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When myths shape verdicts: A theory and intervention for jury biases against victim-survivors in rape trials

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ABSTRACT

This paper examines a trio of studies which examine the role and malleability of rape myths in juror's victim evaluations and their verdict decisions. The first two studies aimed to identify and test a newly-developed theoretical model to understand how rape myths impact juror verdict decisions. Utilizing a trial transcript methodology, Study 1 examined the proposed model using structural equation modelling, and Study 2 tested the validity of the model when the level of rape-myth-congruent information in the trial was varied experimentally. Results found that the proposed model showed good fit to the data, supporting the theory that rape myth factors influence credibility, responsibility and empathy judgements, which subsequently impact verdicts. Study 2 demonstrated that the theoretical model was robust to changes in the trial content, with the high rape-myth-congruency condition resulting in significantly fewer guilty verdicts. The final study aimed to assess whether the impact of rape myths on jurors' evaluations of the victim could be reduced. Based on the theoretical model, an educational intervention video was developed and tested. Results from Study 3 suggested that the intervention was successful in reducing the influence of rape myths on credibility judgements and verdict decisions. Overall, these studies provide a coherent model which identifies how rape myths influence juror decisions, as well as a successful intervention which can reduce their impact. These resources provide opportunities for further research, including expanding the theoretical model and utilizing the intervention to reduce rape myths biases in jurors and other criminal justice decision-makers.

The high rate of attrition in sexual assault and rape cases is a widely recognized problem (Hohl & Stanko, 2015; Munro & Kelly, 2009; Murphy et al., 2022). Over a decade ago, Daly and Bouhours (2010) examined the consistent drops in rape cases from reporting to prosecution to conviction, in Australia, Canada, England and Wales, Scotland, and the United States. While the authors expressed concern that the average conviction rate for sexual offences had declined to 12.5%, recent years have seen a drastic expansion of the justice gap, to the point that such statistics would now be exemplary. According to the Australian Bureau of Statistics (2022a, 2022b) between 2010 and 2019, there were 221,751 recorded victim-survivors of sexual assaults compared to 14,519 guilty charges in sexual assault cases during the same period. In the UK, the End-to-End Rape review revealed, despite yearly increases in the number of cases reported to the police, in 2021 just 1.6% of reported rapes resulted in a charge (HM Government, 2021). While in the US, Hoffman et al.'s (2025) systematic review of sexual assault case outcomes found that while the majority of reported cases warranted investigation by police, most cases dropped out prior to arrest. On

average, 27% of cases resulted in arrest, with 31% of cases referred to prosecutors, of which 20% were charged, and subsequently just 8% yielded a conviction. Further confounding this problem are the numerous barriers faced by victim-survivors—such as self-blame, knowing the perpetrator and a lack of trust in the justice system—with evidence suggesting fewer than one in six victim-survivors of sexual violence will report their experience to police (Daly & Bouhours, 2010; Wieberneit et al., 2024).

These statistics exemplify prosecutors' concerns regarding juries' reluctance to convict in cases of serious sexual assault. UK Labour MP Ann Coffey perfectly described the knock-on effect demonstrated by these statistics:

A perfect storm is developing where juries are reluctant to convict young men charged with rape, the CPS are therefore reluctant to prosecute, and the police are therefore reluctant to refer. The result of this is that victims will stop coming forward, and justice will be denied (Topping, 2018, para. 5).

What is most alarming, is that Coffey's statement mirrors the same

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sentiment expressed in a quote from [Lonsway and Fitzgerald's \(1994\)](#) review - "It is the fear of what a jury will think that drives survivors not to report, police to refuse to arrest in 'futile' cases, and prosecutors to dismiss prosecutions as unwinnable" ([Senate Judiciary, 1993](#), p. 38); demonstrating the problem still exists 30 years later. Throughout this paper, we have used the term "victim-survivor" when discussing people who have experienced sexual violence. The dichotomy of associations for the terms "victim" and "survivor" has been well-established ([Papendick & Bohner, 2017](#)); thus, we used the combined term of "victim-survivor", given the validity of each identity.

1. Rape myths

One factor which has been repeatedly implicated in juries' reluctance to convict in cases of sexual assault is the acceptance of rape myths. [Gerger et al. \(2007, p.426\)](#) provide one of the most widely cited definitions for rape myths as: "descriptive or prescriptive beliefs about rape (i.e., about its causes, context, consequences, perpetrators, victim-survivors, and their interaction) that serve to deny, downplay or justify sexual violence that men exert against women". It should be noted that while statistics suggest female victim-survivors of male perpetrators is by far the most common scenario of sexual violence, it is imperative to acknowledge that men and non-binary persons can also be sexually victimized by men or women, and these victim-survivors face unique and equally problematic rape myths ([Willmott & Widanaralage, 2024](#)). While further research into these overlooked populations is strongly encouraged, the focus of this paper remains on female victim-survivors.

[Bohner et al. \(2009\)](#) outline four categories of rape myths, namely: myths that doubt allegations, myths that blame victim-survivors, myths that disqualify events as rape and myths which exonerate the perpetrator. The authors also provide a detailed examination of the various psychological functions of rape myths. They propose that rape myths can be conceptualized as a general cognitive schema which organizes and guides how individuals interpret information about rape. [Bohner et al. \(2009\)](#) note that schematic influences are particularly strong when "external facts are uninformative or ambiguous" (p.24), which is often the case in cases of rape where physical evidence is either entirely lacking or unhelpful in determining whether consent was obtained. Additionally, rape myths can be connected to just world beliefs which refers to the tendency for individuals to believe the world is generally fair and that people get what they deserve ([Bohner et al., 2009](#); [Lerner, 1980](#)). Accepting that anyone, including innocent people, can be victim-survivors of sexual violence challenges this belief, and so to avoid cognitive dissonance, individuals may place blame on the victim-survivor ([Vonderhaar & Carmody, 2015](#)).

This explanation is closely related to one of the gendered purposes of rape myths, which is to protect women's self-esteem and sense of security. In a series of studies, [Bohner et al. \(1993, 2004\)](#) found that the more women endorse rape myths, the less threatened and vulnerable they feel about the possibility of their own victimization. Finally, rape myths also provide a rationalization for men's sexually aggressive tendencies ([Bohner et al., 2009](#)). By normalizing male sexual entitlement, minimizing the harm of rape and denial of the perpetrator's responsibility through victim-blaming, rape myths enable perpetrators to avoid perceiving their actions as a criminal offence. A considerable amount of research has focused on the relationships between RMA, sexual aggression and sexual coercion, with [Suarez and Gadalla's \(2010\)](#) meta-analysis finding a significant relationship, and more recent longitudinal research suggesting causality between the acceptance of rape myths and rape proclivity ([O'Connor, 2020](#)). Furthermore, one of the most consistent and strongest relationships with RMA is gender, with men displaying significantly higher endorsement of rape myths compared to women ([Suarez & Gadalla, 2010](#)).

2. The effect of rape myths

Unsurprisingly, the negative effects of rape myths on victim-survivors of sexual violence are extensive. Research has shown that rape myths not only inform the general public's response to rape victim-survivors, but they are also internalized by victim-survivors resulting in self-blame, shame and anticipatory stigma ([Anderson & Overby, 2021](#); [Kennedy & Prock, 2016](#)). Research has also found RMA moderates the impact of rape acknowledgement on mental health outcomes, such that acknowledged rape victim-survivors with high rape myth endorsement reported worse outcomes compared to unacknowledged rape victim-survivors ([Wilson et al., 2018](#)). In light of the increasing awareness of how damaging rape myths can be, various efforts have been made to dispel them; including dedicated resources providing information about the realities of rape from numerous organizations ([Amnesty International, 2021](#); [Crown Prosecution Service, 2021](#)), as well as, educational campaigns and social movements such No More ([Hestia, 2026](#)), #MeToo ([Brookes, 2018](#)) and I Just Froze ([Adams, 2017](#)).

Problematically, prevalence studies such as the YouGov survey conducted by the [End Violence Against Women \(2018\)](#), highlight how ingrained rape myths are in society, despite best efforts to dispel rape myths by various organizations. The YouGov survey found concerning levels of RMA in the representative sample of 3922 adults in Great Britain, including 33% of the sample agreeing "It isn't usually rape if a woman is pressured into having sex but there is no physical violence" ([End Violence Against Women, 2018](#)). In addition, a content analysis study of tweets on the topics of rape and sexual assault over a 4-month period found 2% of the 1.3 million tweets about rape and sexual assault referred to false allegations or deceitful complaints, including terms such as lying, fake or fabricated, for an average of 28,000 tweets per month ([Stabile et al., 2019](#)). A subset of these tweets was further analyzed, revealing 44–48% were accusatory (i.e., expressed doubts about or undermined allegations). This clearly demonstrates the relevance of the *victim's lie* rape myth factor in social discourse. It is unsurprising rape myths are still affecting our criminal justice system when research indicates that lawyers ([Krahé et al., 2008](#)), police officers ([Parratt & Pina, 2017](#)), prosecutors ([Munro & Kelly, 2009](#)) and judges ([Temkin et al., 2018](#)) have not only demonstrated that they possess these biases, but also utilize them in their decision-making regarding sexual assault and rape cases.

3. Jury decision-making

As previously noted, few rape cases successfully navigate the criminal justice process to end up at trial, with fewer still achieving a conviction. Undoubtedly, rape cases present unique challenges for juries considering the often private nature of the incident and the centrality of consent, which is rarely evidenced by physical evidence or eyewitness testimony ([Centre for Women's Justice, End Violence Against Women coalition, Imkaan, and Rape Crisis England, and Wales, 2020](#); [Larcombe et al., 2016](#)). Various legislative criteria for rape, from the inclusion of proving the defendant did not have "reasonable belief" in consent within the UK ([Smith & Skinner, 2017](#)), to the numerous European countries which require victim-survivors to prove the use of force or threat ([Amnesty International, 2021](#)), further exacerbate the evidential challenges in proving rape cases. Rape trials typically involve conflicting accounts from the complainant and defendant, where the main issue is not whether sexual contact occurred, but whether it was consensual ([Temkin & Krahé, 2008](#)). This shifts the focus from factual dispute to interpretation of subjective experiences, complicating jurors' assessments and facilitating judgement based on prejudicial and stereotypical views ([Carline & Gunby, 2011](#)).

A considerable amount of research has suggested that RMA in jurors in particular poses a barrier to achieving justice in sexual assault cases ([Gillen, 2019](#); [Temkin, 2010](#)). Observation studies such as [Smith and Skinner's \(2017\)](#) suggested rape myths were used extensively

throughout rape and sexual offence trials, particularly by defense lawyers in attempts to discredit victim-survivors. Meta-analyses have also found a significant impact of RMA on jury decision making, with [Leverick's \(2020\)](#) review of 28 quantitative and six qualitative studies finding “overwhelming evidence” (p.256) that rape myths affect jury decision-making. These results suggest jurors' level of RMA is far from trivial. Furthermore, research has indicated that traditional quantitative methods may not be capturing the true level of RMA due to issues surrounding social desirability. For example, a study by [Zidenberg et al. \(2021\)](#) found that despite low levels of self-reported acceptance of rape myths on the Updated Illinois Rape Myth Acceptance (U-IRMA) scale, participants' qualitative responses to a fictitious police report of sexual coercion included notable endorsement of each of the four rape myth factors.

Regarding jury decision-making, the literature consistently demonstrates that high levels of RMA result in lower rates of guilty verdicts ([Dinos et al., 2015](#); [Leverick, 2020](#)). However, to date, the literature has not provided a coherent model explaining how RMA impacts individual juror decision-making. If educating jurors and other members of the criminal justice system is imperative, as [Gillen \(2019\)](#) suggests, it is essential to first understand the process by which rape myths impact decision making. Furthermore, as highlighted by [Willmott et al. \(2021\)](#), research into jury decision-making is often characterized by several methodological limitations—thus, it is imperative to replicate trial processes as closely as is feasible. This leads to the aims of this paper which are to develop and examine a new hypothetical model of how rape myths impact juror verdict decisions (Study 1). Additionally, we aim to examine the robustness of the model (Study 2) and test the malleability of rape myths impact within juror decision making (Study 3) utilizing a methodology that is high in ecological validity.

More specifically, our first study aims to identify and consolidate key variables in verdict decisions in rape trials, to provide a broad and accessible theoretical model framework upon which further research can be built and connected. Rape cases are complex and idiosyncratic in terms of the events of the alleged offences, as well as the strengths/weaknesses of available evidence. Rape myths may also influence the course of a rape case at any point along its timeline, from the situational context of the perpetration of the sexual offence, to the likelihood of prosecution proceeding, to the outcome of the case itself. It is beyond the scope of this research to map all of these processes. Instead, we focus on how jurors may form opinions around the victim-survivor in a rape case, which may inform the verdict they render and may reflect pre-existing rape myths.

4. Existing models of jury decision-making

To date, the literature has only offered a handful of structural equation models for jury decision-making specific to sexual assault cases. One of the earliest and most influential models proposed was [Schuller and Hastings' \(2002\)](#) integrative model of guilt assessments, which posited that prior sexual history between the complainant and defendant, along with RMA, predicts complainant credibility ratings, which in turn predict participants' perception of the defendant's guilt. Using [Ward's \(1988\)](#) Attitudes Towards Rape Victims Scale to measure RMA, results indicated gender and RMA indirectly influenced guilt assessments through perceptions of complainant's credibility, perceptions of the defendant's belief in consent and the likelihood of complainant's consent. [Schuller and Hastings' \(2002\)](#) study provided solid groundwork for the development of our proposed model, with advances in programs for structural equation modelling allowing us to utilize dichotomous dependent variables.

Another model which provides an insight into participants' evaluations of sexual assault complainants is [Miller et al.'s \(2011\)](#) path model of victim-survivor culpability attributions. While the model omits verdict decisions, it clearly illustrates that women's personal history of sexual assault predicted their ratings of similarity to the victim-survivor

depicted in a vignette. This in turn predicted ratings of victim-survivor empathy, which subsequently predicted participants' RMA, which directly predicted participants' ratings of victim-survivor blameworthiness and responsibility. While the culpability model demonstrated excellent model fit, it is necessary to note that the model was based on a sample comprised of just 69 female undergraduate students.

There are also a handful of structural models predicting verdict decisions, which exclude RMA, such as [Sperry and Siegel's \(2013\)](#) model of attribution. Their model, based on [Weiner's \(1980\)](#) attribution model, posits that perceptions of a person's level of responsibility will result in less sympathy and reduced willingness to help the person. [Sperry and Siegel \(2013\)](#) found that ratings of complainant responsibility negatively predicted sympathy for the complainant, which simultaneously predicted their willingness to help the complainant and the complainant's perceived credibility, which ultimately predicted verdict decisions. However, contrary to their hypothesis, sympathy did not mediate the relationship between perceptions of complainant responsibility and verdict decisions. Instead, a direct path from responsibility accounted for 22% of the variance in verdict. Overall, the model demonstrated good levels of fit ($CFI = .92$, $SRMR = 0.08$), although it is unfortunate that the impact of rape myths was excluded in the final structural equation model, after controlling for it in previous analyses.

Similarly, the Juror Decision Scale by [Willmott et al. \(2018\)](#) provided further evidence for the importance of credibility evaluations, with confirmatory factor analyses revealing three factors, namely complainant believability, defendant believability and decision confidence (controlling for a general factor). When the model was investigated within the parameters of a mock jury rape trial, significant relationships were found between high complainant believability scores with a) lower RMA scores and b) higher rates of guilty verdicts, both pre- and post-deliberation. Overall, the Juror Decision Scale provides useful insights into jury decision-making and may indirectly account for some of the influence of rape myths on verdicts, although it does not provide a way to directly measure the impact of RMA within jury decisions.

5. Proposed model specification

Based on the models above and the overall literature in jury decision-making in sexual assault trials, we developed a hypothetical model where pre-existing rape myth schemas bias how jurors evaluate the case through judgements of credibility, responsibility and empathy, which ultimately predict verdicts. Overall, the model represents a schema → evaluation → decision sequence, consistent with cognitive and socio-legal theories of juror reasoning. However, in our research into these factors, it was evident that the terminology and measurement varied considerably. Credibility is often referred to as “believability” or “trustworthiness” (e.g., [Harsey & Freyd, 2020](#); [Klettke et al., 2016](#)) and the terms “blame” and “culpability” are used interchangeably with responsibility (e.g., [van der Bruggen & Grubb, 2014](#)). Additionally, there was little standardization in the measurements for these constructs, which included single items (see: [Rollero & Tartaglia, 2019](#); [Ryan & Westera, 2018](#); [Stewart & Jacquin, 2010](#)) and multiple items (see: [Angelone et al., 2015](#); [Krahé et al., 2008](#); [Lynch et al., 2013](#); [Wenger & Bornstein, 2006](#)) rated on Likert scales, ranging from 5 to 10 options. While these variables are often examined in relation to both the victim-survivor and the perpetrator, jurors' evaluations of defendants and complainants are typically measured as conceptually distinct judgement processes. With judgements about each party treated as separate outcomes even when they both contribute to the final verdict decision ([Lynch et al., 2013](#); [McKimmie et al., 2014](#)).

We decided to focus on victim-survivor credibility, victim-survivor responsibility and empathy towards the victim-survivor. This was partially due to the added complexity and associated recruitment costs of including perpetrator evaluations within the model. However, the conceptual link from victim-focused rape myth factors to the victim-survivor evaluation factors (outlined below), offers a straightforward

theoretical underpinning to the model. Including perpetrator evaluations would mix two theoretically distinct judgement pathways, increasing model complexity without improving theoretical precision. As noted by Thompson (2000), more parsimonious models are preferable to almost just-identified models, therefore, excluding perpetrator evaluations supports theoretical parsimony, not merely methodological convenience. Furthermore, we argue there is merit in adopting a victim-centric approach—evaluations of victim-survivors provide meaning beyond the contrast they offer to defendant evaluations; particularly considering that rape myths and those who endorse them typically focus more on the actions and characteristics of victim-survivors relative to perpetrators (Süssenbach et al., 2017).

In light of this, only the victim-focused rape myth factors were included in the model (i.e., *victims lie*, *victims ask for it* and *not really rape*). These rape myth factors were taken from the “She Lied”, “She Asked for It” and “Not Really Rape” factors in McMahon and Farmer’s (2011) U-IRMA scale (which is based on the short-form of Payne et al.’s (1999) Illinois Rape Myth Acceptance scale). It must be noted that the majority of the literature utilize rape myth scales as a unidimensional measure, despite the establishment of distinct rape myth factors in Payne et al.’s (1999) original paper. Rollero and Tartaglia (2019) study examined the relationship between the *victims ask for it* factor and attributions of responsibility specifically, although they aggregated the number of items for each subscale to reduce the number of variables in the model, whereas we chose to keep all the relevant subscale items and specified covariance between the latent factors. To the best of our knowledge, the only other study which has examined the individual rape myth factors predictive ability for verdicts and evaluations of the victim-survivor is Pica et al. (2021). Using Payne et al.’s (1999) full Illinois Rape Myth Acceptance scale, Pica et al. (2021) found that endorsement of each of the seven original rape myth factors predicted not-guilty verdicts, in addition to each factor predicting overall negative perceptions of the victim.

Looking at the content of the rape myth factors, there is clear alignment from the decision-making factors (credibility, responsibility and empathy) with victim-focused rape myth factors. The *victims lie* factor is primarily concerned with deception and false accusations, with items such as “rape accusations are often used as a way of getting back at guys”. Acceptance of such beliefs directly constrains individuals believing victim-survivors’ testimony and thus directly influences credibility judgements. Rollero and Tartaglia (2019) previously established that high scores on the *victims ask for it* factor are a significant positive predictor of victim-survivor responsibility judgements. Considering the

items for this factor focus on blaming victim-survivors’ behavior as the cause of the assault, responsibility judgements reflect the core outcome of causal attribution processes.

The *not really rape* factor is concerned with the stereotypical so-called “real rape” scenario, which depicts a woman who gets injured by fighting back while being raped by a stranger with a weapon (Estrich, 1987). While the scenario is unsupported by statistics, which suggest women are most likely to be raped by someone they know (Office for National Statistics, 2021) and that freezing is a common response (Möller et al., 2017), this scenario is often most compatible with the general public’s schemas about rape and rape victim-survivors (Crown Prosecution Service & Equally Ours, 2024). Krahé (2016) notes the prescriptive nature of this stereotype can result in victim-survivors who do not meet the criteria of the “real rape” scenario not being identified as legitimate or genuine victim-survivors. In line with Burgoon’s (1978) expectancy-violation theory, when a complainant does not match this schema, jurors experience lower empathic alignment and reduced identification. In addition, some items relate to threat and harm, which could lead to increased sympathy for the victim-survivor.

The proposed model, illustrated in Fig. 1, encapsulated these connections as pathways, where the three rape myth factors lead to their respective decision-making factors. It was anticipated that these three pathways would subsequently predict verdict decisions. Given the strong evidence connecting RMA to verdict decisions, we expected significant indirect paths from the rape myth factors to verdict, mediated by their respective evaluative factor, in addition the direct paths (shown below in dashed lines) from the rape myth factors to verdict also remaining significant. While cross-factor effects, such as *victims ask for it* influencing empathy evaluations, are theoretically plausible, the model was specified to reflect the distinct primary processes represented by each rape myth factor. The inclusion of cross-factor paths would have resulted in an unfocused, over-specified model that obscured the core mechanisms of the model and complicated interpretation. Thus, we chose to model covariances between the rape myth factors to capture the shared attitudinal variance (rather than direct cross-factor paths) in order to preserve theoretical clarity and parsimony.

Furthermore, due to the strong correlations between the evaluative factors, we also anticipated significant pathways between credibility, responsibility and empathy. However, relations among these judgements have been conceptualized inconsistently in the literature, with some accounts treating these evaluations as parallel outcomes (Pica et al., 2021; Stewart & Jacquin, 2010) and others proposing varying directional dependencies among them (Ask, 2018; Cook & Lane, 2015;

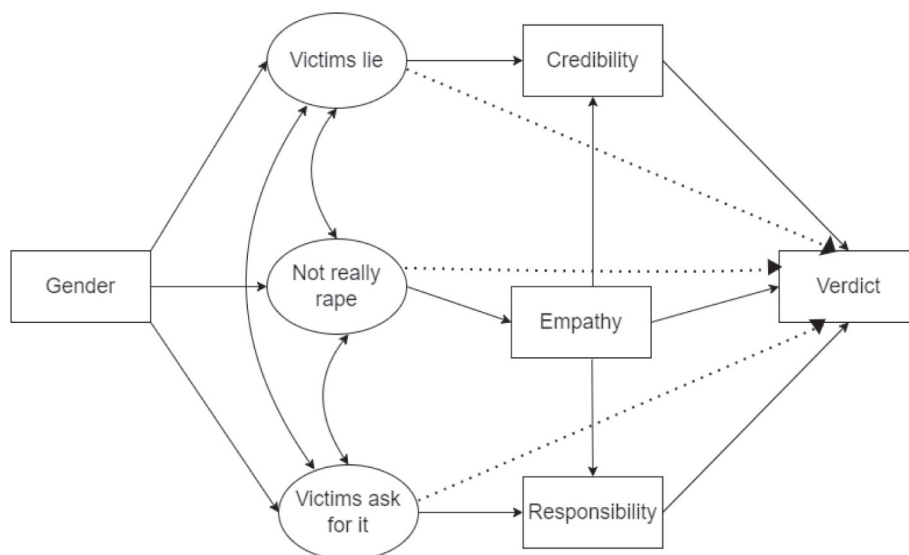


Fig. 1. Proposed Structural Model of Jury Decision-Making in Rape Cases.

Miller et al., 2011; Rinehart et al., 2023; Sperry & Siegel, 2013; Venema, 2019). Given this theoretical indeterminacy, we therefore evaluated theoretically plausible alternatives (including alternative directionality and covariances in the paths between judgements) and prioritized a process-oriented specification in which empathy informs subsequent responsibility and credibility evaluations, consistent with affect-as-information and appraisal-based models of judgement in which emotional reactions shape evaluations of credibility and attributional judgements under ambiguity (Lerner et al., 2015; Schwarz & Clore, 2007). This approach was preferred over modelling undirected covariances, as the latter avoids explicit theoretical commitment but offers limited insight into decision-making processes and resulted in unstable or inadmissible solutions across studies. The selected specification demonstrated stable estimation, theoretical coherence, and replicability across studies, whereas alternative specifications did not. We emphasize that this model should be interpreted as one plausible representation of the judgement process under the present experimental conditions, rather than a definitive resolution of causal ordering among these constructs.

It is highly probable that various other elements (such as the strength of evidence presented in the case or the demographic/ attitudinal characteristics of jurors) inform evaluations of the complainant and influence verdict decisions. In particular, we also acknowledge that the rape myth factors themselves are likely predicted by (or systematically vary by) gender—as such we also specified gender as a predictor for each of the rape myth latent factors. Of course, research has long demonstrated notable gender differences in verdicts for sexual assault cases, with women more likely to vote guilty (Schutte & Hosch, 1997; Thompson & Pozzulo, 2025). Gender differences have also been shown to influence evaluations of victim-survivors—research from Nason et al. (2019) found that men rated the victim-survivor as significantly less believable and had lower empathy towards the victim-survivor, compared to women. Hockett et al.'s (2016) meta-analysis also highlighted that men consistently attribute more blame to rape victim-survivors. However, some research suggests these gender differences may be partially explained by differences in attitudes such as sexism and rape myth acceptance (Angelone et al., 2020). Thus, by adding gender as a predictor of the rape myth factors, the indirect effect of gender on credibility, responsibility, empathy and verdicts can be examined.

While it was not feasible to incorporate every relevant variable to the judgement of rape cases by jurors within the model, the details and results of the alternative models which were considered and tested are included within the supplemental materials. In order to maintain a parsimonious and sufficiently statistically powered model, the proposed model will focus exclusively on the impact of rape myths on victim-survivor evaluations and verdict decisions. Fig. 1 illustrates the proposed theoretical model, for which the following hypotheses were specified:

- H1a.** Credibility will mediate the relationship between *victims lie* and verdict.
- H1b.** Responsibility will mediate the relationship between *victims ask for it* and verdict.
- H1c.** Empathy will mediate the relationship between *not really rape* and verdict.
- H2.** Credibility and responsibility will be predicted by empathy.
- H3.** Verdict will be predicted by credibility, responsibility, empathy and the three rape myth factors (*victims lie*, *victims ask for it*, and *not really rape*).
- H4.** The rape myth factors *victims lie*, *victims ask for it*, and *not really rape* will each be predicted by gender.

6. Study 1 methodology

6.1. Participants

Based on the 10:1 ratio proposed by (Schreiber et al., 2006), which specified for each estimated parameter in a structural equation model, there should be 10 observations, it was anticipated that a total sample of 360 participants were necessary for SEM. The *semPower* package (Moshagen & Bader, 2023) was not available at the time of data analysis. However, post-hoc power analysis of the proposed model indicated no concerns. Participants were recruited using a combination of Prolific, an online participant recruitment website, and snowballing methods on social media. Eligibility criteria for the study included being over 18 years old and residing in a country with consent-based sexual assault laws (namely: UK, Ireland, Germany, Iceland, Luxembourg, Belgium, Cyprus and Sweden). These criteria were selected (rather than full juror eligibility) to maximize the number of potential participants while minimizing different legal interpretations of the transcript. The transcript was designed so that neither legal specialization nor experience with jury service was required; instead, participants made judgements based on lay interpretations of the case, consistent with standard practice in mock-jury research.

Using Prolific, 251 participants were recruited, of which 238 were compensated £1 for their participation in line with Prolific's fair-pay threshold.¹ An additional 165 individuals were recruited through social media, including Facebook, Twitter, Instagram and Reddit. Participants recruited from social media were not compensated and 49 participants were excluded for less than 95% survey completion or completing the survey too quickly. Additionally, three cases which failed attention checks and six cases recognized as multivariate outliers (using Mahalanobis distance) were subsequently excluded, leaving a total sample of 345 participants for analysis. As noted by Ullman (2013), most SEM estimation methods assume multivariate normality, and while robust estimation methods such as DWLS can correct for non-normality, these methods are not robust to multivariate outliers (Savalei, 2014).

In the total sample, 63% identified as cisgender female and 38% identified as cisgender male. The median age was 27 years ($M = 29.86$, $SD = 9.70$), 82% of participants identified as heterosexual and 83% reported their ethnicity as White/Caucasian. Furthermore, 37% of the sample acknowledged reported knowing a victim-survivor of rape, while 8% acknowledged knowing someone who had been falsely accused of rape. A full breakdown of these demographics is represented in Appendix A.

6.2. Design

The study employed a non-experimental design to establish and evaluate the proposed model.

6.3. Measures and procedure

Participants were provided the link to access the survey on Qualtrics, where they were first presented with an information sheet which informed them of what the study would involve, their right to withdraw and the confidentiality of their data. Participants were asked basic demographic questions on gender, age, sexual orientation and ethnicity. Participants were also asked whether they knew a) anyone who was a victim-survivor of rape and b) anyone who was falsely accused of rape, given that such experiences may shape attitudes towards sexual assault,

¹ Prolific participants who did not complete the survey or completed the study too quickly (more than three SD from the mean), were not compensated. The data for this study was collected prior to April 2022, at which time Prolific's fair-pay threshold was £5 per hour. Thus, the average of £5.15 per hour was in line with Prolific's payment standards.

reporting these variables descriptively helped contextualize the main findings. Next, participants were asked to read a five-page fictitious rape trial transcript, which had been previously piloted to assess it was suitably balanced to achieve sufficient variability in verdicts—88% of the sample ($n = 42$) provided guilty verdicts in the first pilot, followed by 84% of the sample ($n = 44$) in the second pilot. The third pilot study ($n = 20$) found a rate of 74% guilty verdicts—this transcript was retained for the main study as a sufficient representation of guilty and not-guilty verdicts were achieved for stable estimation of a binary outcome within the planned analytic framework, and these results were combined with the final dataset for Study 1. The final pilot study indicated that 85% of the participants strongly agreed the situation was realistic, with the remaining 15% agreeing somewhat.

The trial follows the format of a trial in England and Wales and involves a fictional case of a female college student who reported she was raped at a house party by a male peer. The case characteristics, while not intended to be representative of all sexual assault cases, align with several features that are statistically common in reported cases. For example, the majority of rape cases involve acquaintances rather than strangers (Murphy et al., 2022; Office for National Statistics, 2021), and factors such as alcohol consumption (Australian Bureau of Statistics, 2022b; Hohl & Stanko, 2015) and delayed reporting (Hohl & Stanko, 2015; Quilter et al., 2022) are frequently present, whereas factors such as physical resistance and injury evidence are uncommon (Möller et al., 2017; Murphy et al., 2022). The case therefore offered a context in which rape myths are likely to be salient, while still reflecting circumstances that occur regularly in practice. The transcript starts with the charges against the defendant, followed by the judge presenting the undisputed facts of the case. The trial begins with the prosecution's opening statement and questioning of the witness for the prosecution (i.e., the complainant). Following this, the defense cross-examines the witness and subsequently presents their opening statement. The defense then presents their evidence-in-chief, followed by cross-examination of their witness (i.e., the defendant) by the prosecution. In the closing statements, the prosecution argues that the defendant pressured the complainant into having sex and ignored her telling him to stop, while the defense maintains the complainant willingly engaged but falsely accused the defendant of rape after regretting her actions. The case ends with a brief summary by the judge. The full transcript is available in the supplementary materials.

Following the trial transcript, participants were asked to imagine themselves on the jury and to deliver a verdict of *guilty* (1) or *not guilty* (0) based on the evidence provided and to rate their level of confidence in their verdict (from 0% to 100%). To examine a more sensitive measure of guilt, participants were also asked to rate the likelihood of the defendant's guilt on a 7-point Likert-type scale from *completely unlikely* (1) to *completely likely* (7). Participants were also asked how many years (from 0 to 10) the defendant should be sentenced—given jurors are not responsible for sentencing in real cases, the question purposefully omits sentencing guidelines to capture more subjective perceptions of the seriousness of the case and the culpability of the defendant. Nonetheless, the 0–10-year sentence range provided encompasses the most plausible sentence range for the case scenario, based on the Sentencing Council (n.d.) guidelines.

Victim-survivor credibility, victim-survivor responsibility and empathy towards the victim-survivor were all assessed using single items, i.e., “To what extent do you find the complainant (Claire) to be credible/believable?”, “To what extent do you find the complainant (Claire) is to blame for the events that took place?” and “How much do you empathize with the complainant (Claire)?”. Although multi-item measures exist for related constructs, no widely adopted, validated scales capture victim credibility, responsibility, or target-specific empathy; as such, the use of single-item measures is consistent with a substantial body of mock-jury research (Chalmers et al., 2021; Ellison & Munro, 2010; Hudspeth et al., 2024). We therefore prioritized theoretically precise, face-valid items. Credibility was measured on a 7-point

Likert-type scale from *completely unbelievable* (1) to *completely believable* (7). Responsibility was measured on a 7-point Likert-type scale from *completely irresponsible* (1) to *completely responsible* (7). Finally, empathy was measured on a 5-point Likert-type scale from *not at all* (1) to *completely* (5).

Rape myth acceptance was measured using the Updated Illinois Rape Myth Acceptance (U-IRMA) scale (McMahon & Farmer, 2011) which contains 19 items which load onto five factors: *she lied, she asked for it, it wasn't really rape, he didn't mean to and he didn't mean to (intoxication)*. For the purposes of this paper, we decided to relabel these factors to clarify these beliefs are predisposed rather than a result of a person's interpretations or decisions relating to the specific case presented. Thus, the factors are subsequently labelled *victims lie, victims ask for it, not really rape, perpetrators do not mean to rape and intoxication*. In the present study, the full scale achieved good reliability ($\alpha = 0.92$). Participants rated their level of agreement for each item on a 7-point Likert scale from *strongly disagree* (1) to *strongly agree* (7). Two filler items are included from the original IRMA scale (Payne et al., 1999) as attention checks. The items “this society should devote more effort to preventing rape” and “victims of rape should be allowed to choose whether they speak with a male or female police officer”, inhibit response bias as the statements offer a deviation in response from other items.² Finally, participants were thanked and debriefed at the end of the survey. The average time to complete the survey was 14 min ($SD = 11$ min). All materials and data for the studies in this paper can be found at: https://osf.io/vhu5w/overview?view_only=4f231ade3a594dc2acf528c7ce7753ac

6.4. Method of analysis

Descriptive statistics and correlations are provided, with G*Power indicating a sample size of 191 was necessary for two-tailed correlation ($\alpha = 0.05$, Power = 0.80) with a small to medium effect size of 0.02, based on small to medium effect sizes reported throughout the literature (see meta-analyses by Hockett et al., 2016; Persson & Dhingra, 2022, Walters et al., 2025). All the hypotheses above were tested within a framework of structural equation modelling (SEM). SEM enables the examination of latent constructs (i.e., Confirmatory Factor Analysis) in addition to estimating the significance and magnitude of direct and indirect causal relationships between measured variables, similar to path analysis (Kline, 2015). All analyses were conducted on R using RStudio.

7. Study 1 results

7.1. Descriptive statistics

Two hundred and fourteen participants, accounting for 62% of the total sample, delivered a guilty verdict. The mean percentage level of confidence was 70% ($SD = 22\%$), demonstrating considerable variation in participants' confidence in their verdict decisions. The average recommended sentence length for participants who delivered a guilty verdict was 4.48 years ($SD = 2.85$). Responses for the decision-making variables of credibility ($M = 5.54$, $SD = 1.36$), responsibility ($M = 3.19$, $SD = 1.85$) and empathy ($M = 3.72$, $SD = 1.07$) were all skewed in favor of supporting the complainant. Cronbach's alpha for the U-IRMA Scale, excluding the two filler items, was $\alpha = .93$. In line with existing literature, which finds low mean scores on self-report measures of RMA (Zidenberg et al., 2021), the distribution of responses was strongly positively skewed, with a mean score of 41.56 ($SD = 18.67$).

² Participants whose combined scores on these items were statistical outliers (over three standard deviations from the mean), were excluded from analysis. It should be noted that these attention checks did not align with the Prolific's policy which only permits instructional manipulation checks, however, we did not reject participants based on failed attention checks.

7.2. Correlations

All decision-making variables demonstrated significant moderate positive correlations with each other; equally all rape myth factors were highly correlated. Credibility was most strongly correlated with *victims lie* ($r = -.54, p < .001$) supporting the proposed pathway in the model. Equally strong correlations were found for responsibility with *victims ask for it* ($r = .62, p < .001$) and *victims lie* ($r = .62, p < .001$). However, empathy was most strongly correlated with *victims lie* ($r = -.59, p < .001$) as opposed to *not really rape* ($r = -.49, p < .001$). The full correlation matrix is available in Table 1.

7.3. Confirmatory factor analyses

Prior to investigating the full structural model, each section of the measurement model was inspected using confirmatory factor analysis (CFA). This was particularly important given two of the subscales of the U-IRMA (*perpetrators do not mean to rape* and *alcohol*) were not incorporated in the hypothesized model. For the *victims lie* factor, all five items loaded onto the latent factor and the model achieved strong fit indices ($\chi(5) = 10.82, p = .055, CFI = .996, RMSEA = .058, SRMR = .013$), compared to Hu and Bentler's (1999) recommendations ($CFI > .95, RMSEA < .06, SRMR < .08$). CFA results for both the *victims ask for it* factor ($\chi(2) = 11.94, p = .003, CFI = .984, RMSEA = .120, SRMR = .024$) and the *not really rape* factor ($\chi(2) = 14.04, p = .001, CFI = .960, RMSEA = .132, SRMR = .040$) also both demonstrated acceptable fit. Although the chi-square statistics were significant in each case, many have noted absolute fit indices penalize models with large sample sizes and high numbers of free parameters (Kenny, 2020). The model fit for the full three-factor solution (with *victims lie*, *victims ask for it* and *not really rape*) remained strong ($\chi(62) = 268.58, p < .001, CFI = .929, RMSEA = .098, SRMR = .065$), and demonstrated notably better fit than the items in a unidimensional model ($\chi(65) = 592.18, p < .001, CFI = .819, RMSEA = .153, SRMR = .087$), and even demonstrated slightly stronger fit than the full five-factor model, with *victims lie*, *victims ask for it*, *not really rape*, *perpetrators do not mean to rape* and *intoxication* ($\chi(142) = 451.01, p < .001, CFI = .916, RMSEA = .079, SRMR = .068$). Finally, it is also worth noting that the three-factor CFA model achieved scalar level measurement invariance, suggesting no differences in how male and female participants interpreted the scale.

7.4. Structural equation modelling

Following the satisfactory results for the measurement model, the full SEM was run using the Lavaan package on R, with the verdict variable specified as a binary, and using diagonally weighted least squares (DWLS) estimation to calculate the model parameters (Rosseel, 2012). The original model included promising fit indices ($\chi(123) = 154.76, p = .028, CFI = .994, RMSEA = .028, SRMR = .054$). However, on further inspection of the model, a total of seven predicted pathways were non-significant, namely each of the direct paths from the rape myth factors to verdict, empathy to verdict, responsibility to verdict and the indirect paths from the *not really rape* factor to verdict and from the *victims ask for it* factor to verdict. Kenny (2020) states a good fitting model does not prove a model is correctly specified and it is still possible to remove specification error. Nonetheless, while model respecification is a common aspect of SEM analyses, it is important that modifications are theoretically justified to avoid overfitting the model to the sample (Kline, 2015). The findings for the lack of significance in the direct pathways from the rape myth factors to verdict mirrored Schuller and Hastings (2002) results, who also found the direct path between RMA and guilt results proposed in their model was non-significant. Thus, these direct pathways were deleted one at a time from the model, starting with the path from the *not really rape* factor to verdict, followed by the *victims ask for it* to verdict path and finally, the path from *victims lie* to verdict was removed.

At this point, the model maintained strong fit indices ($\chi(126) = 156.88, p = .032, CFI = .994, RMSEA = .027, SRMR = .055$), and all paths were significant, including the three indirect paths from the latent rape myth factors to verdict. Given the high level of fit and the risk of overfitting the model to the data, no further modifications were made. The final Rape Myths in Evaluations and Verdicts (RMEV) model including standardized path coefficients is shown in Fig. 2, with indirect pathways reported in Table 2. A full breakdown of the model can be found in Appendix B. Further analyses examining gender differences across the model can be found in the supplementary materials.

8. Study 1 discussion

Considering the complexity of the RMEV model, the goodness of fit measures indicated a well-fitted model according to Hu and Bentler's (1999) goodness of fit cut-offs ($CFI > .95, RMSEA < .06, SRMR < .08$). While recent research has brought into question the applicability of goodness of fit cut-off points in estimation methods other than maximum-likelihood, particularly for methods suited to categorical outcomes such as DWLS (Xia & Yang, 2019), some indices, such as SRMR, have demonstrated robustness regardless of estimation method used (Shi & Maydeu-Olivares, 2020). The SRMR assesses the magnitude of discrepancies between the correlations in the observed and expected model, with values less than 0.08 indicating a good fit. Our final model achieved an SRMR = .055, indicating a very good fit requiring little to no modification. However, it must be noted that although power analysis was guided by commonly-used recommendations in SEM, rules of thumb and post-hoc power calculations have important limitations (Wang & Rhemtulla, 2021). Another limitation of a model with a categorical outcome is the lack of a suitable significance test for model comparisons. Typically, measures such as Akaike Information Criterion (AIC) are useful to determine whether the differences between models can be deemed significant (Wagenmakers & Farrell, 2004). However, because AIC is a likelihood-based estimator which calculates the likelihood information criteria, it is not applicable to DWLS estimation, which is based on least-squares estimation. As such, it is important to note that the comparison of the proposed model to both the null and alternative models is guided by interpreting changes in goodness of fit measures.

There were some small but interesting changes between the originally hypothesized model and the final model. While the three indirect paths between the rape myth factors and verdict were all significant, the direct pathways were not significant, suggesting full rather than partial mediation. The final model demonstrates the significant indirect effect from each of the victim-focused rape myth factors to verdict, mediated by the three judgement variables of credibility, responsibility and empathy respectively. High *victims ask for it* myth endorsement corresponds to higher complainant responsibility ratings and a lower likelihood of a guilty verdict. Concurrently, high levels of *victims lie* and *not really rape* myth endorsement correspond to lower ratings of complainant credibility and juror's empathy with the complainant, respectively, while higher credibility and empathy ratings are linked with guilty verdicts. Additionally, indirect gender differences also operated through rape myth endorsement. Women (relative to men) endorsed lower levels of rape myths, which in turn were associated with higher credibility and empathy ratings and lower responsibility attributions towards the complainant. The indirect effect of gender differences extended to verdict outcomes via credibility and empathy, suggesting that observed gender differences in juror decision-making may reflect underlying variation in rape myth beliefs.

Overall, this study provided a robust structural model of the process of juror verdict decisions in rape trials, incorporating rape myth factors and key decision-making factors relevant to complainant evaluation. Naturally, some limitations to the study should be noted, such as the inclusion of participants from European countries which do not use a jury system. While these participants may legally comprehend consent in a similar way to the UK judiciary, they are unlikely to experience jury

Table 1
Correlation Matrix for Study 1

	1	2	3	4	5	6	7	8	9	10	11	12	13
1. Gender	1												
2. Age	.01	1											
3. Victim known	.17**	-.05	1										
4. False accusation known	-.11	-.09	.16**	1									
5. Likert verdict	.24***	-.14**	.22***	-.07	1								
6. Confidence	.13*	.03	.11*	-.01	.28***	1							
7. Sentence	.21***	-.19**	.24***	.05	.46***	.31***	1						
8. Credibility	.22***	-.08	.23***	-.09	.67***	.25***	.34***	1					
9. Responsibility	-.24***	.13*	-.24***	.08	-.49***	-.19***	-.36***	-.52***	1				
10. Empathy	.17**	-.19***	.22***	-.01	.60***	.21***	.39***	.61***	-.57***	1			
11. VA total	-.24***	.25***	-.21***	.06	-.45***	-.12*	-.28***	-.45***	.62***	-.58***	1		
12. VL total	-.23***	.16**	-.19***	.15**	-.52***	-.15**	-.28***	-.54***	.62***	-.59***	0.69***	1	
13. NR total	-.15**	.14**	-.14**	.06	-.41***	-.10	-.27***	-.40***	.53***	-.49***	0.64***	0.66***	1

Note: Gender was coded as Male (1) and Female (2). Victim known and False accusation known were both coded as No (1), Yes – a friend or family member (2) and Yes – myself (3). VA = Victims ask for it, VL = Victims lie, NR = Not really rape.

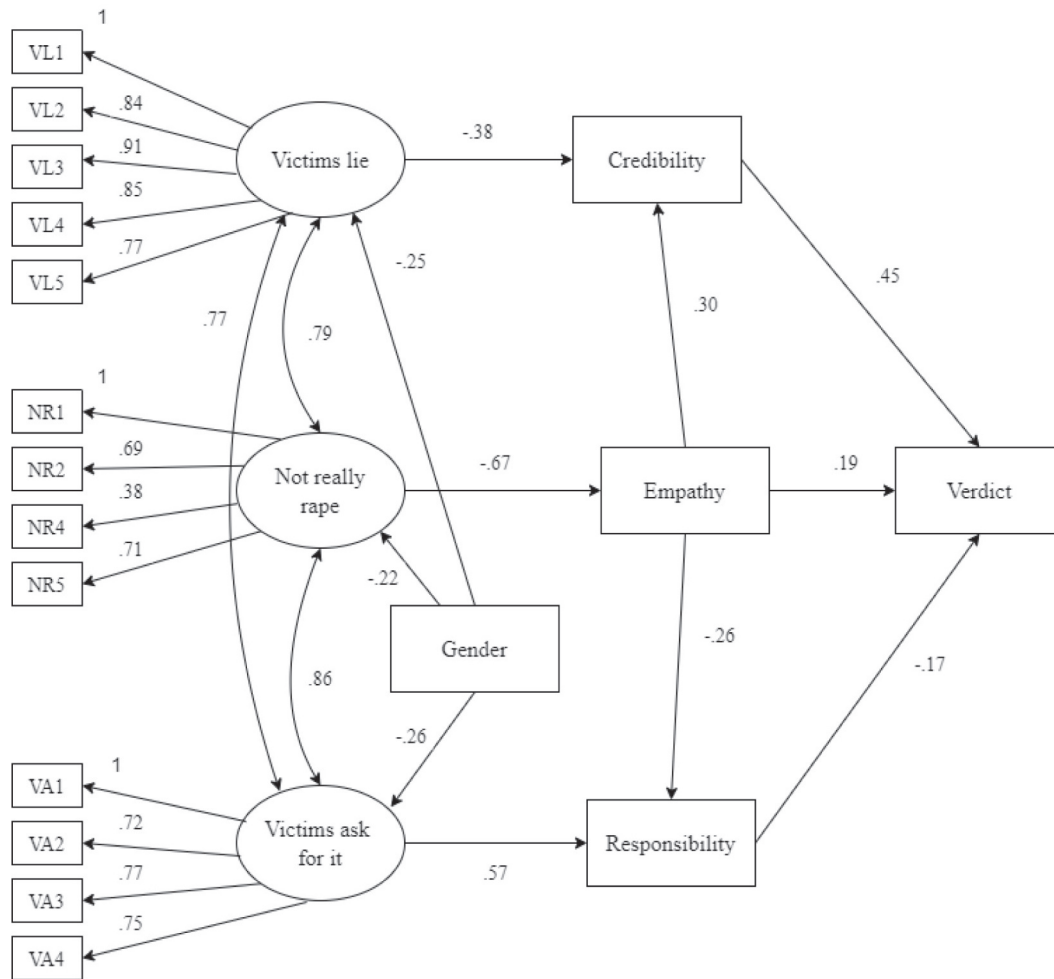


Fig. 2. Final SEM including Standardized Path Coefficients.

duty in reality. This reduces the ecological validity of our sampling in relation to contemporary criteria for realistic jury research (Willmott et al., 2021). Nonetheless, the central processes examined—rape myth acceptance and judgements of credibility, responsibility, empathy and guilt—do not require prior jury service or familiarity with trial structures. Accordingly, while this sampling constraint limits generalizability to UK jurors, it does not undermine the internal validity or theoretical contribution of the present findings.

It is also important to note that although the theoretical model represents the researchers' causal assumptions, causal relationships cannot be confirmed from structural equation modelling alone (Bollen & Pearl, 2013). Although there is considerable evidence that affirms causal mediation can be an appropriate analysis in observational studies (Rijnhart et al., 2021; Shrout & Bolger, 2002; VanderWeele, 2015), the assumptions for causal mediation are notably more difficult to achieve within cross-sectional studies. While the structural elements of our

Table 2
Path Coefficients for SEM Indirect Pathways (Study 1)

Indirect Pathway	<i>b</i>	β	<i>SE</i>	<i>p</i>
Indirect VL → Verdict	-.12	-.17	.02	<.001
Indirect VA → Verdict	-.08	-.10	.04	.050
Indirect NR → Verdict	-.16	-.13	.06	.012
Indirect Gender → Credibility	.26	.09	.06	<.001
Indirect Gender → Responsibility	-.56	-.15	.14	<.001
Indirect Gender → Empathy	.33	.15	.09	<.001
Indirect Gender → Cred. → Verdict	.09	.04	.03	<.001
Indirect Gender → Resp. → Verdict	.05	.02	.03	.077
Indirect Gender → Emp. → Verdict	.06	.03	.03	.037

Note: VL = victims lie; VA = victims ask for it; NR = not really rape.

model generally align with assumptions outlined by ShROUT and Bolger's (2002) regarding temporal ordering,³ measurement reliability and model specification, it is difficult to ascertain that this study met the assumption of no confounding variables. Thus, extrapolating causality among the variables in this study is ill-advised, although the study still holds considerable merit in contributing to knowledge development. The subsequent study aims to further extend the understanding of rape myths impact in verdict decisions, in addition to bolstering the causal implication of the model by introducing manipulated conditions, so that these relationships can be examined experimentally. Furthermore, subsequent studies in this paper employed exclusively UK jury-eligible samples to ensure greater ecological validity and generalizability of the findings.

9. Study 2 introduction

The investigative nature of the first study enabled us to identify a strong, parsimonious model, although only correlation among variables could be inferred due to the non-experimental design. The purpose of the second study, therefore, is to evaluate the potential for a causal model by employing an experimental methodology. By manipulating rape-myth-relevant elements in the fictitious rape case transcript, it is expected this will elicit changes in the respective decision-making factors and subsequently impact verdict decisions.

Across conditions, the transcript manipulations drew on established rape-myth themes concerning how “real” victims are expected to behave and present themselves. Delayed reporting is frequently framed as suspicious, with courtroom observation and mock-jury research showing that even short delays prompt doubts about legitimacy and encourage inferences that a genuine victim would have reported immediately (Ellison & Munro, 2009; Quilter et al., 2022). Similarly, behaviors viewed as counterintuitive—such as a lack of physical resistance—are often taken as evidence that the allegation is less credible, despite the absence of any legal requirement to fight back (Angelone et al., 2015; Ellison & Munro, 2009; Randall, 2010). In their review, van der Bruggen and Grubb (2014) also noted there is considerable evidence indicating victim-survivors who do not resist their attackers are allocated the most blame.

Other manipulations reflected stereotypes surrounding the “ideal victim” (Christie, 1986), whereby victims who are perceived as virtuous, passive, and blameless receive greater sympathy than those who deviate from this image (Grubb & Turner, 2012; Smith & Daly, 2022). Clothing and sexualized presentation are especially potent cues: sexually

objectifying depictions can reduce perpetrator blame and foster assumptions of victim sexual intent (Bernard et al., 2015; Maurer & Robinson, 2008). These judgements extend to alcohol use, with intoxicated complainants routinely viewed as less credible and more responsible for their victimization than sober complainants (Lynch et al., 2013; Maurer & Robinson, 2008; Stewart & Jacquin, 2010).

Similarly, prior social interaction—such as, friendly or flirtatious exchanges—can be taken as indicative of consent, particularly among those lower in rape empathy; thereby shifting responsibility towards the complainant (Adolfsson & Strömwall, 2017; Nason et al., 2019). Finally, juror expectations also extend to how victims should feel and display emotion. Evidence suggests that distressed victims are often deemed more believable than calm or neutral complainants, reinforcing narrow expectations about seemingly appropriate trauma responses (Menaker & Cramer, 2012; Nitschke et al., 2019; Schuller et al., 2010).

Together, these factors reflect well-documented rape myths linking complainant behavior and presentation to jurors' case decision-making, thus, providing the theoretical basis for the manipulations introduced in this study. Thus, two versions of the trial transcript were developed which varied a number of rape myth relevant features in the cases simultaneously—resulting in myth-consistent and myth-inconsistent trial transcripts. The myth-consistent condition presented details which align with commonly held rape myths and cultural stereotypes about what constitutes a “real rape” (e.g., sober complainant, immediate reporting, physical resistance), whereas the myth-inconsistent condition presented details that contradicted these stereotypes and expectations (e.g., intoxication, delayed reporting, no physical resistance). Importantly, while it is anticipated that these rape-myth-relevant elements will impact the decision-making variables, it should be noted that these elements *should not* impact jurors evaluations as they do not change the facts of the case or the legal question of consent. Confirming the impact of these factors on verdict decisions therefore reflects the operation of rape myths and will provide an evidence-based theory on which to base future intervention studies. The following hypotheses were developed for the study:

H1a. Participants in the myth-consistent condition will have lower ratings of complainant responsibility compared to those in the myth-inconsistent condition.

H1b. Participants in the myth-consistent condition will have higher ratings of complainant credibility and empathy towards the complainant compared to those in the myth-inconsistent condition.

H1c. Participants in the myth-consistent condition will have higher levels of guilty verdicts compared to those in the myth-inconsistent condition.

H2. Pathways from the latent rape myth factors to their respective decision-making factors will be significantly lower in the myth-consistent condition relative to the myth-inconsistent condition.

10. Study 2 methodology

10.1. Participants

For this study, participants needed to meet the eligibility criteria for jury duty within the UK – this was achieved using Prolific filters. All participants were between the ages of 18 to 74, resided in the UK and were registered on the local electorate (as indicated by voting in the most recent general election). An additional question was included in the survey to determine whether participants were resident in the UK for 5 years or more. It should be noted participants were not screened for disqualifying criteria (i.e., severe mental health issues or criminal history). A-priori power estimation using semPower (Moshagen & Bader, 2023) for two groups of equal sizes recommended a sample of 450 participants. A total of 505 participants were recruited on Prolific. Of these, eight participants completed the survey too quickly and 11

³ The assumption of temporal ordering requires the independent variable to precede the mediator, and the mediator to precede the outcome variable (ShROUT & Bolger, 2002). We follow consensus of the literature that rape myths exist in participants' schemas and consciousness (Bohner et al., 2009), before the study and case were presented; as such, we expect that rape myth acceptance beliefs temporally precede evaluations of the victim, although it must be noted that the U-IRMA measure was presented after the questions on complainant evaluations within the survey.

participants exited the survey before completing 95% of questions; the remaining 486 participants were compensated £1 for their participation, in line with Prolific's fair-pay threshold. Five participants indicated they had resided in the UK for less than five years and were excluded from analysis. Furthermore, four cases were removed for failed attention checks, and 12 cases were also identified as multivariate outliers using Mahalanobis distance and were excluded from analysis, leaving a total sample of 465 participants.

In the total sample, 43% identified as cisgender male and 56% identified as cisgender female. The median age was 34 years ($M = 36.42$, $SD = 12.89$). 84% of participants identified as heterosexual, and 86% identified as White/Caucasian. Furthermore, 32% of the sample acknowledged knowing a victim-survivor of rape, while 9% acknowledged knowing someone who had been falsely accused of rape. A full breakdown of these demographics is represented in [Appendix A](#).

10.2. Design

The study employed an experimental survey design by varying which transcript the participant encountered in the survey. Participants were randomly shown one of the two conditions.

10.3. Materials and procedure

First, participants provided informed consent and completed the same demographic questions as the first study. Following this, participants were presented with one of two fictitious rape trial transcripts, modelled after the previous studies transcript. The circumstances of the crime remained as similar as possible in the transcripts (i.e., involving a female college student who was raped at a house party by a male peer); however, the rape myth relevant information was altered. Numerous aspects related to rape myths were manipulated across the transcripts including: sober versus intoxicated complainant, immediate versus delayed reporting, physical resistance versus verbal resistance, and flirtatious versus friendly conversation history. Additionally, details on the complainant becoming emotional during the trial were added to the myth-consistent condition (with no such details in the inconsistent condition), while details of the complainant wearing “sexy lingerie” were added to the myth-inconsistent condition (with no such details in the consistent condition).

Again, after the transcript, participants were asked to imagine themselves on the jury and to deliver a dichotomous verdict, *guilty* (1) or *not-guilty* (0), rate the likelihood of the defendant's guilt measured from *completely unlikely* (1) to *completely likely* (7), recommend a sentence from 0 to 10 years, and evaluate their level of confidence in their verdict decision (0–100). The single-item questions on complainant credibility, complainant responsibility and complainant empathy were not altered from the previous study, and again, RMA was measured using [McMahon and Farmer's \(2011\)](#) U-IRMA scale, which included the same attention check items as Study 1. Participants were thanked and debriefed at the end of the survey. The average time to complete the full survey was 12 min ($SD = 6$ min).

10.4. Method of analysis

Independent *t*-tests and two-way ANOVAs were run to compare differences in the mean for each dependent variable based on condition and gender. G*Power indicated a sample size of 102 per group was necessary for *t*-tests with a small to medium effect size of .035 ($\alpha = .05$, Power = .80), whereas for a two-way ANOVA with 4 groups and a small to medium effect size of .18 ($\alpha = .05$, Power = .80), a sample size of 245 was required. Multigroup Structural Equation Modelling was conducted to investigate the moderating effect of condition across multiple paths in the proposed model simultaneously. All analyses were conducted on R using RStudio.

11. Study 2 results

11.1. *t*-tests

There were 228 participants in the myth-inconsistent condition, in comparison to 237 in the myth-consistent condition. There were no significant differences for any of the demographic factors or RMA between conditions, and a chi-square test for gender was also non-significant ($\chi^2 = .20$, $df = 1$, $p = .56$), suggesting there were no significant differences between samples. Chi-square tests compared the dichotomous verdict outcomes between conditions and found a significant difference ($\chi^2 = 5.99$, $df = 1$, $p = .014$), suggesting individuals in the inconsistent condition were less likely to provide a guilty verdict. In total, 61% ($n = 138$) of individuals in the inconsistent condition provided a guilty verdict, in contrast to 72% ($n = 170$) in the myth-consistent condition.

11.2. ANOVA

Next, multiple two-way ANOVAs were conducted to examine the differences in the key variables of the model based on the experimental conditions (myth-consistent vs myth-inconsistent) and gender (male vs female). Significant Levene's test indicated that the assumption of equal variances was violated, thus, 2-way aligned rank transform (ART) ANOVAs were conducted for each variable, using a Bonferroni-corrected significance level of $\alpha = .007$. Results indicated that, with the exception of guilt-likelihood which did differ significantly between the myth-consistent and myth-inconsistent conditions ($p = .002$), each of the variables only differed significantly by gender. A full breakdown of the two-way ANOVA results are presented in [Table 3](#).

11.3. Multigroup SEM

The RMEV model established in the previous study was examined under the different conditions using Multigroup Structural Equation Modelling (MG-SEM). However, to ensure the items in the model maintained the same measurement properties in both samples, measurement invariance testing was undertaken by examining increasingly constrained models using confirmatory factor analysis ([Putnick & Bornstein, 2016](#)). [Widaman and Reise \(1997\)](#) outlined four main steps for testing measurement invariance as configural, metric, scalar and strict (by which point factor loadings, intercepts and residuals are constrained between groups). Given chi-square statistics are notably sensitive in large models, we followed [Chen's \(2007\)](#) recommendation that measurement invariance can be assumed so long as CFI changes are less than .01, RMSEA changes are less than .015 and SRMR changes are less than .01. The configural model demonstrated acceptable fit, indicating that the same factor structure held across groups. When factor loadings were constrained to equality, model fit deteriorated; however, freeing the loading for item NR5 resulted in acceptable fit, indicating partial metric invariance. Attempts to constrain intercepts and residual variances led to substantial declines in fit, and therefore scalar and strict invariance were not supported. [Table 4](#) summarizes the fit statistics and chi-square changes for the various levels of measurement invariance testing.

Again, for the MG-SEM, DWLS estimation was used. The model contained 120 parameters (10 factor loadings, 11 regression paths, 16 residual variances, 16 intercepts, three latent factor variances, three covariances and the threshold for the binary verdict, all of which are calculated for each condition). As expected with a large model, the overall chi-square test was significant ($\chi^2(252) = 347.55$, $p < .001$). Goodness of fit measures indicated good fit (CFI = .989, RMSEA = .041, SRMR = .075). To assess whether the differences in the regression coefficients between groups were significant, each specified path was labelled for each condition and difference terms were then calculated with the model. For example, the path from *victims lie* to credibility in

Table 3
Two-Way ANOVAs Comparing Variable Differences for Condition and Gender (Study 2)

Outcome	Variable	Group A		Group B		F	p
		M	SD	M	SD		
Guilt-likelihood	Condition	5.59	1.43	5.32	1.54	9.38	.002
	Gender	5.84	1.26	4.94	1.62	53.33	<.001
	Interaction					1.68	.196
Credibility	Condition	5.77	1.27	5.46	1.45	1.90	.169
	Gender	5.90	1.24	5.24	1.45	28.84	<.001
	Interaction					0.19	.666
Responsibility	Condition	3.15	1.95	3.24	1.93	0.56	.456
	Gender	2.76	1.90	3.78	1.84	37.98	<.001
	Interaction					0.38	.539
Empathy	Condition	3.80	1.09	3.67	1.11	0.05	.824
	Gender	3.95	1.04	3.45	1.11	22.80	<.001
	Interaction					0.53	.467
Total VL	Condition	11.81	6.80	12.26	7.56	0.97	.325
	Gender	9.97	6.08	14.84	7.62	59.35	<.001
	Interaction					0.59	.444
Total VA	Condition	6.62	4.06	6.93	4.45	4.70	.031
	Gender	5.88	3.46	7.98	4.89	35.66	<.001
	Interaction					4.45	.036
Total NR	Condition	5.89	2.93	6.30	3.26	4.24	.040
	Gender	5.64	2.69	6.70	3.49	15.34	<.001
	Interaction					2.20	.139

Note: For condition, Group A = myth-consistent ($n = 237$) and Group B = myth-inconsistent ($n = 228$). For gender, Group A = female ($n = 262$) and Group B = male ($n = 197$).

Table 4
Measurement Invariance Results

Invariance level	Goodness of fit statistics			df	χ^2	p
	CFI	RMSEA	SRMR			
Configural	.989	.041	.075	252	347.55	
Metric	.976	.058	.085	262	461.62	<.001
Partial Metric	.979	.055	.083	261	438.21	.002
Scalar	.918	.081	.087	275	688.10	.208
Strict	.914	.081	.091	292	727.76	<.001

the high condition was labelled A1 and the same path in low condition was labelled A2 so that the significance of the difference term (A1-A2) could be calculated within the model.

Significant differences were found between the paths for *victims lie* to credibility ($b = -.20$, $\beta = -.21$, $z = -3.05$, $p = .002$) and the indirect path from *victims lie* to verdict ($b = -.11$, $\beta = -.17$, $z = -2.24$, $p = .025$). Although the regression coefficients for the paths from *not really rape* to empathy and from *victims ask for it* to responsibility were higher in the myth-inconsistent condition, the differences between conditions did not reach statistical significance for either pathway. A full breakdown of statistics of the MG-SEM is shown in Table 5, with the model including standardized path coefficients for each group illustrated in Fig. 3.

12. Study 2 discussion

Hypothesis one was partially supported with significantly higher ratings of guilt-likelihood ($M = 5.59$) and a higher proportion of guilty verdicts (72%) in the myth-consistent condition. Significant differences did not emerge between conditions for credibility, responsibility and empathy, although the differences were in the expected direction. Instead, significant differences for the decision-making variables were largely attributable to gender. Consistent with Study 1, gender differences operated primarily through rape myth endorsement, while serial indirect effects highlighted that gender differences in verdicts were predominantly indicated through credibility, in both the myth-consistent and myth-inconsistent conditions. Notably, interactions between gender and condition were largely absent, suggesting that gender differences were stable across trial framing rather than contingent on alignment with “real rape” stereotypes. However, it is possible that

Table 5
Path Coefficients for Each Condition in the Multigroup SEM (Study 2)

Pathway	Low Condition			High Condition		
	b	SE	p	b	SE	p
VL1 $\leftarrow$ VL	1.00			1.00		
VL2 $\leftarrow$ VL	1.03	.08	<.001	1.12	.09	<.001
VL3 $\leftarrow$ VL	1.02	.08	<.001	1.07	.08	<.001
VL4 $\leftarrow$ VL	0.76	.07	<.001	0.85	.09	<.001
VL5 $\leftarrow$ VL	0.90	.08	<.001	0.98	.11	<.001
VA1 $\leftarrow$ VA	1.00			1.00		
VA2 $\leftarrow$ VA	0.72	.08	<.001	1.01	.15	<.001
VA3 $\leftarrow$ VA	0.70	.08	<.001	0.82	.13	<.001
VA4 $\leftarrow$ VA	1.02	.15	<.001	1.36	.23	<.001
NR1 $\leftarrow$ NR	1.00			1.00		
NR2 $\leftarrow$ NR	0.79	.08	<.001	1.08	.09	<.001
NR4 $\leftarrow$ NR	0.39	.05	<.001	0.27	.04	<.001
NR5 $\leftarrow$ NR	1.25	.19	<.001	1.88	.26	<.001
Gender $\rightarrow$ VL	-1.09	.19	<.001	-0.93	.21	<.001
Gender $\rightarrow$ VA	-0.59	.16	<.001	-0.49	.16	.002
Gender $\rightarrow$ NR	-0.41	.12	<.001	-0.30	.10	.004
Credibility $\rightarrow$ Verdict	0.60	.05	<.001	0.59	.06	<.001
Responsibility $\rightarrow$ Verdict	-0.07	.05	.133	-0.19	.07	.005
Empathy $\rightarrow$ Verdict	-0.04	.07	.630	-0.17	.11	.124
Empathy $\rightarrow$ Credibility	0.53	.06	<.001	0.53	.07	<.001
Empathy $\rightarrow$ Responsibility	-0.65	.10	<.001	-0.98	.16	<.001
VL $\rightarrow$ Credibility	-0.27	.04	<.001	-0.47	.05	<.001
VA $\rightarrow$ Responsibility	0.61	.13	<.001	0.60	.16	<.001
NR $\rightarrow$ Empathy	-0.88	.12	<.001	-1.03	.15	<.001
VA $\leftrightarrow$ VL	0.92	.18	<.001	0.82	.17	<.001
NR $\leftrightarrow$ VA	0.60	.10	<.001	0.50	.09	<.001
NR $\leftrightarrow$ VL	0.55	.09	<.001	0.68	.11	<.001
Indirect VL $\rightarrow$ Verdict	-0.16	.03	<.001	-0.27	.04	<.001
Indirect VA $\rightarrow$ Verdict	-0.04	.03	.181	-0.12	.05	.030
Indirect NR $\rightarrow$ Verdict	0.03	.07	.629	0.18	.11	.120
Indirect Gender $\rightarrow$ Credibility	0.29	.07	<.001	0.44	.10	<.001
Indirect Gender $\rightarrow$ Responsibility	-0.36	.13	.005	-0.29	.20	.004
Indirect Gender $\rightarrow$ Empathy	0.37	.10	<.001	0.31	.11	.005
Indirect Gender $\rightarrow$ Cred. $\rightarrow$ Verdict	0.18	.05	<.001	0.26	.06	<.001
Indirect Gender $\rightarrow$ Resp. $\rightarrow$ Verdict	0.03	.02	.217	0.06	.03	.063
Indirect Gender $\rightarrow$ Emp. $\rightarrow$ Verdict	-0.01	.03	.634	0.05	.04	.181

Note: VL = victims lie; VA = victims ask for it; NR = not really rape.

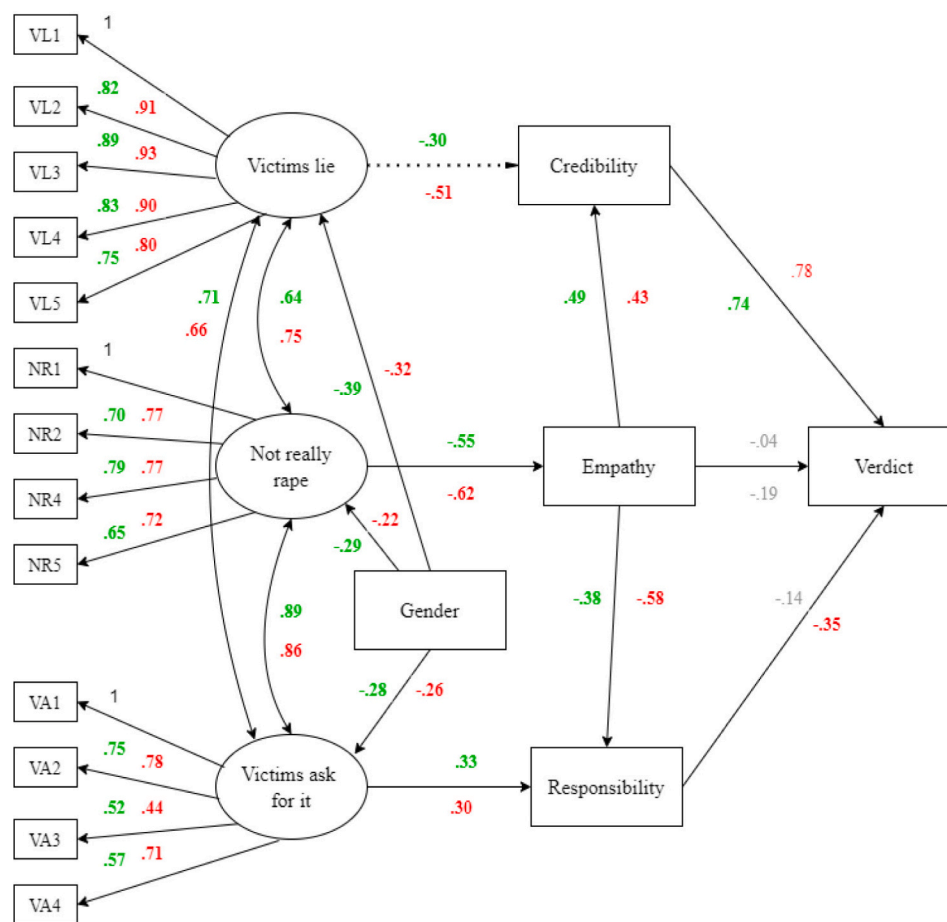


Fig. 3. Multigroup SEM Containing Standardized Estimates for Both Conditions.

Note: Myth-consistent condition coefficients in green, myth-inconsistent condition coefficients in red, while non-significant path coefficients are in grey italics. Paths with significant differences between conditions are represented by dashed arrows.

condition's lack of effect on the judgement variables was due to the differences between the myth-consistent and myth-inconsistent conditions not being evenly distributed, despite efforts to manipulate the transcript so that there was one less or one additional piece of information relating to each of the three rape myth factors being examined. It is likely that the different rape myths do not uniformly contribute to predicting the decision-making factors, such that information about a complainant's delayed reporting may have a larger effect on credibility ratings compared to the effect of information about a complainant's choice of suggestive clothing on responsibility judgements.

Nonetheless, despite the limited differences between the transcripts in each condition, there was a significant overall effect on verdict outcomes. This is particularly interesting considering none of the information altered in the transcripts changed the ultimate facts of the case (i. e., both parties agreed on the events until their sexual interaction, at which point the complainant claims she did not consent and asked the defendant to stop, which the defendant denies). Information pertaining to whether the complainant flirted, reported immediately or was emotional at trial does not indicate whether a crime occurred, and yet participants were significantly more likely to deliver a guilty verdict in the myth-consistent condition. This finding aligns with decades of experimental and observational research, which indicates that jurors rely on pre-existing attitudes and extra-legal factors to inform their trial judgements (Willmott et al., 2021).

Hypothesis two was also partially supported with a significant difference found between conditions on the path from *victims lie* to credibility. Both the *victims lie* and *not really rape* factors were stronger predictors of credibility and empathy respectively in the myth-

inconsistent condition, suggesting that when cases aligning with rape myths stereotypes, a juror's level of RMA contributed more to predicting their evaluations of complainant credibility and empathy. However, neither empathy nor the indirect path from the *not really rape* factor were significant predictors of verdict decisions in either the myth-consistent or the myth-inconsistent condition. This result contradicts expectations based on the findings of the previous study, where empathy ratings were a strong positive predictor of guilty verdicts. Based on these findings, it would appear that credibility and responsibility contribute more to explaining verdict decisions, particularly when the case presented deviates from "real rape" stereotypes.

There were also some other notable differences that emerged between conditions. In the myth-consistent condition, responsibility did not reach significance as a predictor of verdict, but it did still significantly predict verdict in the inconsistent condition. Furthermore, the indirect path from *victims asks for it* to verdict was only significant in the myth-inconsistent condition. These findings bear important implications for reducing the influence of rape myths in jury decisions.

13. Study 3 introduction

The final study of this project was to test a newly developed educational intervention program designed to reduce the impact of RMA on jury decision-making and victim-survivor evaluations. Various efforts have been (and continue to be) made to dispel rape myths generally, including dedicated resources providing information about the realities of rape from numerous organizations (Amnesty International, 2021; Crown Prosecution Service, 2021). While the breadth of the problem has

led to widespread calls to overhaul juries' roles in rape trials, including radical proposals to abolish juries (Munro, 2022; Topping, 2018), many experts have supported the recommendation of educating jurors about rape myths (Gillen, 2019; Leverick et al., 2025; Smith & Daly, 2022; Willmott et al., 2021).

While some research has suggested the success of interventions in individuals with high endorsement of rape myths is tenuous, such as Leone et al.'s (2021) study which found their empathy-based bystander intervention was less effective in men with high RMA, more positive findings emerged from Hudspeth et al.'s (2023, 2024) systematic reviews. The researchers concluded that relatively short, video or web-based interventions can lead to short-term reductions in RMA. They also provided recommendations on incorporating interactive elements, increasing the number of sessions and careful consideration of the intervention presenter. However, a short video intervention targeting prevalent rape myths is a well-suited approach to quickly and effectively educating jurors and reducing their reliance on rape myths during trial decision-making. Thus, a 30-min educational video was developed, addressing key rape myths related to the decision-making factors from the theoretical model (such as delayed reporting, fear and trauma responses and implied consent). A corresponding control video addressing climate change was created, to match the template and content severity as closely as possible. The intervention video is included in the supplementary materials. The following hypotheses were examined for the final study:

H1. Participants in the intervention condition will have lower ratings of complainant responsibility and higher ratings of complainant credibility and empathy towards the complainant, and increased levels of guilty verdicts.

H2a. Condition will moderate the path from *victims lie* to credibility, with weaker relationships in the intervention condition.

H2b. Condition will moderate the path from *victims ask for it* to responsibility, with weaker relationships in the intervention condition.

H2c. Condition will moderate the path from *not really rape* to empathy, with weaker relationships in the intervention condition.

14. Study 3 methodology

14.1. Participants

All participants met the general eligibility criteria for jury duty within the UK – this was achieved using the same Prolific filters as Study 2 for Prolific participants, whereas participants collected through social media were presented with the following statements regarding eligibility (I am aged 18–76, I have lived in England and Wales for five years or more and I am registered to vote) to which they responded *all statements are true for me* or *one or more statements are not true for me*. Five cases which did not meet the eligibility criteria were excluded. Again, participants were not screened for disqualifying criteria relating to severe mental health issues or criminal history. Based on the multigroup-SEM results presented in the previous study, power analysis was conducted using *semPower* (Moshagen & Bader, 2023) recommended 191 observations for each condition, for a total sample of 382. On Prolific, 294 participants were recruited, of which 288 participants were compensated £5 in line with Prolific's fair-pay threshold, after six cases were excluded for failing multiple attention checks, having less than 95% survey completion or completing the survey too quickly. An additional 228 participants were recruited through social media sites including Facebook, Twitter and Reddit—these participants were offered entry to a prize draw for a £50 Amazon voucher. From the social media group, 111 cases were excluded. Although the completion rate was lower in the social media group, there were no systematic differences in the data quality observed in the completed responses. Sixteen cases were identified as multivariate outliers using Mahalanobis distance and were

excluded from analysis, leaving a total sample of 391 participants.

In the total sample, 42% identified as cisgender male and 56% identified as cisgender female. The median age was 37 years ($M = 39.47$, $SD = 14.11$). 86% of participants identified as heterosexual, and 86% identified as White/Caucasian. Furthermore, 32% of the sample acknowledged knowing a victim-survivor of rape, while 9% acknowledged knowing someone who had been falsely accused of rape. Full details of the demographics are shown in [Appendix A](#).

14.2. Design

This study employed an experimental design in order to test the effectiveness of a rape myth intervention in reducing the impact of rape myths in jury decision-making. Participants were randomly allocated to either the experimental or control condition.

14.3. Materials and procedure

After screening questions and providing informed consent, participants were asked to complete the U-IRMA Scale (McMahon & Farmer, 2011), including three instructional attention check items (e.g., Please select “Somewhat agree” for this question). Following this, participants were presented with either the control or intervention video. The intervention video was a 30-min educational presentation on rape myths, guided by the model established in previous studies. The video was created using Canva, including various simple animations to complement the text and audio. The presentation starts with an outline of what will be covered, followed by the question ‘what does a victim look like to you?’ to encourage participants to consider their own expectations. Following this, information is provided about what rape myths are and evidence is presented to counteract these myths. For example, evidence on trauma was presented to explain why delayed and inconsistent reports of rape are not necessarily false allegations, an explanation of obtaining versus inferring consent was provided to challenge blaming victims’ behaviors, and finally, statistics on sexual violence were provided to dispel misconceptions about “real rape”. The video was posted as an unlisted YouTube video and embedded into the survey. The control video was a 30-min education presentation on climate change in a similar style.

After the video, participants were asked to write a sentence answering a question related to the video they had watched—qualitative responses were reviewed by the researchers and inadequate or inappropriate responses were coded as a failed attention check (e.g., “safety is the most important” in response to the question “please explain how rape myths can impact our decisions”). To test the manipulation, a 10-item scale on sexual consent was constructed to evaluate differences between those who received the intervention and those who received the control. Given the intervention video contained a section explicitly focused on consent, the manipulation check examines knowledge directly addressed in the intervention. Participants were then asked to read the five-page fictitious rape trial transcript used in the first study.

Again, after the transcript, participants were asked to imagine themselves on the jury and to deliver a verdict, *guilty* (1) or *not-guilty* (0), rate the likelihood of the defendant's guilt, measured from *completely unlikely* (1) to *completely likely* (7), recommend a sentence from 0 to 10 years and evaluate their level of confidence in their verdict decision (0–100). The single-item questions on complainant credibility, complainant responsibility and complainant empathy were not altered from the previous studies, and again, participants were asked the same demographic questions as the previous studies. Participants were thanked and debriefed at the end of the survey. The average time to complete the survey was 52 min ($SD = 18$ min).

14.4. Method of analysis

Independent *t*-tests and two-way ANOVAs were run to compare

differences in the mean for each dependent variable based on condition and gender. Again, G*Power indicated a sample size of 102 per group was necessary for t-tests with a small to medium effect size of .035 ($\alpha = .05$, Power = .80), whereas for a two-way ANOVA with 4 groups and a small to medium effect size of .18 ($\alpha = .05$, Power = .80), a sample size of 245 was required. Originally, we expected to assess the second set of hypotheses in the same Multigroup SEM framework as the previous study. However, in this study, the binary verdict variable was extremely imbalanced (79 not-guilty vs. 312 guilty), leading to non-positive definite matrices and Heywood cases. Thus, Multigroup SEM was statistically inappropriate for the binary verdict. Our solution was to use moderated-mediation analyses to examine the effect of the condition on the pathways for each rape myth and decision-making variable pairing. All analyses were conducted in R using RStudio.

15. Study 3 results

15.1. Mean comparisons

There were 197 participants in the control condition, in comparison to 194 in the experimental condition. t-tests confirmed there were no significant differences for any of the demographic factors or RMA scores between conditions. Additionally, a series of two-way ANOVAs were conducted to examine differences in key variables based on condition, gender and their interaction. Table 6 presents the results of the two-way ANOVAs. Significant differences emerged across conditions for the manipulation check (consent scores), guilt-likelihood, responsibility and empathy, whereas significant gender differences were found across all variables except consent scores. A chi-square test was also run to test significant differences in verdict decisions for the two conditions, with results indicating a significant difference was marginally achieved ($\chi = 3.82$, $p = .050$). In total, 75% ($n = 147$) of the control group provided a guilty verdict, compared to 84% ($n = 167$) of the intervention group. However, the chi-square test for gender and verdict did not show any significant differences ($\chi = .01$, $p = .920$).

15.2. Moderated mediation

Moderated mediation analyses were undertaken to examine the hypothesis that the intervention would reduce the impact of rape myths on their respective decision-making variable. Thus, it was anticipated that the moderation of condition would occur on the “a path” of the mediation. To aid interpretation, all independent variables and mediators were mean-centered.

In the credibility model (model 1), results indicated that participants in the intervention condition provided significantly higher credibility ratings ($\beta = .14$, $p = .004$). Increases in *victims lie* scores were associated with a decrease in credibility ratings. However, there was a significant difference in this association between participants in the control and intervention conditions ($imm = .08$, $p = .026$). A bias-corrected bootstrapped confidence interval with 5000 samples for the index of moderated mediation also failed to capture zero, 95% CI [.01, .05], suggesting the experimental condition was a significant moderator of the relationship between participants' *victims lie* scores and credibility scores. Additionally, *victims lie* scores made a significant, unique contribution in predicting verdict decisions, indicating partial mediation. Table 7 includes the standardized and unstandardized coefficients for the moderated mediation analysis. Fig. 4 illustrates the structure and standardized coefficients for model 1.

Fig. 5 illustrates the simple slopes for the relationship between the

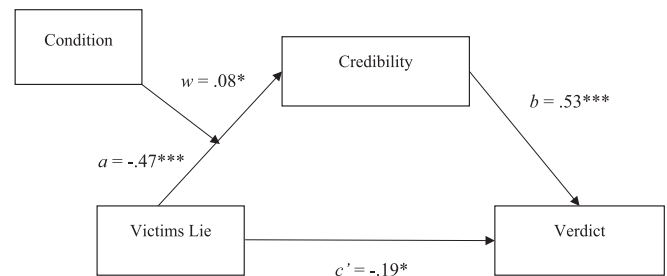


Fig. 4. Credibility Moderated-Mediation (Model 1).

Table 6
Two-Way ANOVAs Comparing Condition and Gender Differences (Study 3)

Outcome	Variable	Group A		Group B		F	p
		M	SD	M	SD		
Guilt-likelihood	Condition	5.95	1.02	6.26	0.95	9.99	.002
	Gender	6.32	0.84	5.86	1.10	21.31	<.001
	Interaction					0.38	.537
Credibility	Condition	5.97	1.11	6.25	0.97	3.17	.076
	Gender	6.31	0.89	5.89	1.17	12.93	<.001
	Interaction					0.31	.860
Responsibility	Condition	2.76	1.67	2.29	1.71	18.87	<.001
	Gender	2.24	1.55	2.86	1.81	16.15	<.001
	Interaction					1.79	.184
Empathy	Condition	3.91	0.94	4.15	0.92	5.08	.025
	Gender	4.23	0.84	3.80	0.98	20.29	<.001
	Interaction					0.22	.637
Total VL	Condition	11.50	6.67	11.41	6.51	0.21	.647
	Gender	9.85	5.56	13.26	7.16	29.94	<.001
	Interaction					1.36	.244
Total VA	Condition	6.40	3.82	6.28	3.63	1.63	.202
	Gender	5.78	3.38	6.97	3.99	20.29	<.001
	Interaction					3.95	.048
Total NR	Condition	5.57	2.58	5.41	2.34	2.64	.105
	Gender	5.20	2.10	5.82	2.79	7.53	.006
	Interaction					1.96	.163
Consent score	Condition	8.22	1.28	8.72	1.02	17.69	<.001
	Gender	8.50	1.12	8.43	1.25	0.16	.688
	Interaction					0.75	.386

Note: For condition, Group A = intervention ($n = 194$) and Group B = control ($n = 197$). For gender, Group A = female ($n = 196$) and Group B = male ($n = 184$).

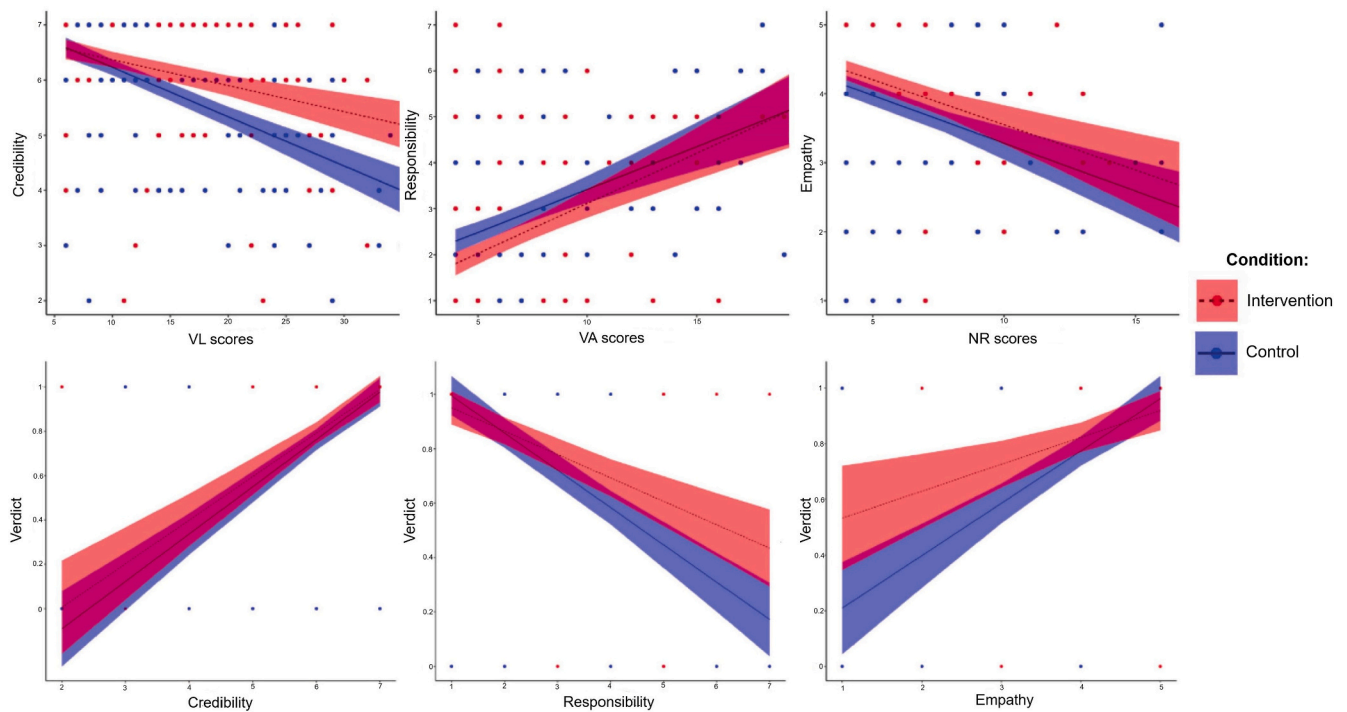


Fig. 5. SS Plots for Moderated Mediation Models.

Note: The shaded ribbons around the slope lines represent the 95% CI.

Note: The top row shows the “a path” plotted for each condition in the 1) credibility, 2) responsibility and 3) empathy models. The bottom row shows the “b path” plotted for each condition in the 1) credibility, 2) responsibility and 3) empathy models.

victims lie factor and credibility ratings in the two conditions in the top left corner of the image. As shown, participants with low *victims lie* scores rate the complainant as highly credible, with no differences between conditions. However, as *victims lie* scores increase, participants

rate the complainant as less credible, but this effect is weakened for participants who received the intervention. For those with the highest *victims lie* scores, participants in the control group generally selected “slightly incredible”, compared to participants in the intervention group

Table 7

Mediation Analysis for Credibility with Moderation on Path A

					CI	
Path	<i>B</i>	<i>SE</i>	β	<i>p</i>	Lower	Upper
Model 1						
VL → Credibility (a1)	−.068	.008	−.465	<.001	−.084	−.054
Condition → Credibility (a2)	.287	.100	.134	.004	.097	.494
Interaction → Credibility (a3)	.043	.016	.146	.007	.012	.075
Credibility → Verdict (b1)	.554	.104	.531	<.001	.408	.757
VL → Verdict (c')	−.028	.013	−.185	.025	−.054	−.005
Moderation index (imm)	.024	.011	.077	.026	.004	.045
Control indirect effect	−.050	.012	−.285	<.001	−.072	−.030
Intervention indirect effect	−.026	.009	−.208	.003	−.044	−.011
Model 2						
VA → Responsibility (a1)	.202	.020	.443	<.001	.163	.240
Condition → Responsibility (a2)	−.480	.163	−.480	.003	.793	−.154
Interaction → Responsibility (a3)	.044	.043	.048	.304	−.040	.125
Responsibility → Verdict (b1)	−.350	.036	−.572	<.001	−.417	−.276
VA → Verdict (c')	−.028	.013	−.185	.025	−.046	.029
Moderation index (imm)	−.015	.016	−.028	.325	−.047	.014
Control indirect effect	−.063	.013	−.239	<.001	−.090	−.041
Intervention indirect effect	−.078	.014	−.267	<.001	−.107	−.053
Model 3						
NR → Empathy (a1)	−.135	.020	−.358	<.001	−.173	−.094
Condition → Empathy (a2)	.257	.092	.135	.005	.075	.437
Interaction → Empathy (a3)	.003	.041	.004	.934	−.080	.079
Empathy → Verdict (b1)	.418	.072	.377	<.001	.274	.554
NR → Verdict (c')	−.076	.031	−.182	.013	−.135	−.016
Moderation index (imm)	.001	.017	.002	.935	−.036	.032
Control indirect effect	−.057	.015	−.136	<.001	−.088	−.030
Intervention indirect effect	−.056	.017	−.134	.001	−.094	−.028

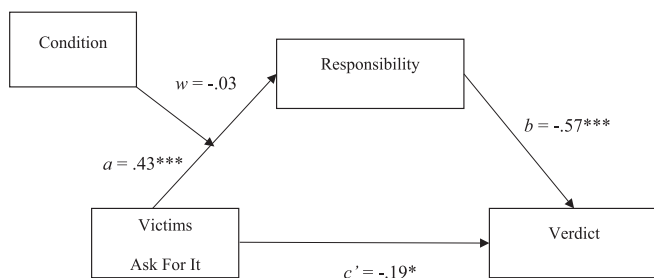


Fig. 6. Responsibility Moderated-Mediation (Model 2).

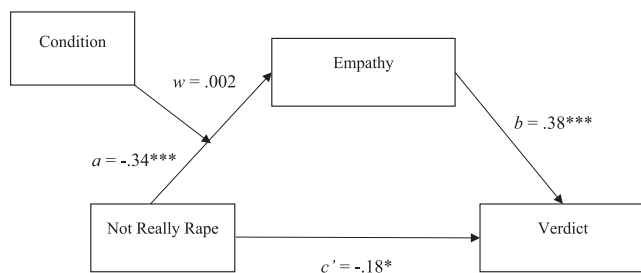


Fig. 7. Empathy Moderated-Mediation (Model 3).

who selected “slightly credible”.

In the responsibility model, increasing *victims ask for it* scores were associated with increases in responsibility ratings and decreasing probability of a guilty verdict. *Victims ask for it* scores also made a significant unique contribution in predicting verdict decisions, indicating partial mediation. However, although results showed that participants in the intervention condition provided significantly lower responsibility ratings ($\beta = -.48, p = .003$), the moderation index was non-significant ($imm = -.28, p = .325$) and the bootstrapped confidence intervals also crossed zero, 95% CI $[-.05, .01]$. Full statistics for the responsibility moderated-mediation (model 2) are described in Table 7 and Fig. 6 illustrates the structure and standardized coefficients for model 2.

Finally, low *not really rape* scores significantly predicted empathy ratings and made a significant unique contribution in predicting guilty verdicts beyond high empathy scores, demonstrating partial mediation. Although participants showed significantly higher empathy ratings in the intervention condition ($\beta = .14, p = .005$), condition was not a significant moderator of the relationship between *not really rape* and empathy. The moderation index was non-significant ($imm = .002, p = .935$) and the bootstrapped confidence intervals also crossed zero, 95% CI $[-.04, .03]$. Full statistics for the empathy moderated-mediation (model 3) are described in Table 7 and illustrated with the standardized coefficients in Fig. 7.

Considering significant differences were found for both responsibility and empathy between conditions, we decided to run exploratory moderated-mediation analyses on alternative paths to explore whether condition moderated any of these relationships. However, despite some diversion between conditions on the “b” path (from the respective mediators to verdict) for both the responsibility and empathy models illustrated in the simple slope plots in Fig. 4, none of these models obtained a significant moderation index.

16. Study 3 discussion

The results of the final study lend support for the use of educational interventions about rape myths to reduce their influence on jurors' evaluations of the complainant and, ultimately, on verdict decisions. The manipulation check showed significant differences between individuals who received the intervention compared to those who received the control video, suggesting the content of the intervention led

to greater awareness and understanding of issues surrounding sexual consent. Numerous studies have found strong predictive relationships between attitudes to/knowledge of sexual consent with RMA (see: Kilimnik & Humphreys, 2018; Klement et al., 2017; Warren et al., 2015), making it suitable for testing the effectiveness of the intervention. Hypothesis 1 was partially supported with significantly higher ratings of empathy towards the complainant, as well as significantly lower ratings of complainant responsibility for participants who received the intervention. Additionally, the chi-square test demonstrated differences in verdicts between conditions, just reaching significance.

One concern in educating jurors about the realities of sexual violence and correcting rape myths, is that it could inadvertently create a negative bias against defendants in rape trials, which would impinge on defendants' rights to a fair trial. And so, the fact that verdict decisions were only marginally statistically different can be considered a positive outcome. Out of the 194 participants in the intervention condition, 32 delivered a not-guilty verdict (compared to 47 out of 197 participants in the control condition). Evidently, the educational intervention video did not eradicate not-guilty verdicts, suggesting at the very least the intervention does not overly bias participants in favor of the complainant. Future studies should examine participants' evaluations of the defendant following the intervention.

It was not possible to evaluate the differences between conditions for the full model tested in the previous studies, due to the binary verdict variable being highly imbalanced. There were notable differences in the guilty verdict rates across the three studies, with Study 1 producing the lowest proportion of guilty verdicts (62%), in contrast to Study 2 (72%) and the intervention group (84%) in this study. Considering the rate of guilty verdicts was elevated even in the control group (75%) relative to previous studies, it is important to consider that differences may reflect variation in the composition of the samples, rather than the intervention alone. For example, the proportion of male participants increased across studies (35% in Study 1; 42% in Study 2; 47% in Study 3). In addition to this, mean total scores for the U-IRMA were also lower in the Study 3 sample ($M = 36.98$) than in Studies 1 ($M = 41.56$) and 2 ($M = 39.55$). Importantly, the current study was not designed to experimentally manipulate baseline RMA across samples. Future work would benefit from explicitly controlling for or equating baseline RMA across conditions (e.g., through stratified sampling or statistical adjustment) to more clearly isolate intervention effects.

Despite these sampling differences, a consistent pattern of gender effects emerged across studies, with women endorsing lower levels of rape myths and evaluating the complainant more favorably. Interestingly, consent knowledge scores did not differ by gender, suggesting that gender differences in evaluative judgements are unlikely to reflect disparities in basic understandings of consent. Again, interactions between gender and condition were largely absent, suggesting that the intervention operated similarly across genders. One notable exception is that small but consistent interaction which emerged for the *victims ask for it* factor, replicating a similar pattern observed in Study 2. Although this effect was modest, it may suggest that certain behavioral rape myths are comparatively more sensitive to contextual or educational influences across genders.

Furthermore, moderated-mediation models also presented some interesting findings. The credibility model showed moderation of condition on the expected path between total scores for the *victims lie* rape myth factor and credibility assessments. The simple slopes diagram illustrates that for participants in the control condition, high *victims lie* scores correspond to low to medium ratings of complainant credibility. Meanwhile, for participants who received the intervention, participants with high *victims lie* scores typically still provided medium to high ratings of complainant credibility.

Conversely, there was no significant moderation of condition on the paths from rape myth scores to either responsibility or empathy. This is interesting considering there were significant differences in ratings for responsibility and empathy between conditions, as expected.

Examination of the simple slope plots shows minimal variation in the “a” pathway for both responsibility and empathy. However, there was notably more divergence in the simple slopes for the “b” pathway, i.e., from responsibility to verdict and from empathy to verdict. Although the moderation index did not reach significance for either variable, the diagrams indicate that for participants who received the intervention, high ratings of responsibility have a noticeably weaker relationship with not-guilty verdicts compared to participants in the control group who judged the complainant as highly responsible.

Similarly, low ratings of empathy show a strong correspondence to not-guilty verdicts for participants in the control condition, but this effect is evidently weakened for participants in the intervention condition. As to why condition was not a significant moderator on the “a” pathway for either responsibility or empathy, it could be that an individual's RMA for the respective factors (*victims ask for it* and *not really rape*) is a more fixed predictor of responsibility and empathy ratings, while the influence of responsibility and empathy on verdict decisions is more flexible. In other words, it suggests levels of *victims ask for it* myth endorsement are extremely difficult to disentangle from complainant responsibility judgements, and levels of *not really rape* myth endorsement are difficult to disentangle from ratings of empathy with the complainant. This is not to say it is not worth addressing *victims ask for it* and *not really rape* myths in interventions as there is still a notable impact on responsibility and empathy evaluations, however, it would appear *victims lie* rape myths (generally covering myths related to motivations for and frequency of false accusations), are less impervious to education efforts.

It is important to recognize that caveats which temper these conclusions—in particular, one limitation of this study concerns the sequencing of the manipulation check. The consent scale was always presented immediately after the trial materials and before participants evaluated the case. This ordering may have unintentionally primed participants to think explicitly about consent at the very moment they were forming their judgements, making it difficult to separate the effect of the intervention video itself from possible measurement-induced priming. We recommend future work counterbalance or separate these elements to clarify their independent contributions.

Second, the possibility of demand characteristics and socially desirable responding must be taken seriously. Participants exposed to an explicitly educational video on rape myths may have inferred that “appropriate” responses involved greater sympathy towards the complainant, regardless of their underlying attitudes. In this interpretation, the findings might reflect compliance with perceived expectations rather than deeper cognitive change. Although the present data cannot distinguish between attitude change and conformity, both explanations suggest that contextual cues can shift the evaluative frame jurors bring to a case. Effectively, whether this immediate change is mediated by a shift in underlying attitude or by participants' heightened awareness and motivation to conform to a desirable norm, the practical effect is that the intervention modified judgements in the intended direction. Nevertheless, practical claims are offered cautiously until further research using longer delays and less transparent manipulations are conducted.

17. General discussion

In this paper we presented a robust theoretical model detailing the process of how jurors pre-existing beliefs about rape influence their evaluations of the victim-survivor and ultimately their verdict decisions, which provided the foundation for a targeted jury educational intervention. The Rape Myth, Evaluation and Verdict (RMEV) model demonstrated exceptional goodness of fit indices in Study 1 and remained relatively stable in Study 2 despite varying rape-myth-relevant information in the cases presented to participants, suggesting the temporal structure of the model (latent rape myth factors predict victim-survivor evaluations, which in turn predict verdict decisions) is generally robust to variability in rape trials. Similarly, gender differences were

largely indirect across studies and operated through the same rape myth pathways identified in the model. The intervention also demonstrated considerable potential in reducing the impact of rape myths on juror decisions, particularly for disentangling rape myths from judgements of victim-survivor credibility.

Results from Study 2 and Study 3 suggest that credibility judgements are one of the most malleable elements in juror decisions. Study 2 found that *victims lie* endorsement is a much stronger predictor of credibility when the case presented is inconsistent with the “real rape” stereotype (i.e., the victim-survivor was intoxicated, did not physically resist), while Study 3 found the intervention significantly reduced the influence of *victims lie* rape myth endorsement on credibility judgements compared to the control. Responsibility judgements and evaluations of empathy with the victim-survivor demonstrated notably more resilience to change in both studies. However, the strong relationships between empathy and responsibility with credibility may account for some of the variance. Study 2 suggests empathy may not even predict verdict decisions depending on the case information presented, despite strong predictive ability in both Study 1 and Study 3 (which used the same case). This is interesting considering that out of the three judgement factors included, we would have assumed empathy with the victim-survivor would be considerably pre-determined by external factors, such as personality or experience with a known victim-survivor of rape.

Consistent with a large proportion of jury research (Bornstein et al., 2017; Kerr & Bray, 2005), our findings are limited to understanding individual jurors' deliberation. While the use of written case materials is widely contested (Kerr & Bray, 2005), meta-analyses have found that variations in trial modality do not appear to result in any significant differences in outcomes in studies with community samples (Bornstein, 1999; Bornstein et al., 2017). Bornstein et al.'s most recent meta-analysis found that short vignettes were, in fact, the most common modality utilized. We have strived for stronger ecological validity in our research by using a trial transcript which includes a written summary of judge's directions, prosecution and defense opening statements, witness questioning and cross-examination and closing arguments from the prosecution and defense, as well as using a jury eligible sample as recommended by the standards set out by Willmott et al. (2021) for realistic mock jury research. Planned future research aims to improve adherence to these standards, including replicating our findings using a live or videoed trial and the inclusion of group deliberation.

Another important limitation to note is that although the findings from studies 2 and 3 provide support consistent with the hypothesized causal pathways, it remains extremely difficult to confirm causal relationships due to the likely influence of unmeasured confounding variables. While the experimental manipulation strengthens causal inference, we caution that these effects should be interpreted in the context of broader situational and individual differences that may also shape juror decision-making. In particular, the exclusion of demographic control variables, such as age and experiences of sexual victimization, from the structural model is a notable limitation. While data on sexual victimization was collected for contextual purposes in these studies, by not providing a formal definition of rape when asking participants if they know anyone who has been raped or falsely accused may have led to subjective interpretations. Research such as Lilley et al. (2023) found that jurors' ethnicity and experiences of sexual victimization predicted their verdict decisions. However, incorporating such factors into the model would have substantially increased its complexity and moved beyond the study's theoretical focus. Importantly, while gender differences were observed across studies, these appeared to operate primarily through rape myth endorsement rather than as independent predictors within the structural model, further underscoring the centrality of jurors' beliefs in shaping their decision-making. Nonetheless, we encourage future research which integrates these demographic and attitudinal variables to contextualize the model further and evaluate its generalizability across different juror profiles. In particular, the responsivity of *victims ask for it* beliefs to educational or situational

influences should be further examined considering the modest but consistent interaction for condition and gender found in Studies 2 and 3.

The findings presented offer a wide range of opportunities for future research, beyond merely extending to a full mock trial with jury deliberation. The model was created using rape myth factors based on female victim-survivors of rape, and as such is likely not equally applicable to cases with male or non-binary victim-survivors. While there has been limited psychometric evaluation of male rape myths since Melanson (1998), who developed a unidimensional measure, recent research has successfully adapted the items in McMahon and Farmer's (2011) U-IRMA to be gender-neutral (Reddy et al., 2022). Furthermore, for research capable of attracting large samples, the inclusion of multi-item scales to explore broader dimensions of the credibility, responsibility and empathy judgements for both victim-survivors and defendants, which would allow further validation of the proposed model. It should also be noted that given variables can only be entered as predictors or covariates within SEM, future research should consider more advanced methods that allow for the inclusion and control of other pertinent factors, such as gender.

The newly developed intervention also offers extensive potential for future research. While further testing to definitively rule out social desirability or demand characteristics as the primary mechanism for change is encouraged, the intervention could also be examined as an educational resource for other members of the criminal justice system, such as police, prosecutors and defense lawyers within the UK and in other jurisdictions, either as a stand-alone intervention or as part of a more extensive and interactive program. Furthermore, the intervention also has important policy implications. In particular, the consistent indirect gender differences observed across studies may contribute to ongoing discussions regarding jury composition in sexual offence trials (Chalmers et al., 2020; Willmott et al., 2021), where concerns have been raised about the role of jurors' beliefs in shaping case outcomes. The present findings suggest that gender differences in victim evaluations and verdict outcomes may operate through underlying rape myth beliefs. This distinction is important, as it suggests that addressing rape myths directly—through education or judicial guidance—may mitigate bias regardless of juror gender. In light of increasing reviews in common law jurisdictions on how rape myths should be addressed in rape trials, such as the Gillen (2019), the intervention evaluated in this research offers a feasible, cost-effective measure which can be easily and quickly implemented.

Our findings further support existing calls for jury education to combat myths and misconceptions about sexual violence (Gillen, 2019; Hudspith et al., 2024; Leverick et al., 2025; Willmott et al., 2021). Our intervention demonstrated it is entirely feasible to reduce the influence

of harmful rape myths and stereotypes on both victim-survivor evaluations and verdict decisions with a brief educational video, without creating a positive bias towards the victim-survivor. While further validation of the intervention video is necessary, there are clear implications for the applicability of similar juror education programs for criminal justice systems around the world. Given the plasticity of credibility judgements based on the content presented to jurors, we believe it particularly important to develop further regulations regarding the use of rape myths to undermine victim-survivor credibility by defense lawyers, as noted by Smith and Skinner (2017). Finally, the model presented offers a coherent theoretical framework to assess and evaluate juror decisions within cases of rape and sexual violence, which clearly elucidates the impact of rape myth acceptance on these decisions.

Author note

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CRedit authorship contribution statement

Sarah Mairead Kelleher: Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Caoilte Ó Ciardha:** Writing – review & editing, Supervision. **Gaye Ildeniz:** Writing – review & editing, Formal analysis.

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Declaration of competing interest

None

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Appendix A. Appendices

Appendix A
Demographic breakdown of sample (Study 1).

Sample characteristics	Study 1 (n)		Study 2 (n)		Study 3 (n)	
	Prolific	Socials	Prolific		Prolific	Socials
Recruited	251	165	505		294	235
Excluded - missing data	9	43	11		3	104
Excluded - too fast	4	6	8		1	1
Excluded - failed attention checks	2	1	4		2	6
Excluded - failed eligibility	–	–	5		5	0
Removed outliers	6		12		16	
Total sample for analysis	345 (100%)		465 (100%)		391 (100%)	
Gender						
Male	120 (35%)		197 (42%)		184 (47%)	
Female	218 (63%)		262 (56%)		196 (50%)	

(continued on next page)

Appendix A (continued)

Sample characteristics	Study 1 (n)		Study 2 (n)		Study 3 (n)	
	Prolific	Socials	Prolific		Prolific	Socials
Trans Male	4 (1%)		0 (0%)		2 (0.5%)	
Trans Female	1 (0.3%)		2 (0.4%)		1 (0.3%)	
Gender Non-Conforming	1 (0.3%)		4 (0.9%)		8 (2.0%)	
Other	1 (0.3%)		0 (0%)		0 (0%)	
Ethnicity						
African	2 (0.6%)		1 (0.2%)		3 (0.8%)	
Black/African American	2 (0.6%)		0 (0%)		2 (0.5%)	
Black/British	4 (1%)		4 (0.9%)		5 (1%)	
Caribbean	1 (0.3%)		0 (0%)		0 (0%)	
East Asian	9 (3%)		10 (2%)		2 (0.5%)	
Middle Eastern	2 (0.6%)		3 (0.6%)		3 (0.8%)	
Mixed	8 (2%)		12 (2%)		6 (2%)	
Native American	2 (0.6%)		1 (0.2%)		0 (0%)	
Other	10 (3%)		5 (1%)		8 (2%)	
South Asian	16 (5%)		19 (4%)		14 (4%)	
White/Caucasian	286 (83%)		403 (87%)		348 (89%)	
White/Mexican	3 (0.9%)		5 (1%)		0 (0%)	
White/Sephardic Jew	0 (0%)		2 (0.4%)		0 (0%)	
Sexuality						
Heterosexual	281 (81%)		399 (86%)		320 (82%)	
Bisexual	36 (10%)		39 (8%)		45 (12%)	
Homosexual	19 (5%)		20 (4%)		19 (5%)	
Asexual	5 (1%)		3 (0.6%)		2 (0.5%)	
Other	4 (1%)		4 (0.9%)		5 (1%)	
Known victim of rape						
No	180 (52%)		302 (65%)		225 (58%)	
Yes, a friend or family member	128 (37%)		119 (26%)		119 (30%)	
Yes, myself	26 (7%)		28 (6%)		28 (7%)	
Prefer not to answer	11 (3%)		16 (3%)		19 (5%)	
Known victim of false allegation						
No	309 (90%)		419 (90%)		345 (88%)	
Yes, a friend or family member	29 (8%)		36 (8%)		39 (10%)	
Yes, myself	2 (0.6%)		5 (1%)		4 (1%)	
Prefer not to answer	5 (1%)		5 (1%)		3 (0.8%)	

Note: The mean age for Study 1 was $M = 29.81$ ($SD = 9.66$), the mean age for Study 2 was $M = 36.65$ ($SD = 12.94$), and the mean age for Study 3 was $M = 39.33$ ($SD = 13.90$).

Appendix B

Regression and Covariance Coefficients for Study 1 SEM.

Pathway	b	SE	β	p
VL1 $\leftarrow$ VL	1.00			<.001
VL2 $\leftarrow$ VL	1.04	.07	.84	<.001
VL3 $\leftarrow$ VL	1.01	.06	.91	<.001
VL4 $\leftarrow$ VL	0.82	.06	.85	<.001
VL5 $\leftarrow$ VL	0.89	.08	.77	<.001
VA1 $\leftarrow$ VA	1.00			<.001
VA2 $\leftarrow$ VA	0.77	.09	.72	<.001
VA3 $\leftarrow$ VA	0.69	.09	.77	<.001
VA4 $\leftarrow$ VA	1.11	.15	.75	<.001
NR1 $\leftarrow$ NR	1.00			<.001
NR2 $\leftarrow$ NR	0.84	.09	.69	<.001
NR4 $\leftarrow$ NR	0.19	.03	.38	<.001
NR5 $\leftarrow$ NR	1.62	.20	.71	<.001
Gender $\rightarrow$ VL	−0.78	.18	−.25	<.001
Gender $\rightarrow$ VA	−0.65	.15	−.26	<.001
Gender $\rightarrow$ NR	−0.37	.11	−.22	.001
Credibility $\rightarrow$ Verdict	0.35	.04	.45	<.001
Responsibility $\rightarrow$ Verdict	−0.09	.04	−.17	.035
Empathy $\rightarrow$ Verdict	0.18	.07	.19	.009
Empathy $\rightarrow$ Credibility	0.36	.06	.30	<.001
Empathy $\rightarrow$ Responsibility	−0.43	.09	−.26	<.001
VL $\rightarrow$ Credibility	−0.33	.04	−.38	<.001
VA $\rightarrow$ Responsibility	0.86	.12	.57	<.001
NR $\rightarrow$ Empathy	−0.89	.12	−.67	<.001
VA $\leftrightarrow$ VL	1.30	.21	.77	<.001
NR $\leftrightarrow$ VA	0.80	.12	.86	<.001
NR $\leftrightarrow$ VL	0.90	.13	.79	<.001

Appendix C
Summary table.

Criteria	Study 1	Study 2	Study 3
Aims	Test theoretical model where rape myths → victim evaluations → verdict decisions	Demonstrate model robust to case variations & establish basis for causal relationships in model	Disrupt strength of rape myth → victim evaluation relationship using brief intervention
Sample	N = 345 (Prolific & social media, not fully eligible for jury duty)	N = 465 (Prolific, all jury eligible)	N = 391 (Prolific & social media, all jury eligible)
Average rape myth acceptance	M = 41.56, SD = 18.67	M = 39.55, SD = 17.53	M = 36.98, SD = 15.92
Manipulation	N/A	Case consistent with rape stereotypes Vs. Case inconsistent with rape stereotypes	Video intervention targeting rape myths Vs. Video control on climate change
Main analysis	SEM	Multigroup-SEM	Moderated-mediation
Guilty verdicts	62%	Consistent = 61% Inconsistent = 74%	Control = 75% Intervention = 84%

Appendix B. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jcrimjus.2026.102619>.

Data availability

All materials and data for the studies in this paper can be found at: https://osf.io/vhu5w/?view_only=0e73f58524f442b78dfb52c6e87b14c4.

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