigma, helping clients to separate their own sense of credibility from anticipated prejudices. Psychoeducation about common juror concerns (e.g., inconsistencies, memory, delayed reporting) and rehearsal of giving evidence in a supported way may also help victims feel more prepared and less defined by their diagnosis (Hudspith et al., 2024).

The results also highlight how clinical and legal professionals might work together in court settings. Where a victim's mental health is likely to be raised, mental health professionals can support legal teams to present diagnostic information in a way that is accurate, non-sensationalised, and clearly linked to the victim's experience of the offence rather than to stereotypes about risk or unreliability. Expert evidence may be particularly important in explaining how symptoms such as dissociation, trauma-related memory difficulties, or self-harm relate to both mental illness and sexual violence, and why these should not automatically be taken as indicators of untruthfulness. More broadly, clinical psychologists and psychiatrists can contribute to the development of training or guidance materials for judges and jurors, aimed at improving mental health literacy and challenging myths that people with mental illness are inherently less credible.

For the legal system, the findings underline the importance of attending to juror attitudes rather than assuming managing labels alone is sufficient. Judicial directions, pre-trial educational materials, or written summaries that briefly explain common mental health conditions and their relevance to witness evidence may help to anchor jurors' reasoning in evidence rather than stigma. Given the observed links between political orientation, stigma, and victim blame, there may also be value in future policy work that considers whether, and how, juror education on mental illness and rape myths could be incorporated into broader initiatives on fair trial practice.

Overall, this study adds to a small but growing body of UK research on how jurors make sense of mental health information in the context of sexual offences. It suggests that, at least in this sample briefly cueing mental health information via medication description did not meaningfully shift verdicts, but that underlying attitudes and beliefs about mental illness and responsibility remain highly relevant. Continued collaboration between psychologists, psychiatrists, legal professionals, and policymakers will be essential to ensure that victims with mental health conditions can participate in legal processes without being unfairly

disadvantaged, and to support jurors to reach decisions that are informed by evidence rather than by stigma.

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CHAPTER FIVE:

DISCUSSION AND CRITICAL EVALUATION

Discussion and Critical Evaluation

This thesis portfolio examined how extra-legal beliefs shape juror decision-making in rape cases, with a specific focus on rape myth acceptance and stigma towards people with severe mental illness. The systematic review and meta-analysis updated Dinos et al.'s (2015) work by synthesising post-2012 mock-juror studies that measured RMA in the context of rape and sexual assault trials, quantifying its association with guilt judgements and victim blame. The empirical study extended this literature by adapting Levi and Golding's (2024) paradigm to an England and Wales context, testing how a rape complainant's mental health status and jurors' attitudes towards people with mental illness influenced verdicts and perceptions in an acquaintance rape case framed under the Sexual Offences Act 2003. In combination, the two papers consider how rape-specific schemas and mental health stigma may intersect in shaping responses to rape complainants, with particular relevance for rape victims with SMI.

This chapter summarises and integrates the main findings from the systematic review and empirical paper, critically evaluates the methods and decisions made across the portfolio, and situates the work within wider legal and theoretical debates about juror prejudice in rape trials. It then outlines priorities for future research and considers the implications of the findings for theory, clinical practice and service delivery, and criminal justice policy.

Summary of Findings

The systematic review and meta-analysis updated the work of Dinos et al. (2015) by focusing on post-2012 experimental and mock-juror studies reporting associations between RMA and two core outcomes: guilt-related decisions and victim blame. Across the included studies, higher RMA was consistently associated with increased attribution of blame to the complainant, and to a lesser extent, a reduced likelihood of guilty verdicts or lower guilt ratings. Overall, this pattern echoed the pre-2012 findings reported by Dinos et al. (2015) but the pooled associations were generally smaller and between-study heterogeneity was high. This heterogeneity appeared to be shaped by differences in the contextual features manipulated across studies (e.g., delayed reporting, levels of intoxication, relationship between complainant and defendant). In addition, variation and inconsistent reporting in other decision-making outcomes (such as credibility or sentencing) meant that only guilt and victim-blame outcomes could be synthesised meta-analytically; the scope of the review's conclusions therefore remained restricted to these two constructs.

The empirical project added to juror decision-making literature by examining how stigma towards mental illness and victim mental health status influenced mock-juror decision-making in an acquaintance-rape case framed under the Sexual Offences Act 2003. Victim mental health status was briefly cued via a medication prompt, rather than explicit diagnostic labels. In contrast to Levi and Golding's (2024) findings, the mental health manipulation did not produce significant differences in guilty verdicts, guilt ratings, or victim-focused evaluations. Baseline stigma towards people with SMI (as measured by the APWMI) was associated with increased victim blame, but these effects were smaller, less consistent across models, and more difficult to interpret given the only modest internal consistency of the scale. By comparison, political orientation and participant gender emerged as more robust predictors of variation in responses. Participants who reported more right-leaning political views tended to attribute more blame to the victim, rate her as less credible and less deserving of sympathy and allocate less blame to the perpetrator. Gender effects were also evident but countered the pattern often reported in the rape-attitudes literature. In this sample, male participants were more likely than female participants to convict and generally evaluated the victim more favourably (higher credibility and sympathy, lower blame). Overall, the empirical findings suggest that, in this specific trial context and with mental health information only implicitly cued, juror decisions were shaped more by their broader socio-political attitudes than by stigma towards mental illness or the particular victim mental health condition.

Integration of Findings

Considered together, the two projects in this portfolio suggest juror decision-making in rape cases is more strongly associated with underlying attitudinal variables than with the specific experimental manipulations employed. The systematic review and meta-analysis indicated that rape myth acceptance (RMA) remains a consistent correlate of guilt-related decisions and victim blame in post-2012 experimental rape and sexual assault trials, although average effect sizes were smaller and more heterogeneous than those reported in the pre-2012 synthesis by Dinos et al. (2015). By contrast, the empirical study found that experimentally cued victim mental health status (via brief medication information) did not significantly alter verdicts, guilt ratings, or most evaluative outcomes; instead, variation in responses was more clearly associated with broader socio-political attitudes (particularly self-rated broad political orientation), gender, and, to a lesser extent, general stigma towards people with mental illness.

Conceptually, the patterns observed across the two projects are consistent. RMA is commonly understood as one component of a broader attitudinal pattern organised around beliefs about personal responsibility, gender roles, and social order (e.g., Suarez & Gadalla,

2010; Grubb & Turner, 2012). Meta-analytic work on juror decision-making similarly indicates that attitudinal constructs such as authoritarianism and related belief systems are often more predictive of guilt judgements than most demographic variables or case characteristics (Devine & Caughlin, 2014). Although the empirical study did not include a direct measure of RMA, the finding that more right-leaning participants blamed the victim more, rated her as less credible and less deserving of sympathy, and attributed less blame to the perpetrator aligns with evidence that conservative ideological orientations tend to co-occur with higher rape myth acceptance and stronger mental health stigma (e.g., Schomerus & Angermeyer, 2021; Szabó et al., 2024). In this respect, the empirical project complements the systematic review: whereas the review quantified the association between RMA and guilt- and blame-related outcomes, the empirical study illustrates how a related cluster of socio-political attitudes and gender can structure responses to the same rape scenario, even when mental health cues are brief and relatively low in salience.

At the same time, the finding in the empirical study should be interpreted cautiously in light of how political belief was measured. Contemporary psychological research often examines political ideology using more differentiated constructs, such as authoritarianism, social dominance orientation, or system-justifying beliefs, rather than a single broad self-placement item (e.g., Devine & Caughlin, 2014; Jost et al., 2004). In the present study, political belief was measured using a simple 3-point self-rated broad political category, which was useful as a low-burden indicator of general orientation but cannot identify which specific ideological features were most relevant to juror judgements. The findings therefore suggest a broad association with political self-positioning in this context, rather than a precise claim about any one ideological construct.

The two projects also converge in highlighting the limitations of focusing on single cues or case elements in isolation. In the systematic review, formal moderation analyses were constrained by inconsistent reporting, but the included studies nevertheless demonstrated that the impact of RMA is context-dependent; effect sizes varied across scenarios involving, for example, intoxication, delayed reporting, and acquaintance versus stranger assaults. In the empirical study, the schizophrenia cue successfully increased perceived mental illness severity, yet this did not translate into systematic differences in credibility, sympathy, guilt ratings, or verdicts. Across both strands of the portfolio, the interaction between case features and pre-existing beliefs appears central. Attitudinal constructs provide a framework through which jurors interpret ambiguous or contested evidence, and this framework may be more or less engaged depending on how salient and explicitly signalled the case details are.

These integrated findings sit within, and contribute to, ongoing debates about the role of extra-legal beliefs in real juries. Empirical work with serving jurors in England and Wales, most notably by Cheryl Thomas, has been interpreted as suggesting that explicit rape-myth endorsement among jurors is relatively low and that many jurors reject overtly rape-supportive statements. In contrast, a growing body of commentary has highlighted methodological and interpretive limitations in this research and argued that it may understate the more subtle ways in which stereotypes and rape myths can shape the evaluation of evidence and credibility (Herriott, 2023; Leverick et al., 2025).

Finally, this portfolio extends previous work by bringing rape myths and mental health stigma into the same empirical framework. Dinos et al. (2015) highlighted how extra-legal attitudinal constructs such as RMA and related prejudices can shape juror decision-making, while also revealing substantial gaps in the literature regarding other forms of stigma. Taken together with broader work on mental illness stigma, this suggests a need for more systematic, theory-driven research into how jurors respond to victims and complainants with psychiatric diagnoses. Building on this, Levi and Golding (2024) provided initial evidence that victim mental health status can indirectly influence rape verdicts via perceived credibility and sympathy in a U.S. mock-juror sample. The present meta-analysis indicates that RMA remains an important factor in guilt-related decisions and victim blame in post-2012 rape trials, while the empirical study suggests that, in an English and Welsh context, relatively subtle cues to serious versus no mental illness may be less influential than jurors' existing political orientations, gender, and broader evaluative frameworks. Taken together, the two projects support a more integrated view in which rape myths, mental health stigma, ideology, and gender operate together to shape how jurors evaluate victims, defendants, and evidence in sexual offence trials.

Strengths and Limitations

The two projects have several strengths that enhance the contribution of the portfolio as a whole. The portfolio was structured so the systematic review updated Dinos et al. (2015) by focusing on post-2012 experimental studies of RMA, while the empirical project was conceived as a partial replication and extension of Levi and Golding's (2024) study in a different legal context. Methodologically, both studies centre on attitudinal constructs and legal decision-making in rape cases, and they are closely aligned in their emphasis on guilt-related outcomes and victim blame or responsibility judgements. This alignment strengthens the conceptual coherence of the portfolio and allows a more integrated examination of rape myths and mental

health stigma in juror decision-making as extra-legal factors than would be possible from either project in isolation.

The systematic review has a number of specific strengths. A transparent search strategy and predefined eligibility criteria were used to identify experimental and mock-juror studies that reported quantitative associations between RMA and guilt- or blame-related outcomes. The review focused on extracting effect sizes that could be synthesised meta-analytically, allowing the direction and magnitude of RMA-outcome relationships to be quantified rather than only summarised narratively. Quality appraisal using the AXIS tool indicated that most of the post-2012 studies were of relatively high methodological quality once items less applicable to mock-juror experiments were excluded from the total score. This suggests that, despite smaller and more heterogeneous effects than in earlier work, the association between higher RMA and more defendant-favourable, more victim-blaming decisions is not confined to a small number of weaker studies.

Nonetheless, there are several limitations to the systematic review. The decision to take a broad approach across rape and sexual assault scenarios enabled identification of an overall association between RMA, guilt and blame, but it restricted the extent to which potential case-type differences could be examined. In practice, the number of eligible studies within specific configurations (for example, intoxication, delayed reporting, or acquaintance assaults) was often small, which means that a more focused review of any one subtype would currently be constrained by the limited evidence base – highlighting a gap in the literature as much as a limitation of the present review.

Furthermore, the AXIS tool provided a transparent framework for appraising study quality, but it was developed for observational and cross-sectional designs more generally. Several items were only partially applicable to experimental mock-juror paradigms, and the tool does not capture features that are central in this area, such as the realism of trial materials, use of deliberation, or recruitment of jury-eligible samples. A quality appraisal instrument tailored specifically to experimental jury studies would therefore allow a more detailed assessment of methodological strengths and risks of bias. The review was also restricted to English-language, peer-reviewed publications from 2012 onwards, which raises the possibility of language and publication bias and means that relevant grey literature and earlier work may not have been captured. Finally, the evidence base was geographically narrow, with most included studies conducted in North America. This limits the extent to which the findings can be generalised to the legal context of England and Wales, where criminal procedure, jury service, and public

discourse about rape differ in important ways, and provides little insight into juror decision-making in non-Western jurisdictions with different legal, political, and gender-role norms.

There are also specific strengths within the empirical study. It adds to the limited mock-juror literature using jury-eligible adults from England and Wales and presents the rape case within the legal framework of the Sexual Offences Act 2003, which enhances relevance to this jurisdiction. The design was closely based on Levi and Golding's (2024) acquaintance-rape case and their manipulation of victim mental health status via medication information, which allows direct comparison on core features while testing the paradigm in a different legal and cultural context. The study extended this work by incorporating an explicit measure of stigma towards people with mental illness (APWMI; Kobau et al., 2010) and by examining political orientation, alongside gender and other demographics, as correlates of juror judgements. This combination of replication and extension supports comparison across legal contexts and helps to identify which effects do and do not generalise to this setting. Analytically, the use of hierarchical logistic and linear regression, together with planned mediation models, provided a structured way to examine both the impact of victim mental health status and the contribution of individual-difference variables to variation in verdicts and evaluative ratings.

At the same time, the empirical study has important limitations. Although the a priori power analysis indicated the achieved sample size was adequate for detecting medium-sized main effects, the study was likely underpowered to detect smaller effects, interactions, or more complex indirect pathways. The largely null findings for victim mental health condition should therefore be interpreted as an absence of evidence for sizeable effects in this context, rather than firm evidence that mental health cues are irrelevant. The manipulation of mental health status was also relatively subtle; information was conveyed via brief medication cues, and the manipulation check suggested that only the schizophrenia condition was clearly distinguished from the control condition in terms of perceived severity. This may have weakened the effective contrast between conditions and contributed to the absence of clear condition effects on verdicts and ratings, particularly for the borderline personality disorder condition. In addition, the APWMI captures general attitudes towards 'people with mental illness' rather than diagnosis-specific stigma, and its internal consistency in this sample was modest, which limits the strength of inferences about stigma effects.

More broadly, both projects share limitations that are characteristic of mock-juror research. Participants in the empirical study made decisions individually in an online survey, without group deliberation, face-to-face interaction, or the formal obligations associated with

real jury service. As with other online experiments, engagement with the materials could not be fully monitored, and participants were self-selecting, compensated, and aware their decisions carried no real-world consequences. These features raise questions about ecological validity and about the extent to which the observed patterns would generalise to actual jurors who are randomly summoned, obliged to attend, and embedded in a courtroom context (e.g., Diamond, 1997; Thomas, 2020). Similarly, the systematic review is constrained by the same underlying methodological issues in the primary studies it synthesises, including limited use of deliberating juries, reliance on written vignettes, and heavy use of Western, particularly North American, samples. Finally, the mediation analyses were conducted on cross-sectional data. Although the models were specified as if victim condition influenced perceptions, which in turn influenced verdicts, all variables were measured in a single session. The findings therefore indicate patterns that are consistent with the proposed pathways, but they cannot establish causality; designs that establish temporal ordering more clearly would be needed to test mediation more robustly.

Overall, these strengths and limitations suggest the portfolio provides a structured and relatively rigorous contribution to understanding how RMA, mental health stigma, political orientation, and gender relate to juror decision-making in rape cases, while also highlighting the constraints of the existing evidence base. The combination of a systematic review and a jurisdiction-specific replication and extension represents a valuable step in a field where real-jury research is rare and experimental paradigms are often highly heterogeneous. At the same time, the work draws attention to the need for future studies with larger and more diverse samples, richer measures of attitudinal constructs, and designs that move closer to real-world trial conditions.

Future Research

The portfolio highlights several directions for future research on juror decision-making in rape and sexual assault cases. For the systematic review, a key priority is to build a stronger evidence base that would allow case-type differences to be examined more systematically. The present review adopted a broad inclusion strategy across rape and sexual assault scenarios, which was necessary given the limited number of eligible post-2012 studies but meant that specific scenario features (such as intoxication, delayed reporting, or acquaintance versus stranger assaults) could not be synthesised separately. As more studies accumulate, future reviews could apply narrower inclusion criteria, focusing on particular contexts (for example, alcohol-involved assaults or delayed reporting) or specific jurisdictions or populations,

including reviews confined to England and Wales or to non-North American legal systems. In parallel, systematic reviews that take mental illness stigma, rather than RMA, as the primary attitudinal construct in rape or sexual assault trials would help to clarify how different belief systems operate across case types.

Methodologically, future systematic reviews would benefit from appraisal tools and reporting standards that are more closely aligned with experimental jury research. In the present review, the AXIS tool provided a clear and systematic way of assessing study quality, but several items were only partially applicable and it did not capture features that are central to mock-juror paradigms (e.g., presence and nature of deliberation). Developing a quality appraisal instrument designed specifically for experimental jury studies, alongside more standardised reporting of effect sizes and associations for core outcomes (e.g. guilt, credibility, blame, sympathy), would facilitate more robust synthesis and comparison across studies. Addressing geographical gaps in the evidence base is also important, as most work on RMA and juror decision-making has been conducted in North America, with relatively little empirical research in England and Wales or in non-Western legal systems with distinct legal and political contexts and norms.

The empirical study also highlights several priorities for future experimental work. A key step will be to replicate and extend the victim mental health manipulation using more varied and potentially stronger modes of presenting diagnostic information. In the present design, mental health status was signalled briefly via medication description; although the schizophrenia condition was rated as more severe than the control condition, this did not produce systematic differences in verdicts or evaluative ratings. Subsequent studies could directly compare implicit cues (e.g. medication type) with more explicit diagnostic labels, fuller descriptions of symptoms, or courtroom-style references to psychiatric history, and examine whether these formats differentially affect credibility, blame, and guilt judgements. It would also be valuable to include diagnosis-specific stigma measures (e.g. attitudes towards schizophrenia or borderline personality disorder) alongside generic mental-illness stigma scales, and to incorporate RMA, authoritarianism, and related constructs within the same analytic models. This would allow for a more direct examination of how distinct but overlapping attitudinal systems combine to shape decision-making in rape cases.

In terms of design and analysis, future studies should aim for larger samples with sufficient power to detect smaller effects, interactions, and indirect pathways. The present study was powered to identify medium-sized main effects, which constrains the conclusions

that can be drawn about more subtle relationships or interactions. Designs that establish temporal ordering more clearly would also strengthen inferences about mediation. For example, multi-stage procedures in which participants first form initial impressions, then provide ratings of credibility or perceived severity, and only subsequently deliver a verdict, or longitudinal designs tracking changes in perceptions over the course of more extended case materials, would allow stronger tests of hypothesised pathways between diagnostic information, perceptions, and outcomes. Where group deliberation or repeated assessments are incorporated, multilevel or mixed-effects models could be used to account for clustering within juries and within-person dependence over time.

More broadly, both the review and the empirical study highlight the need for research that more closely reflects real-world jury conditions. Most existing work, including the present empirical study, relies on individual decision-making based on written vignettes completed online. Future studies could develop this by using filmed or audio-recorded trial materials, recruiting samples that more closely resemble real jury panels, and incorporating structured group deliberations. Such designs would allow examination of how attitudes such as RMA, political orientation, and mental health stigma operate in group settings, including whether deliberation attenuates or amplifies their influence. Mixed-methods approaches that combine quantitative verdict data with qualitative analysis of deliberation transcripts or post-trial interviews would also be valuable for clarifying how mental health information and rape myths are referenced, contested, or endorsed in collective reasoning (e.g., Baker et al., 2022).

Future studies should also consider the possible role of social desirability more explicitly. In studies of rape myths, stigma, and juror decision-making, participants may be aware that some responses are more socially acceptable than others, particularly when the study topic makes prejudice relatively visible. This may suppress explicit endorsement of stigmatising or rape-supportive views and complicate interpretation of self-report measures and open-ended responses (Henderson et al., 2012; Corrigan et al., 2015). Future work could address this by considering more indirect or process-focused measures (such as sequential judgments or think-aloud procedures, which may provide a fuller picture of how stigma or rape-supportive beliefs enter juror reasoning) and, where theoretically justified, the inclusion of social desirability measures or instruments designed to reduce socially desirable responding in stigma research.

Finally, future research should seek to integrate experimental findings more directly with the experiences of rape and sexual assault victims with SMI in the criminal justice system.

Qualitative and participatory studies with complainants, legal professionals, and mental health practitioners could explore how concerns about credibility, stigma, and diagnostic labels are managed in practice, and how these concerns intersect with other social positions such as gender, ethnicity, and class. Integrating such work with experimental paradigms of the kind used in this portfolio would help to develop a more integrated account of how rape myths, mental health stigma, and broader ideological beliefs shape responses to sexual violence, and could more directly inform clinical, legal, and policy efforts to support victims with mental health conditions through the trial process.

Implications and Conclusions

Across both projects, the findings indicate that juror decision-making in rape cases is systematically associated with extra-legal attitudes as well as with the evidential features of a case. This has implications for how 'fair trial' is understood in practice, as it suggests that verdicts are shaped not only by the information presented in court and judicial directions, but also by the beliefs jurors bring with them. At the same time, the empirical study did not identify large, direct effects of briefly cued information about the complainant's mental health on verdicts or guilt ratings. In this paradigm, a short reference to the complainant's mental health status did not, on its own, lead to markedly less favourable outcomes, particularly where the case included relatively strong corroborative evidence. For victims with SMI, this suggests the presence of mental health information in a rape trial does not automatically result in more negative decisions, even though wider attitudinal frameworks around mental illness and sexual violence remain relevant.

From a theoretical perspective, the portfolio supports the view that rape myths and mental health stigma are embedded within broader attitudinal systems rather than operating as separate influences on decision-making. The systematic review showed that RMA continues to be reliably associated with more defendant-favourable responses and higher levels of victim blame in experimental rape and sexual assault trials (e.g., Devine & Caughlin, 2014; Grubb & Turner, 2012). The empirical study, in turn, indicated that political orientation, gender and, to a lesser extent, general stigma towards people with mental illness, were associated with how the same case was evaluated, even when mental health cues were brief and implicit. Taken together, these findings are consistent with accounts that conceptualise rape myths, stigma, and related beliefs as components of wider ideological frameworks organised around responsibility, social order, and the perceived legitimacy of existing hierarchies (e.g., Jost et al., 2004; Schweitzer et al., 2024). In this way, the portfolio adds to evidence that attitudinal

constructs such as RMA, authoritarianism, and system-justifying beliefs can be more informative predictors of guilt judgements than single case features (Devine & Caughlin, 2014).

Clinically, the findings underline the importance of attending to how victims anticipate and internalise stigma in the context of rape trials. Qualitative research with victims of crime who have mental health problems indicates that many report not being believed, being blamed, or fearing coercive responses (such as detention) when they seek help, which can contribute to expectations that their mental health information, treatment history, or psychiatric medication will undermine their credibility (Pettitt et al., 2013). Although the empirical study suggested that, in this specific paradigm, a brief cue to mental illness did not substantially reduce conviction rates or uniformly damage victim evaluations, broader political and attitudinal factors showed clearer associations with blame, credibility and sympathy. These findings do not imply that mental health information is harmless in all contexts, but they may provide a useful reference point in clinical conversations about what is, and is not, determinative of juror decision-making. Clinicians can support victims to distinguish between what the law formally requires (for example, the standard of proof and the lack of any requirement to conform to an 'ideal victim' stereotype) and how they fear jurors will respond, and to work directly with internalised mental health stigma and self-blame.

Alongside this, guidance on achieving best evidence in criminal proceedings emphasises the importance of preparing witnesses for the court process, reducing anxiety, and supporting them to give clear, accurate evidence (Courts and Tribunals Judiciary, 2025). This typically involves neutral information about what to expect, familiarisation with the courtroom environment, and opportunities to practise answering questions in a non-leading way (Home Office, 2002; Soanes, 2014). Drawing on similar principles, clinicians working with rape victims who have SMI might incorporate psychoeducation about common juror concerns (such as delayed reporting, apparent inconsistencies, or trauma-related memory difficulties) and supported rehearsal of giving evidence, with the aim of helping victims feel more prepared for court and less defined by their mental health condition when engaging with the legal process.

At a legal and policy level, the portfolio supports efforts to address extra-legal attitudes in rape trials, particularly in relation to rape myths and mental health. Experimental work continues to show that RMA is associated with more defendant-favourable responses and greater victim blame, even in post-2012 studies, while the empirical project indicates that political orientation and gender are linked to how a rape victim is evaluated.

These patterns are consistent with recent analyses of jury reasoning in sexual offence cases, which suggest that expectations about 'typical rape victims' can still surface in deliberations and shape how evidence is discussed and evaluated (e.g. Richardson et al., 2025). In practical terms, the findings align with proposals to strengthen juror education and guidance on sexual violence. Recent work has highlighted the potential value of clearer and more targeted judicial directions, pre-trial guidance, written aids for deliberation, and, where appropriate, expert evidence on trauma responses, aimed at helping jurors recognise and resist reliance on rape stereotypes when reaching verdicts (Hudspith et al., 2024; Law Commission, 2023; Richardson et al., 2025). The portfolio complements these proposals and suggests that policy responses focused solely on managing the presence or absence of psychiatric information may not be sufficient; and supports continued attention to how jurors are prepared and guided, and to how information about mental health is introduced and explained in court.

In summary, this thesis portfolio contributes to understanding how extra-legal beliefs shape juror decision-making in rape cases, with a particular focus on rape myths, mental health stigma, and related socio-political attitudes. The systematic review and meta-analysis indicate that RMA remains associated with more defendant-favourable and more victim-blaming judgements in contemporary experimental work, while the empirical study suggests that, in an English and Welsh context, subtly cued information about a complainant's mental health status does not necessarily exert large, direct effects on verdicts. However, political orientation, gender, and to a lesser extent, general attitudes towards people with mental illness were associated with how the same rape complainant was evaluated. Overall, these findings suggest the fairness of rape trials cannot be understood solely in terms of formal rules of evidence and procedure; it also depends on how jurors' beliefs, values, and expectations shape their interpretation of the cases they are asked to decide.

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Appendices

Appendix A: International Journal of Forensic Mental Health Author Guidelines

About the journal

Aims and scope

The International Journal of Law, Crime and Justice is an international and fully peer reviewed journal which welcomes high quality, theoretically informed papers on a wide range of fields linked to criminological research and analysis. It invites submissions relating to: studies of crime and interpretations of forms and dimensions of criminality; analyses of criminological debates and contested theoretical frameworks of criminological analysis; research and analysis of criminal justice and penal policy and practices; research and analysis of policing policies and policing forms and practices; research and analysis of criminal law and practice from a socio-legal perspective. W

We particularly welcome submissions relating to more recent and emerging areas of criminological enquiry including cyber-enabled crime, fraud-related crime, terrorism and hate crime.

Papers should be between 7,000-10,000 words in length, although shorter papers relating to policy analysis and debate will be considered. The peer review process and decision on publication will normally be completed within 90 days of receipt of submissions. The Editors also welcome proposals for Special Issues and Guest Editorships.

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Authorship

All authors should have made substantial contributions to all of the following:

1. The conception and design of the study, or acquisition of data, or analysis and interpretation of data.
2. Drafting the article or revising it critically for important intellectual content.
3. Final approval of the version to be submitted.

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The resulting Word document containing your declaration should be uploaded at the "attach/upload files" step in the submission process. It is important that the Word document is saved in the .doc/.docx file format. Author signatures are not required.

Funding sources

Authors must disclose any funding sources who provided financial support for the conduct of the research and/or preparation of the article. The role of sponsors, if any, should be declared in relation to the study design, collection, analysis and interpretation of data, writing of the report and decision to submit the article for publication. If funding sources had no such involvement this should be stated in your submission.

List funding sources in this standard way to facilitate compliance to funder's requirements:

Funding: This work was supported by the National Institutes of Health [grant numbers xxxx, yyyy]; the Bill & Melinda Gates Foundation, Seattle, WA [grant number zzzz]; and the United States Institutes of Peace [grant number aaaa].

It is not necessary to include detailed descriptions on the program or type of grants, scholarships and awards. When funding is from a block grant or other resources available to a university, college, or other research institution, submit the name of the institute or organization that provided the funding.

If no funding has been provided for the research, it is recommended to include the following sentence:

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Declaration of generative AI use

Authors must declare the use of generative AI in the manuscript preparation process upon submission of the paper.

Elsevier recognizes the potential of generative AI and AI-assisted technologies ("AI Tools"), when used responsibly, to help researchers work efficiently, gain critical insights fast and achieve better outcomes. Increasingly, these tools, including AI agents and deep research tools, are helping researchers to synthesize complex literature, provide an overview of a field or research question, identify research gaps, generate ideas, and provide tailored support for tasks such as content organization and improving language and readability.

Authors preparing a manuscript for this journal can use AI Tools to support them. However, these tools must never be used as a substitute for human critical thinking, expertise and evaluation. AI technology should always be applied with human oversight and control.

Ultimately, authors are responsible and accountable for the contents of their work. This includes accountability for:

- Carefully reviewing and verifying the accuracy, comprehensiveness, and impartiality of all AI-generated output (including checking the sources, as AI-generated references can be incorrect or fabricated).
- Editing and adapting all material thoroughly to ensure the manuscript represents the author's authentic and original contribution and reflects their own analysis, interpretation, insights and ideas.
- Ensuring the use of any tools or sources, AI-based or otherwise, is made clear and transparent to readers. If AI Tools have been used, we require a disclosure statement upon submission; please see example below.
- Ensuring the manuscript is developed in a way that safeguards data privacy, intellectual property and other rights, by checking the terms and conditions of any AI tool that is used.

Finally, authors must not list or cite AI Tools as an author or co-author on the manuscript since authorship implies responsibilities and tasks that can only be attributed to, and performed by, humans.

The use of AI Tools in the manuscript preparation process must be declared by adding a statement at the end of the manuscript when the paper is first submitted. The statement will appear in the published work and should be placed in a new section before the references list.

An example:

- Title of new section: *Declaration of generative AI and AI-assisted technologies in the manuscript preparation process.*
- Statement: *During the preparation of this work the author(s) used [NAME OF TOOL / SERVICE] in order to [REASON]. After using this tool/service, the author(s) reviewed and edited the content as needed and take(s) full responsibility for the content of the published article.*

The declaration does not apply to the use of basic tools, such as tools used to check grammar, spelling and references. If you have nothing to disclose, you do not need to add a statement.

Please read Elsevier's author policy on the use of generative AI and AI-assisted technologies, which can be found in the generative AI policies for journals.

Please note: to protect authors' rights and the confidentiality of their research, this journal does not currently allow the use of generative AI or AI-assisted technologies such as ChatGPT or similar services by reviewers or editors in the peer review and manuscript evaluation process, as is stated in Elsevier's generative AI policies for journals. Elsevier is actively evaluating compliant AI Tools and may revise this policy in the future.

Preprints

Preprint sharing

Authors may share preprints in line with Elsevier's article sharing policy. Sharing preprints, such as on a preprint server, will not count as prior publication.

We advise you to read our policy on multiple, redundant or concurrent publication.

Use of inclusive language

Inclusive language acknowledges diversity, conveys respect to all people, is sensitive to differences, and promotes equal opportunities. Authors should ensure their work uses inclusive language throughout and contains nothing which might imply one individual is superior to another on the grounds of: age; gender; race; ethnicity; culture; sexual orientation; disability or health condition

We recommend avoiding the use of descriptors about personal attributes unless they are relevant and valid. Write for gender neutrality with the use of plural nouns ("clinicians, patients/clients") as default. Wherever possible, avoid using "he, she," or "he/she."

No assumptions should be made about the beliefs of readers and writing should be free from bias, stereotypes, slang, reference to dominant culture and/or cultural assumptions.

These guidelines are meant as a point of reference to help you identify appropriate language but are by no means exhaustive or definitive.

Reporting sex- and gender-based analyses

There is no single, universally agreed-upon set of guidelines for defining sex and gender. We offer the following guidance:

- Sex and gender-based analyses (SGBA) should be integrated into research design when research involves or pertains to humans, animals or eukaryotic cells. This should be done in accordance with any requirements set by funders or sponsors and best practices within a field.
- Sex and/or gender dimensions of the research should be addressed within the article or declared as a limitation to the generalizability of the research.
- Definitions of sex and/or gender applied should be explicitly stated to enhance the precision, rigor and reproducibility of the research and to avoid ambiguity or conflation of terms and the constructs to which they refer.

We advise you to read the Sex and Gender Equity in Research (SAGER) guidelines and the SAGER checklist (PDF) on the EASE website, which offer systematic approaches to the use of sex and gender information in study design, data analysis, outcome reporting and research interpretation.

For further information we suggest reading the rationale behind and recommended use of the SAGER guidelines.

Definitions of sex and/or gender

We ask authors to define how sex and gender have been used in their research and publication. Some guidance:

- Sex generally refers to a set of biological attributes that are associated with physical and physiological features such as chromosomal genotype, hormonal levels, internal and external anatomy. A binary sex categorization (male/female) is usually designated at birth ("sex assigned at birth") and is in most cases based solely on the visible external anatomy of a newborn. In reality, sex categorizations include people who are intersex/have differences of sex development (DSD).
- Gender generally refers to socially constructed roles, behaviors and identities of women, men and gender-diverse people that occur in a historical and cultural context and may vary across societies and over time. Gender influences how people view themselves and each other, how they behave and interact and how power is distributed in society.

Depending on the focus of a paper, sex and/or gender may or may not be relevant to the content of the paper. We recognize that beliefs, attitudes, and laws relating to sex and gender may vary. These articles do not attempt to dictate author beliefs but rather require that, where relevant to an author's research or paper, the author must provide clear explanations of how the paper and research define and use sex and gender.

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- Institutional affiliations: Authors should use either the full, standard title of their institution or the standard abbreviation of the institutional name so that the institutional name can be independently verified for research integrity purposes.

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If you need to delay the publication of your article for any reason, please contact the editorial office of the journal or our Journal Article Publishing Support Center at the earliest possible opportunity. While we will try to ensure that your article is not published before a certain date if you make your request on a timely basis, we cannot guarantee this. Please also note that once the agreed period of delay has expired the article may be published at any time thereafter.

Writing and formatting

File format

We ask you to provide editable source files for your entire submission (including figures, tables and text graphics). Some guidelines:

- Save files in an editable format, using the extension .doc/.docx for Word files and .tex for LaTeX files. A PDF is not an acceptable source file.
- Format Word files in a single-column layout. Double-column formatting is only permitted for LaTeX submissions.
- Remove any strikethrough and underlined text from your manuscript, unless it has scientific significance related to your article.
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We advise you to read our Step-by-step guide to publishing with Elsevier.

Title page

You are required to include the following details in the title page information:

- Article title. Article titles should be concise and informative. Please avoid abbreviations and formulae, where possible, unless they are established and widely understood, e.g. DNA.
- Author names. Provide the given name(s) and family name(s) of each author. The order of authors should match the order in the submission system. Carefully check that all names are accurately spelled. If needed, you can add your name between parentheses in your own script after the English transliteration.
- Affiliations. Add affiliation addresses, referring to where the work was carried out, below the author names. Indicate affiliations using a lower-case superscript letter immediately after the author's name and in front of the corresponding address. Ensure that you provide the full postal address of each affiliation, including the country name and, if available, the email address of each author.
- Corresponding author. Clearly indicate who will handle correspondence for your article at all stages of the refereeing and publication process and also post-publication. This responsibility includes answering any future queries about your results, data, methodology and materials. It is important that the email address and contact details of your corresponding author are kept up to date during the submission and publication process.
- Present/permanent address. If an author has moved since the work described in your article was carried out, or the author was visiting during that time, a "present address" (or "permanent address") can be indicated by a footnote to the author's name. The address where the author carried out the work must be retained as their main affiliation address. Use superscript Arabic numerals for such footnotes.

Double anonymized peer review

As this journal follows a double anonymized review process, author identities are concealed from reviewers and vice versa. To facilitate this process, submit your title page (including author details) and anonymized manuscript (excluding author details) as separate files. Next to the items mentioned in the title page section of this guide, the title page should also include:

- Acknowledgements. Include any individuals who provided you with help during your research such as help with language, writing or proof reading in the acknowledgements section. Include

acknowledgements only in the title page, do not add them as a footnote to your title, or anywhere else in your article.

- Declaration of competing interests (when a separate declaration of interest file is not submitted)
- Corresponding author address (full address is required)
- Corresponding author email address

The anonymized manuscript should contain the main body of your paper, including references and tables. It is important that your manuscript does not contain any identifying information such as author names or affiliations, or acknowledgements.

Read more about peer review.

Abstract

You are required to provide a concise and factual abstract which does not exceed 250 words. The abstract should briefly state the purpose of your research, principal results and major conclusions. Some guidelines:

- Abstracts must be able to stand alone as abstracts are often presented separately from the article.
- Avoid references. If any are essential to include, ensure that you cite the author(s) and year(s).
- Avoid non-standard or uncommon abbreviations. If any are essential to include, ensure they are defined within your abstract at first mention.

Keywords

You are required to provide 1 to 7 keywords for indexing purposes. Keywords should be written in English. Please try to avoid keywords consisting of multiple words (using "and" or "of").

We recommend that you only use abbreviations in keywords if they are firmly established in the field.

Highlights

You are encouraged to provide article highlights at submission.

Highlights are a short collection of bullet points that should capture the novel results of your research as well as any new methods used during your study. Highlights will help increase the discoverability of your article via search engines. Some guidelines:

- Submit highlights as a separate editable file in the online submission system with the word "highlights" included in the file name.
- Highlights should consist of 3 to 5 bullet points, each a maximum of 85 characters, including spaces.

We encourage you to view example article highlights and read about the benefits of their inclusion.

Tables

Tables must be submitted as editable text, not as images. Some guidelines:

- Place tables next to the relevant text or on a separate page(s) at the end of your article.
- Cite all tables in the manuscript text.
- Number tables consecutively according to their appearance in the text.

- Please provide captions along with the tables.
- Place any table notes below the table body.
- Avoid vertical rules and shading within table cells.

We recommend that you use tables sparingly, ensuring that any data presented in tables is not duplicating results described elsewhere in the article.

Figures, images and artwork

Figures, images, artwork, diagrams and other graphical media must be supplied as separate files along with the manuscript. We recommend that you read our detailed artwork and media instructions. Some excerpts:

When submitting artwork:

- Cite all images in the manuscript text.
- Number images according to the sequence they appear within your article.
- Submit each image as a separate file using a logical naming convention for your files (for example, Figure_1, Figure_2 etc).
- Text graphics may be embedded in the text at the appropriate position. If you are working with LaTeX, text graphics may also be embedded in the file.

Figure captions

All images must have a caption. A caption should consist of a brief title (not displayed on the figure itself) and a description of the image. We advise you to keep the amount of text in any image to a minimum, though any symbols and abbreviations used should be explained.

Read how to add captions to your submission here.

Artwork formats

When your artwork is finalized, "save as" or convert your electronic artwork to the formats listed below taking into account the given resolution requirements for line drawings, halftones, and line/halftone combinations:

1. Vector drawings: Save as EPS or PDF files embedding the font or saving the text as "graphics."
2. Color or grayscale photographs (halftones): Save as TIFF, JPG or PNG files using a minimum of 300 dpi (for single column width: min. 1063 pixels, full page width: 2244 pixels).
3. Bitmapped line drawings: Save as TIFF, JPG or PNG files, using a minimum of 1000 dpi (for single column width: min. 3543 pixels, full page width: 7480 pixels).
4. Combinations bitmapped line/halftones (color or grayscale): Save as TIFF, JPG or PNG files using a minimum of 500 dpi (for single column: min. 1772 pixels, full page width: 3740 pixels).

Please do not submit:

- Files that are too low in resolution.
- Disproportionally large images compared to font size, as text may become unreadable.

Color artwork

If you submit usable color figures with your accepted article, we will ensure that they appear in color online.

Please ensure that color images are accessible to all, including those with impaired color vision. Learn more about color and web accessibility.

For articles appearing in print, you will be sent information on costs to reproduce color in the printed version, after your accepted article has been sent to production. At that stage you can indicate if your preference is to have color only in the online version of your article or also in the printed version.

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Please read our policy on the use of generative AI and AI-assisted tools in figures, images and artwork, which can be found in Elsevier's GenAI Policies for Journals. This policy states:

- We do not permit the use of Generative AI or AI-assisted tools to create or alter images in submitted manuscripts.
- The only exception is if the use of AI or AI-assisted tools is part of the research design or methods (for example, in the field of biomedical imaging). If this is the case, such use must be described in a reproducible manner in the methods section, including the name of the model or tool, version and extension numbers, and manufacturer.
- The use of generative AI or AI-assisted tools in the production of artwork such as for graphical abstracts is not permitted. The use of generative AI in the production of cover art may in some cases be allowed, if the author obtains prior permission from the journal editor and publisher, can demonstrate that all necessary rights have been cleared for the use of the relevant material, and ensures that there is correct content attribution.

Supplementary material

We encourage the use of supplementary materials such as applications, images and sound clips to enhance research. Some guidelines:

- Supplementary material should be accurate and relevant to the research.
- Cite all supplementary files in the manuscript text.
- Submit all supplementary materials at the same time as your manuscript.
- Include a concise, descriptive caption for each supplementary file, describing its content.
- After submission supplementary files can only be added or replaced in the revision stage of the editorial process.
- Be aware that all supplementary materials provided will appear online in the exact same way as received. These files will not be checked, formatted or typeset by the production team.

Video

This journal accepts video material and animation sequences to support and enhance your scientific research. We encourage you to include links to video or animation files within articles. Some guidelines:

- When including video or animation file links within your article, refer to the video or animation content by adding a note in your text where the file should be placed.
- Clearly label files ensuring the given file name is directly related to the file content.

- Provide files in one of our recommended file formats. Files should be within our preferred maximum file size of 150 MB per file, 1 GB in total.
- Provide "stills" for each of your files. These will be used as standard icons to personalize the link to your video data. You can choose any frame from your video or animation or make a separate image.
- Provide descriptive text in your manuscript to refer to the video content. This text helps ensure accessibility for visually impaired readers who rely on descriptive information. For journals publishing in print this is also essential, as video and animation files cannot be embedded in the print version.

We publish all video and animation files supplied in the electronic version of your article.

For more detailed instructions, we recommend that you read our guidelines on submitting video content to be included in the body of an article.

Research data

We are committed to supporting the storage of, access to and discovery of research data, and our research data policy sets out the principles guiding how we work with the research community to support a more efficient and transparent research process.

Research data refers to the results of observations or experimentation that validate research findings, which may also include software, code, models, algorithms, protocols, methods and other useful materials related to the project.

Please read our guidelines on sharing research data for more information on depositing, sharing and using research data and other relevant research materials.

Research data deposit, citation and linking

For this journal, **Option B** instructions from our research data guidelines apply. This means that you are **encouraged** to:

- Deposit your research data in a relevant data repository.
- Cite and link to this dataset in your article.
- If this is not possible, make a statement explaining why research data cannot be shared.

Data statement

To foster transparency, you are encouraged to state the availability of any data at submission.

Ensuring data is available may be a requirement of your funding body or institution. If your data is unavailable to access or unsuitable to post, you can state the reason why (e.g., your research data includes sensitive or confidential information such as patient data) during the submission process. This statement will appear with your published article on ScienceDirect.

Read more about the importance and benefits of providing a data statement.

Data linking

Linking to the data underlying your work increases your exposure and may lead to new collaborations. It also provides readers with a better understanding of the described research.

If your research data has been made available in a data repository there are a number of ways your article can be linked directly to the dataset:

- Provide a link to your dataset when prompted during the online submission process.
- For some data repositories, a repository banner will automatically appear next to your published article on ScienceDirect.
- You can also link relevant data or entities within the text of your article through the use of identifiers. Use the following format: Database: 12345 (e.g. TAIR: AT1G01020; CCDC: 734053; PDB: 1XFN).

Learn more about linking research data and research articles in ScienceDirect.

Research Elements

This journal enables the publication of research objects (e.g. data, methods, protocols, software and hardware) related to original research in Elsevier's Research Elements journals.

Research Elements are peer-reviewed, open access journals which make research objects findable, accessible and reusable. By providing detailed descriptions of objects and their application with links to the original research article, your research objects can be placed into context within your article.

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You are encouraged to publish a description of your research data, methods, or protocols related to your regular article as a co-submission article in *Data in Brief* or *MethodsX*.

This co-submission can be submitted at the same time as your regular manuscript submission to this journal. If both your regular and co-submitted manuscripts are accepted for publication, they will be linked together on ScienceDirect, thereby promoting research reproducibility, interoperability, and open science.

- Prepare a separate manuscript describing your research data, methods, or protocols using the **mandatory templates** from *Data in Brief* or in *MethodsX*.
- Submit this co-submission alongside your regular manuscript as a separate submission item; on the **'Attach files'** page in Editorial Manager, after which it will be automatically forwarded to the *Data in Brief* or *MethodsX* and assigned an editor.
- For associated Article Processing Charge (APC) details, please visit:
    - MethodsX
    - Data in Brief

Please note: Due to the automated process of submitting to *Data in Brief* or *MethodsX*, only after acceptance will you be presented with a personalized OA Article Publishing Charge based on your individual context (your country, institutional affiliation, and any society membership for example).

Please visit **co-submission Researcher Support page** for more information on how to submit a co-submission.

Article structure

Article sections

- Divide your article into clearly defined and numbered sections. Number subsections 1.1 (then 1.1.1, 1.1.2, ...), then 1.2, etc.
- Use the numbering format when cross-referencing within your article. Do not just refer to "the text."
- You may give subsections a brief heading. Headings should appear on a separate line.
- Do not include the article abstract within section numbering.

Glossary

Please provide definitions of field-specific terms used in your article, in a separate list.

Footnotes

We advise you to use footnotes sparingly. If you include footnotes in your article, ensure that they are numbered consecutively.

You may use system features that automatically build footnotes into text. Alternatively, you can indicate the position of footnotes within the text and present them in a separate section at the end of your article.

Author contributions: CRediT

Corresponding authors are required to acknowledge co-author contributions using CRediT (Contributor Roles Taxonomy) roles: conceptualization; data curation; formal analysis; funding acquisition; investigation; methodology; project administration; resources; software; supervision; validation; visualization; writing – original draft; writing – review and editing.

Not all CRediT roles will apply to every manuscript and some authors may contribute through multiple roles.

We advise you to read more about CRediT and view an example of a CRediT author statement.

Appendices

We ask you to use the following format for appendices:

- Identify individual appendices within your article using the format: A, B, etc.
- Give separate numbering to formulae and equations within appendices using formats such as Eq. (A.1), Eq. (A.2), etc. and in subsequent appendices, Eq. (B.1), Eq. (B. 2) etc. In a similar way, give separate numbering to tables and figures using formats such as Table A.1; Fig. A.1, etc.

References

References within text

Any references cited within your article should also be present in your reference list and vice versa. Some guidelines:

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- We recommend that you do not include unpublished results and personal communications in your reference list, though you may mention them in the text of your article.

- Any unpublished results and personal communications included in your reference list must follow the standard reference style of the journal. In substitution of the publication date add "unpublished results" or "personal communication."
- References cited as "in press" imply that the item has been accepted for publication.

Linking to cited sources will increase the discoverability of your research.

Before submission, check that all data provided in your reference list are correct, including any references which have been copied. Providing correct reference data allows us to link to abstracting and indexing services such as Scopus, Crossref and PubMed. Any incorrect surnames, journal or book titles, publication years or pagination within your references may prevent link creation.

We encourage the use of Digital Object Identifiers (DOIs) as reference links as they provide a permanent link to the electronic article referenced.

Reference style

All citations in the text should refer to:

- Single author: the author's name (without initials, unless there is ambiguity) and the year of publication.
- Two authors: both authors' names and the year of publication.
- Three or more authors: first author's name followed by 'et al.' and the year of publication.

Citations can be made directly (or parenthetically). Groups of references can be listed either first alphabetically, then chronologically, or vice versa. Examples: "as demonstrated (Allan, 2020a, 2020b; Allan and Jones, 2019)" or "as demonstrated (Jones, 2019; Allan, 2020). Kramer et al. (2023) have recently shown".

The list of references should be arranged alphabetically and then chronologically if necessary. More than one reference from the same author(s) in the same year must be identified by the letters 'a', 'b', 'c', etc., placed after the year of publication.

Abbreviate journal names according to the List of Title Word Abbreviations (LTWA).

Examples:

Reference to a journal publication:

Van der Geer, J., Handgraaf, T., Lupton, R.A., 2020. The art of writing a scientific article. J. Sci. Commun. 163, 51–59. https://doi.org/10.1016/j.sc.2020.00372.

Reference to a journal publication with an article number:

Van der Geer, J., Handgraaf, T., Lupton, R.A., 2022. The art of writing a scientific article. Heliyon. 19, e00205. https://doi.org/10.1016/j.heliyon.2022.e00205.

Reference to a book:

Strunk Jr., W., White, E.B., 2000. The Elements of Style, fourth ed. Longman, New York.

Reference to a chapter in a book:

Mettam, G.R., Adams, L.B., 2023. How to prepare an electronic version of your article, in: Jones, B.S., Smith, R.Z. (Eds.), Introduction to the Electronic Age. E-Publishing Inc., New York, pp. 281–304.

Reference to a website:

Cancer Research UK, 2023. Cancer statistics reports for the UK. http://www.cancerresearchuk.org/aboutcancer/statistics/cancerstatsreport/ (accessed 13 March 2023).

Reference to a dataset:

Oguro, M., Imahiro, S., Saito, S., Nakashizuka, T., 2015. Mortality data for Japanese oak wilt disease and surrounding forest compositions [dataset]. Mendeley Data, v1. https://doi.org/10.17632/xwj98nb39r.1.

Reference to software:

Coon, E., Berndt, M., Jan, A., Svyatsky, D., Atchley, A., Kikinzon, E., Harp, D., Manzini, G., Shelef, E., Lipnikov, K., Garimella, R., Xu, C., Moulton, D., Karra, S., Painter, S., Jafarov, E., & Molins, S., 2020. Advanced Terrestrial Simulator (ATS) v0.88 (Version 0.88) [software]. Zenodo. https://doi.org/10.5281/zenodo.3727209.

Web references

When listing web references, as a minimum you should provide the full URL and the date when the reference was last accessed. Additional information (e.g. DOI, author names, dates or reference to a source publication) should also be provided, if known.

You can list web references separately under a new heading directly after your reference list or include them in your reference list.

Data references

We encourage you to cite underlying or relevant datasets within article text and to list data references in the reference list.

When citing data references, you should include: author name(s); dataset title; data repository; version (where available); year; global persistent identifier.

Add [dataset] immediately before your reference. This will help us to properly identify the dataset. The [dataset] identifier will not appear in your published article.

Preprint references

We ask you to mark preprints clearly. You should include the word "preprint" or the name of the preprint server as part of your reference and provide the preprint DOI.

Where a preprint has subsequently become available as a peer-reviewed publication, use the formal publication as your reference.

If there are preprints that are central to your work or that cover crucial developments in the topic, but they are not yet formally published, you may reference the preprint.

Reference management software

Most Elsevier journals have their reference template available in popular reference management software products. These include products that support Citation Style Language (CSL) such as Mendeley Reference Manager.

If you use a citation plug-in from these products, select the relevant journal template and all your citations and bibliographies will automatically be formatted in the journal style. We advise you to remove all field codes before submitting your manuscript to any reference management software product.

If a template is not available for this journal, follow the format given in examples in the reference style section of this Guide for Authors.

Submitting your manuscript

Submission checklist

Before completing the submission of your manuscript, we advise you to read our submission checklist:

- One author has been designated as the corresponding author and their full contact details (email address, full postal address and phone numbers) have been provided.
- All files have been uploaded, including keywords, figure captions and tables (including a title, description and footnotes) included. This includes any supplementary materials and videos.
- Spelling and grammar checks have been carried out.
- All references in the article text are cited in the reference list and vice versa.
- Permission has been obtained for the use of any copyrighted material from other sources, including the Web.
- For open access articles, all authors understand that they are responsible for payment of the article publishing charge (APC) if the manuscript is accepted. Payment of the APC may be covered by the corresponding author's institution, or the research funder.

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Our online submission system guides you through the process steps of entering your manuscript details and uploading your files. The system converts your article files to a single PDF file used in the peer-review process.

Editable files (e.g., Word, LaTeX) are required to typeset your article for final publication. All correspondence, including notification of the editor's decision and requests for revision, is sent by email.

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Authors will be asked to complete a publishing agreement after acceptance. The corresponding author will receive a link to the online agreement by email. We advise you to read Elsevier's policies related to copyright to learn more about our copyright policies and your, and your employer’s/institution’s, additional rights for subscription and open access articles.

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Authors will be offered open access user license options which will determine how you, and third parties, can reuse your open access article. We advise that you review these options and any funding body license requirements before selecting a license option.

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Updated 11th February 2026

Appendix C: Final sample demographic characteristics

Table C1

Demographic characteristics of the sample (N = 130)

Variable		*N*	%
Age (continuous)	M = 37.98 (SD= 14.174), 18-76		
Gender			
	Male	85	65.4
	Female	40	30.8
	Other or PNS	5	3.8
Ethnicity			
	White British (or any other White background)	96	73.8
	Black or Black British, Caribbean or African	9	6.9
	Asian or Asian British	7	5.4
	Mixed/Multiple ethnic background	12	9.2
	Other or PNS	6	4.6
Political orientation			
	Left leaning	66	50.8
	Centre	35	26.9
	Right leaning	15	11.5
	Other or PNS	14	8.5

Appendix D: Trial Summary

This is a criminal trial for the alleged rape of Ms. Kimberly Fleming by Mr. Michael Turner. The Crown alleges that Mr. Michael Turner raped Ms. Kimberly Fleming in the afternoon of April 20th, 2020, at approximately 4:00 PM at her residence. At the time of the alleged rape, Ms. Fleming was 25 years old, and Mr. Turner was 28 years old. The prosecution charges Mr. Turner with Rape under Section 1 of the Sexual Offences Act 2003.

Mr. Turner pleads not guilty to the charge of Rape. The defence contends that Mr. Turner is a law-abiding individual with no previous criminal record and asserts that any sexual activity that occurred between them was consensual. They deny the use of force and argue that the encounter was a misunderstanding.

PROSECUTION'S CASE

Witness No. 1: Detective John Perry

Direct Examination by the Crown Prosecutor:

Detective Perry was assigned to the case after Ms. Fleming contacted the police on the afternoon of the alleged rape. He testified that upon arrival at Ms. Fleming's residence, she appeared dishevelled and visibly shaken, clutching a couch pillow. Her clothing was slightly unkempt, and there were visible bruises on her arms. Detective Perry took Ms. Fleming to the hospital for examination and evidence collection. He maintained custody of all evidence in the case.

Cross-Examination by the Defence Counsel:

Detective Perry acknowledged uncertainty regarding the cause of Ms. Fleming's dishevelled appearance, suggesting it could be due to fatigue or other activities earlier in the day. He also noted that the bruises could have resulted from accidental falls or other incidents.

PROSECUTION'S CASE

Witness No. 2: Ms. Kimberly Fleming

Direct Examination by the Crown Prosecutor:

Ms. Kimberly Fleming testified that on April 20th, 2020, she walked to a nearby pharmacy to pick up her prescription for depression medication. During the 20-minute walk, she encountered Mr. Michael Turner, who engaged her in conversation while they waited in line at the pharmacy. Mr. Turner accompanied Ms. Fleming on her walk home, continuing their conversation about recent films and the weather. Upon arriving at her residence approximately 20 minutes later, Ms. Fleming invited Mr. Turner inside. She described Mr. Turner's demeanour as friendly.

Direct Examination by the Crown Prosecutor:

Ms. Fleming recounted that around 3:50 PM, upon reaching her door, Mr. Turner approached her from behind, brandishing a knife and threatening her. He allegedly coerced her inside under threat of harm, where he proceeded to remove her clothing and sexually assault her. Ms. Fleming asserted that any resistance provoked Mr. Turner's anger, escalating the assault. She identified the weapon as a pocketknife. The assault lasted approximately 15 minutes, after which Mr. Turner left and Ms. Fleming immediately contacted the police.

Cross-Examination by the Defence Counsel:

Ms. Fleming admitted to being friendly and engaging in conversation with Mr. Turner during their interaction. She acknowledged that her friendliness might have been misinterpreted as flirtation.

DEFENCE'S CASE

Witness No. 1: Mr. Michael Turner

Direct Examination by the Defence Counsel:

Mr. Turner calmly testified that on April 20th, 2020, he left his apartment just before 3:00 PM to go to the pharmacy, arriving around 3:20 PM. He noticed Ms. Fleming in line and struck up a conversation, which continued as they walked to her residence. He claimed Ms. Fleming invited him inside, where consensual sexual intercourse occurred. Mr. Turner admitted the encounter was physically intense but denied using any threats or force, including the use of a knife.

Cross-Examination by the Crown Prosecutor:

Mr. Turner acknowledged that his extensive conversation with Ms. Fleming and his decision to accompany her home were voluntary. He conceded that Ms. Fleming's friendliness could have been misconstrued.

DEFENCE'S CASE

Witness No. 2: Mr. Paul Franklin

Direct Examination by the Defence Counsel:

Mr. Franklin, a neighbour of Ms. Fleming, witnessed Mr. Turner and Ms. Fleming conversing at the pharmacy. He testified that their interaction appeared normal and friendly, observing them for approximately 5 minutes before continuing his walk with his dog.

Cross-Examination by the Crown Prosecutor:

Mr. Franklin acknowledged the possibility that events could have unfolded differently after he left the scene.

PROSECUTION CLOSING ARGUMENTS

The Crown asserted that Mr. Turner must be convicted based on the evidence, particularly Ms. Fleming's testimony, which indicates non-consensual sexual intercourse occurred. The Crown emphasized that Ms. Fleming's initial friendliness does not imply consent for sexual activity.

DEFENCE CLOSING ARGUMENTS

The defence argued that the evidence does not establish Mr. Turner's guilt beyond a reasonable doubt. They highlighted inconsistencies in Ms. Fleming's testimony and argued that the encounter, however regrettable, did not constitute rape. They urged the jury to deliver a not guilty verdict.

PROSECUTION CLOSING ARGUMENTS

The Crown reiterated that Ms. Fleming's credible testimony corroborated by evidence supports Mr. Turner's guilt for Rape under Section 1 of the Sexual Offences Act 2003. They dismissed suggestions of fabrication or mistake, emphasizing the gravity of the crime and the need for justice.

JURY INSTRUCTIONS

Judge Albert Graham

Judge Graham instructed the jury as follows:

"You will find the Defendant guilty of Rape under Section 1 of the Sexual Offences Act 2003 if, and only if, you are satisfied beyond a reasonable doubt of the following:

A. That on or about April 20th, 2020, in this jurisdiction, Mr. Michael Turner engaged in sexual intercourse with Ms. Kimberly Fleming.

AND

B. That he did so using physical force or the threat of physical force, express or implied, placing Ms. Fleming in fear of immediate harm or sexual offence. The absence of physical resistance from Ms. Fleming does not negate this requirement."

Appendix E: Attitudes Towards Persons with Mental Illness (APWMI) scale

Please answer the following questions by circling your answer choice, where 1 = strongly disagree, and 5 = strongly agree.

1) I believe a person with mental illness is a danger to others

1	2	3	4	5
Strongly Disagree				Strongly Agree

2) I believe a person with mental illness is unpredictable

1	2	3	4	5
Strongly Disagree				Strongly Agree

3) I believe a person with mental illness is hard to talk with

1	2	3	4	5
Strongly Disagree				Strongly Agree

4) I believe a person with mental illness has only himself/herself to blame for his/her condition

1	2	3	4	5
Strongly Disagree				Strongly Agree

5) I believe a person with mental illness would improve if given treatment and support

1	2	3	4	5
Strongly Disagree				Strongly Agree

6) I believe a person with mental illness feels the way we all do at times

1	2	3	4	5
Strongly Disagree				Strongly Agree

7) I believe a person with mental illness could pull himself/herself together if he/she wanted

1	2	3	4	5
Strongly Disagree				Strongly Agree

8) I believe a person with mental illness can eventually recover

1	2	3	4	5
Strongly Disagree				Strongly Agree

9) I believe a person with mental illness can be as successful at work as others

1	2	3	4	5
Strongly Disagree				Strongly Agree

10) Treatment can help people with mental illness lead normal lives

1	2	3	4	5
Strongly Disagree				Strongly Agree

11) People are generally caring and sympathetic to people with mental illness

1	2	3	4	5
Strongly Disagree				Strongly Agree

Appendix F: Participant Information Sheet

Participant Information Sheet

Investigating the decision-making processes of mock jurors in a hypothetical rape case

(1) What is this study about?

You are invited to take part in this study looking into decision-making processes in a hypothetical rape case. We are recruiting participants who would make up a typical jury in England and Wales, to understand the decision-making processes involved in rape sentencing.

This Information Sheet outlines the study to help you decide whether you would like to take part, please read it carefully and raise any questions you may have. Your participation is voluntary and personal information that identifies you will not be collected. However, please be aware that once you agree to proceed and click submit, you will not be able to withdraw participation from the study.

By giving consent to take part in this study you are telling us that you:

- ✓ Understand what you have read.
- ✓ Agree to take part in the research study as outlined below.
- ✓ Understand that your personal information (e.g. your name) will not be collected.
- ✓ Understand that withdrawal from the study is not possible once you have pressed the 'submit' button
- ✓ You have received a copy of this Participant Information Statement to keep.

(2) Who is running the study?

This study is being conducted by: Kiah Bouillaut, ClinPsyD Researchers, Norwich Medical School, University of East Anglia. The primary research supervisor is Dr Peter Beazley, Deputy Programme Director & Senior Clinical Tutor at the UEA ClinPsyD. The secondary supervisor is Dr Lucy Fitton, Clinical Lecturer in Clinical Psychology at UEA.

(3) What will the study involve for me?

Your participation requires completion of an online survey, which has multiple sections and will take approximately 30 minutes. You will be provided information about a rape case, followed by case material. You will then be asked to complete a questionnaire measuring for your verdict and perceptions of the case.

(4) How much of my time will the study take?

The survey will take approximately 30 minutes to complete.

(5) Do I have to be in the study? Can I withdraw from the study once I've started?

Participation is voluntary, your decision whether to participate will not affect current or future relationships with anyone associated with the University of East Anglia. You can decline to participate any time prior to pressing the 'submit' button but are not able to withdraw once you have pressed 'submit'.

(6) Are there any risks or costs associated with being in the study?

This study may to cause some distress due to the sensitive nature of the topic, however you are advised to stop completing the survey if at any time you feel uncomfortable. If you complete the survey and then experience distress, please contact us by email (k.bouillaut@uea.ac.uk) to discuss issues of concern and signpost you to further support if needed. You can also contact your GP for mental health support. Samaritans offer a 24/7 listening service via 116 123.

(7) Are there any benefits associated with being in the study?

This study will hopefully provide insight into factors influencing sentencing of rape and contribute towards understanding and developing fairer processes in court.

(8) What will happen to information about me that is collected during the study?

By consenting to participate, you are agreeing to the information shared to be collected and used for the purpose of this research study. Any information provided will only be used for the purposes outlined in this Participant

Information Sheet unless you consent otherwise. The 2018 General Data Protection Regulation Act and the University of East Anglia Research Data Management Policy (2019) will be adhered to at all times. Your information will be stored securely and your identity/information will only be disclosed with your permission, except as required by law. Findings from this study may be included in publication, but you will not be identifiable. Data will be stored until analysis and publication are completed, then destroyed.

(9) What if I would like further information about the study?
When you have read this information, I will be available to discuss it with you further and answer any questions you may have. You can contact me via k.bouillaut@uea.ac.uk

(10) Will I be told the results of the study?
You have a right to receive feedback about the overall results of this study. You can request this by contacting me via k.bouillaut@uea.ac.uk. Overall results will be provided in the form of a one-page lay summary which you will receive after the study is finished.

(11) What if I have a complaint or any concerns about the study?
Should you need more information about the research study, please do not hesitate to contact Kiah Bouillaut at k.bouillaut@uea.ac.uk.

The ethical aspects of this study have been approved under the regulations of the University of East Anglia's School of Education and Lifelong Learning Research Ethics Committee. If you are concerned about the way this study is being conducted or you wish to make a complaint to someone independent from the study, please contact the administration team who will direct your concerns to a senior faculty member: med.reception@uea.ac.uk

(12) OK, I want to take part – what do I do next?
You need to click to confirm you have read this form before completing the online survey.

Appendix G: Participant Consent Form

Participant Consent Form

By acknowledging that I have read this consent form and clicking to proceed with the online survey, I agree to take part in this research study.

In giving my consent, I state that:

✓ I understand the purpose of the study, what I will be asked to do, and any risks/benefits involved.

✓ I have read the Participant Information Sheet and have been able to discuss my involvement in the study with the researchers if I wished to do so.

✓ The researchers have answered any questions that I had about the study and I am happy with the answers.

✓ I understand that being in this study is completely voluntary and I do not have to take part. My decision whether to be in the study will not affect my relationship with the researchers or anyone else at the University of East Anglia now or in the future.

✓ I Understand that withdrawal from the study is not possible once I have pressed the 'submit' button.

✓ I understand that I may stop the survey at any time if I do not wish to continue, and that unless I indicate otherwise any responses will then be erased and the information provided will not be included in the study. I also understand that I may refuse to answer any questions I don't wish to answer.

✓ I understand that any personal information about me that is collected over the course of this project will be stored securely and will only be used for purposes that I have agreed to. I understand that information about me will only be told to others with my permission, except as required by law.

✓ I consent to my anonymised data being stored indefinitely in a secure data repository and made publicly available for future research in accordance with UEA's Research Data Management Policy.

✓ I understand that the results of this study may be published, but these publications will not contain my name or any identifiable information about me.

Appendix H: Demographic Questions

Please answer the following demographic questions:

1. What is your gender

- Male
- Female
- Non-binary / third gender
- Other (please specify)
- Prefer not to say

2. What is your age (please type it in the box below)

3. What is your ethnicity

- White British (or any other White background)
- Black or Black British, Caribbean or African
- Asian or Asian British
- Mixed/Multiple ethnic background
- Other ethnic background (please specify)
- Prefer not to say

4. What is your political orientation

- Left-leaning
- Centre
- Right-leaning
- Other (please specify)
- Prefer not to say

Appendix I: Participant Debrief Sheet

Participant Debrief Sheet

Investigating the decision-making processes of mock jurors in a hypothetical rape case

Thank you for taking part in this study exploring juror decision-making processes and how a victim's mental health status may influence perceptions in a hypothetical rape case. We recognise that the subject of this study may be sensitive and could cause distress for some participants.

If you are feeling upset or distressed after participating, we encourage you to access support through the following resources:

- **Samaritans**: A confidential listening service available 24/7 at **116 123** or www.samaritans.org.
- **Rape Crisis England & Wales**: Support for victims of sexual violence. Visit www.rapecrisis.org.uk or call **0808 802 9999** (available daily).
- **Mind**: Mental health support and information (available Monday to Friday, 9am–6pm). Visit www.mind.org.uk or call **0300 123 3393**.
- **NHS Talking Therapies**: Free mental health support for adults in England. Visit Find an NHS talking therapies services - NHS for more information.

We hope you found the study engaging and valuable. If you have any questions or concerns about the research, please feel free to contact the research team at p.beazley@uea.ac.uk

Thank you again for your participation.

Appendix J: UEA FMH Ethical Approval

University of East Anglia
Norwich Research Park
Norwich. NR4 7TJ

Email: ethicsmonitor@uea.ac.uk
Web: www.uea.ac.uk

Study title: How victim mental health status impacts juror decision-making in a rape case: A partial replication study with an English and Welsh sample

Application ID: ETH2425-0020

Dear Kiah,

Your application was considered on 6th February 2025 by the FMH S-REC (Faculty of Medicine and Health Sciences Research Ethics Subcommittee).

The decision is: **approved**.

You are therefore able to start your project subject to any other necessary approvals being given.

This approval will expire on **31st March 2026**.

Please note that your project is granted ethics approval only for the length of time identified above. Any extension to a project must obtain ethics approval by the FMH S-REC (Faculty of Medicine and Health Sciences Research Ethics Subcommittee) before continuing.

It is a requirement of this ethics approval that you should report any adverse events which occur during your project to the FMH S-REC (Faculty of Medicine and Health Sciences Research Ethics Subcommittee) as soon as possible. An adverse event is one which was not anticipated in the research design, and which could potentially cause risk or harm to the participants or the researcher, or which reveals potential risks in the treatment under evaluation. For research involving animals, it may be the unintended death of an animal after trapping or carrying out a procedure.

Any amendments to your submitted project in terms of design, sample, data collection, focus etc. should be notified to the FMH S-REC (Faculty of Medicine and Health Sciences Research Ethics Subcommittee) in advance to ensure ethical compliance. If the amendments are substantial a new application may be required.

Approval by the FMH S-REC (Faculty of Medicine and Health Sciences Research Ethics Subcommittee) should not be taken as evidence that your study is compliant with the UK General Data Protection Regulation (UK GDPR) and the Data Protection Act 2018. If you need guidance on how to make your study UK GDPR compliant, please contact the UEA Data Protection Officer (dataprotection@uea.ac.uk).

Please can you send your report once your project is completed to the FMH S-REC (fmh.ethics@uea.ac.uk).

I would like to wish you every success with your project.

On behalf of the FMH S-REC (Faculty of Medicine and Health Sciences Research Ethics Subcommittee)

Yours sincerely,

Dr Paul Linsley

Appendix K: Regression model outputs for victim and perpetrator outcomes

Table K1.

Hierarchical linear regression models predicting victim-related judgments from victim mental health status, stigma, and demographics

	Model 1: Condition only	Model 2: Model 1 + APWMI	Model 3: Model 2 + Demographics
Dependent Variable: Guilt Rating			
R^2	.024	.031	.096
Overall Model	F(2,113)=1.366, p=.259	F(3,112)=1.194, p=.315	F(7,108)=1.640, p=.132
Condition (BPD vs Control)	B=8.231, SE=6.476, β=.137, t=1.271, p=.206	B=8.396, SE=6.483, β=.140, t=1.295, p=.198	B=6.993, SE=6.404, β=.116, t=1.092, p=.277
Condition (Schizophrenia vs Control)	B=-1.682, SE=6.234, β=-.029, t=-.270, p=.788	B=-.447, SE=6.379, β=-.008, t=-.070, p=.944	B=-1.162, SE=6.360, β=-.020, t=-.183, p=.855
Stigma Measure (APWMI)		B=.494, SE=6.483, β=.088, t=.924, p=.358	B=.672, SE=.540, β=.120, t=1.246, p=.215
Age			B=-.082, SE=.190, β=-.042, t=-.433, p=.666
Gender			B=9.031, SE=5.419, β=.154, t=1.667, p=.098
Political Orientation			B=-6.763, SE=3.568, β=-.181, t=-1.896, p=.061
Ethnicity			B=-2.265, SE=6.383, β=-.035, t=-.355, p=.723
	Model 1: Condition only	**Model 2: Model 1 + APWMI**	**Model 3: Model 2 + Demographics**
Dependent Variable: Victim Credibility Composite			
R^2	.007	.008	.118
Overall Model	F(2,113)=.417, p=.660	F(3,112)=.293, p=.830	F(7,108)=2.059, p=.054
Condition (BPD vs Control)	B=1.289, SE=5.642, β=.025, t=.228, p=.820	B=1.324, SE=5.668, β=.025, t=.234, p=.816	B=-.447, SE=5.467, β=-.009, t=-.082, p=.935
Condition (Schizophrenia vs Control)	B=-3.519, SE=5.431, β=.025, t=.228, p=.820	B=-3.255, SE=5.578, β=-.065, t=-.584, p=.561	B=-4.431, SE=5.429, β=-.089, t=-.816, p=.416
Stigma Measure (APWMI)		B=.106, SE=.468, β=.022, t=.226, p=.816	B=.360, SE=.461, β=.074, t=.781, p=.436
Age			B=-.039, SE=.162, β=-.023, t=-.240, p=.811
Gender			B=6.794, SE=4.626, β=.134, t=1.469, p=.145
Political Orientation			B=-9.407, SE=3.045, β=-.291, t=-3.089, p=.003**
Ethnicity			B=-3.811, SE=5.449, β=-.068, t=-.699, p=.486

	Model 1: Condition only	Model 2: Model 1 + APWMI	Model 3: Model 2 + Demographics
Dependent Variable: Victim Blame Composite			
R^2	.029	.093	.247
Overall Model	F(2,113)=1.658, p=.195	F(3,112)=3.828, p=.012*	F(7,108)=5.073, p=<.001***
Condition (BPD vs Control)	B=-7.588, SE=4.512, β=-.181, t=-1.682, p=.095	B=-7.249, SE=4.380, β=-.173, t=-1.655, p=.101	B=-5.656, SE=4.081, β=-.135, t=-1.386, p=.169
Condition (Schizophrenia vs Control)	B=-1.075, SE=4.343, β=-.027, t=-.247, p=.805	B=1.475, SE=4.311, β=.036, t=.342, p=.733	B=2.780, SE=4.053, β=.069, t=.686, p=.494
Stigma Measure (APWMI)		B=1.020, SE=.361, β=.261, t=2.822, p=.006**	B=.770, SE=.344, β=.197, t=2.240, p=.027*
Age			B=.113, SE=.121, β=.083, t=.932, p=.353
Gender			B=-8.749, SE=3.454, β=-.214, t=-2.533, p=.013*
Political Orientation			B=6.962, SE=2.274, β=.266, t=3.062, p=.003**
Ethnicity			B=7.299, SE=4.068, β=.161, t=1.794, p=.076
	Model 1: Condition only	**Model 2: Model 1 + APWMI**	**Model 3: Model 2 + Demographics**
Dependent Variable: Victim Sympathy			
R^2	.015	.017	.118
Overall Model	F(2,113)=.882, p=.417	F(3,112)=.635, p=.594	F(7,108)=2.068, p=.053
Condition (BPD vs Control)	B=7.346, SE=5.565, β=.143, t=1.320, p=.189	B=7.407, SE=5.589, β=.144, t=1.325, p=.188	B=5.821, SE=5.413, β=.113, t=1.075, p=.285
Condition (Schizophrenia vs Control)	B=4.239, SE=5.357, β=.086, t=.791, p=.430	B=4.692, SE=5.500, β=.095, t=.853, p=.395	B=3.989, SE=5.376, β=.081, t=.742, p=.460
Stigma Measure (APWMI)		B=.181, SE=.461, β=.038, t=.392, p=.696	B=.394, SE=.456, β=.082, t=.864, p=.389
Age			B=-.134, SE=.160, β=-.080, t=-.836, p=.405
Gender			B=6.390, SE=4.581, β=.128, t=1.395, p=1.66
Political Orientation			B=-8.331, SE=3.016, β=-.260, t=-2.763, p=.007**
Ethnicity			B=-3.341, SE=5.395, β=-.060, t=-.619, p=.537
	Model 1: Condition only	**Model 2: Model 1 + APWMI**	**Model 3: Model 2 + Demographics**
Dependent Variable: Victim Anger			
R^2	.034	.053	.091
Overall Model	F(2,113)=1.995, p=.141	F(3,112)=2.100, p=.104	F(7,108)=1.537, p=.163
Condition (BPD vs Control)	B=-5.982, SE=4.520, β=-.142, t=-1.324, p=.188	B=-5.796, SE=4.497, β=-.137, t=-1.289, p=.200	B=-5.116, SE=4.508, β=-.121, t=-1.135, p=.259
Condition (Schizophrenia vs Control)	B=2.708, SE=4.351, β=.067, t=.622, p=.535	B=4.104, SE=4.425, β=.101, t=.927, p=.356	B=5.306, SE=4.477, β=.131, t=1.185, p=.239
Stigma Measure (APWMI)		B=.558, SE=.371, β=.142, t=1.505, p=.135	B=.387, SE=.380, β=.098, t=1.018, p=.311

Age			B=.019, SE=.134, β=.014, t=.141, p=.888
Gender			B=1.560, SE=3.815, β=.038, t=.409, p=.683
Political Orientation			B=3.079, SE=2.511, β=.117, t=1.226, p=.223
Ethnicity			B=8.149, SE=4.493, β=.179, t=1.814, p=.073
	Model 1: Condition only	**Model 2: Model 1 + APWMI**	**Model 3: Model 2 + Demographics**
Dependent Variable: Victim Memory			
R^2	.014	.019	.059
Overall Model	F(2,113)=.827, p=.440	F(3,112)=.718, p=.544	F(7,108)=.973, p=.455
Condition (BPD vs Control)	B=3.640, SE=6.642, β=.059, t=.548, p=.585	B=3.771, SE=6.659, β=.061, t=.566, p=.572	B=2.462, SE=6.669, β=.040, t=.369, p=.713
Condition (Schizophrenia vs Control)	B=-4.645, SE=6.394, β=.059, t=.548, p=.585	B=-3.668, SE=6.553, β=-.062, t=-.560, p=.577	B=-5.237, SE=6.623, β=-.089, t=-.791, p=.431
Stigma Measure (APWMI)		B=.391, SE=.549, β=.068, t=.712, p=.478	B=.621, SE=.562, β=.109, t=1.105, p=.272
Age			B=.061, SE=.198, β=.031, t=.309, p=.758
Gender			B=4.996, SE=5.643, β=.084, t=.885, p=.378
Political Orientation			B=-6.300, SE=3.715, β=-.165, t=-1.696, p=.093
Ethnicity			B=-5.936, SE=6.647, β=-.089, t=-.893, p=.374

Note. All coefficients are from the final model (Step 3) of each hierarchical regression. APWMI = Attitudes towards Persons With Mental Illness; higher scores indicate greater stigma. *p < .05. **p < .01. ***p < .001.

Table K2.

Hierarchical linear regression models predicting perpetrator-related judgments from victim mental health status, stigma, and demographics

	Model 1: Condition only	Model 2: Model 1 + APWMI	Model 3: Model 2 + Demographics
Dependent Variable: Perpetrator Credibility Composite			
R^2	.023	.042	.114
Overall Model	F(2,113)=1.328, p=.269	F(3,112)=1.650, p=.182	F(7,108)=1.991, p=.063
Condition (BPD vs Control)	B=-1.557, SE=5.903, β=-.028, t=-.264, p=.793	B=-1.314, SE=5.873, β=-.024, t=-.224, p=.823	B=-.420, SE=5.777, β=-.008, t=-.073, p=.942
Condition (Schizophrenia vs Control)	B=7.129, SE=5.683, β=.135, t=1.254, p=.212	B=8.951, SE=5.779, β=.170, t=1.549, p=.124	B=8.651, SE=5.737, β=.164, t=1.508, p=.134
Stigma Measure (APWMI)		B=.729, SE=.484, β=.143, t=1.505, p=.135	B=.652, SE=.487, β=.128, t=1.340, p=.183
Age			B=.279, SE=.171, β=.157, t=1.629, p=.106
Gender			B=-6.606, SE=4.888, β=-.161, t=-1.760, p=.081
Political Orientation			B=3.576, SE=3.218, β=.105, t=1.111, p=.269
Ethnicity			B=1.380, SE=5.758, β=.023, t=.240, p=.811
	Model 1: Condition only	**Model 2: Model 1 + APWMI**	**Model 3: Model 2 + Demographics**
Dependent Variable: Perpetrator Blame Composite			
R^2	.009	.013	.065
Overall Model	F(2,113)=.509, p=.602	F(3,112)=.473, p=.701	F(7,108)=1.065, p=.391
Condition (BPD vs Control)	B=4.523, SE=6.819, β=.072, t=.663, p=.509	B=4.643, SE=6.840, β=.074, t=.679, p=.499	B=3.435, SE=6.809, β=.055, t=.504, p=.615
Condition (Schizophrenia vs Control)	B=-2.110, SE=6.565, β=-.035, t=-.321, p=.748	B=-1.211, SE=6.731, β=-.020, t=-.180, p=.858	B=-1.134, SE=6.762, β=-.019, t=-.168, p=.867
Stigma Measure (APWMI)		B=.360, SE=.564, β=.061, t=.637, p=.525	B=.487, SE=.574, β=.083, t=.848, p=.398
Age			B=-.125, SE=.202, β=-.061, t=-.618, p=.538

Gender			B=2.380, SE=5.762, β=.039, t=.413, p=.680
Political Orientation			B=-7.794, SE=3.793, β=-.199, t=-2.055, p=.042*
Ethnicity			B=1.456, SE=6.787, β=.021, t=.214, p=.831
	Model 1: Condition only	**Model 2: Model 1 + APWMI**	**Model 3: Model 2 + Demographics**
Dependent Variable: Perpetrator Sympathy			
R^2	.038	.038	.057
Overall Model	F(2,113)=2.223, p=.113	F(3,112)=1.495, p=.220	F(7,108)=.936, p=.482
Condition (BPD vs Control)	B=-4.051, SE=5.302, β=-.082, t=-.764, p=.446	B=-4.011, SE=5.326, β=-.081, t=-.753, p=.453	B=-3.566, SE=5.394, β=-.072, t=-.661, p=.510
Condition (Schizophrenia vs Control)	B=6.683, SE=5.104, β=.140, t=1.309, p=.193	B=6.984, SE=5.241, β=.146, t=1.332, p=.185	B=7.168, SE=5.357, β=.150, t=1.338, p=.184
Stigma Measure (APWMI)		B=.120, SE=.439, β=.026, t=.274, p=.785	B=.084, SE=.454, β=.018, t=.184, p=.854
Age			B=-.021, SE=.160, β=-.013, t=-.130, p=.897
Gender			B=-5.665, SE=4.564, β=-.117, t=-1.241, p=.217
Political Orientation			B=1.969, SE=3.005, β=.064, t=.655, p=.514
Ethnicity			B=-1.290, SE=5.376, β=-.024, t=-.240, p=.811
	Model 1: Condition only	**Model 2: Model 1 + APWMI**	**Model 3: Model 2 + Demographics**
Dependent Variable: Perpetrator Anger			
R^2	.031	.038	.132
Overall Model	F(2,113)=1.834, p=.165	F(3,112)=1.473, p=.226	F(7,108)=2.338, p=.029*
Condition (BPD vs Control)	B=12.902, SE=8.412, β=.165, t=1.534, p=.128	B=13.104, SE=8.424, β=.167, t=1.555, p=.123	B=11.827, SE=8.187, β=.151, t=1.445, p=.151
Condition (Schizophrenia vs Control)	B=-1.737, SE=8.098, β=-.023, t=-.214, p=.831	B=-.223, SE=8.291, β=-.003, t=-.027, p=.979	B=1.133, SE=8.131, β=.015, t=.139, p=.889
Stigma Measure (APWMI)		B=.606, SE=.695, β=.083, t=.872, p=.385	B=.617, SE=.690, β=.084, t=.894, p=.373

Age			B=-.228, SE=.243, β=-.090, t=-.942, p=.348
Gender			B=13.004, SE=6.928, β=.170, t=1.877, p=.063
Political Orientation			B=-7.766, SE=4.561, β=-.159, t=-1.703, p=.091
Ethnicity			B=10.404, SE=8.160, β=.123, t=1.275, p=.205

Note. All coefficients are from the final model (Step 3) of each hierarchical regression. APWMI = Attitudes towards Persons With Mental Illness; higher scores indicate greater stigma. *p < .05. **p < .01. ***p < .001.