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Secondary Victimization of Women Reporting Violence

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Secondary Victimization of Women Reporting Violence*

Abstract

Secondary victimization, the disbelief, trivialization, and stigmatization that victims encounter upon reporting violence, shapes the incentives of both victims and perpetrators, yet its role in violence dynamics has never been formally modeled or causally identified. I develop an economic model in which a publicly observed episode of secondary victimization triggers Bayesian updating of beliefs about victim recognition, simultaneously raising the expected cost of reporting for victims and lowering the expected cost of violence for perpetrators. I test the model's predictions exploiting the 2022 Depp v. Heard defamation trial as a source of quasi-random variation in exposure to secondary victimization. Using daily incident-level data from the National Incident-Based Reporting System and a Regression Discontinuity Difference-in-Differences design, I find that in the week following the verdict, reported intimate partner violence falls by 12.6 percent. In contrast, incidents involving major injuries rise by 34.2 percent, though imprecisely estimated. Both responses are mainly driven by states with less egalitarian gender norms and concentrated at the verdict, rather than during the proceedings, underscoring the central role of social norms and institutions in shaping the dynamics of intimate partner violence.

JEL classification

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Keywords

secondary victimization, intimate partner violence, crime reporting, belief updating, gender norms

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`believability, unlike truth, highlights the dependency of women on the judgment of others'

Banet-Weiser & Higgins 2022:142

1. Introduction

Violence against women (VAW),[1] particularly intimate partner violence (IPV) and sexual violence, is a major public health problem (WHO 2024). Estimates indicate that globally, about 30 percent of women have been subjected to either physical and/or sexual IPV or non-partner sexual violence in their lifetime and 60 percent of all female homicides are committed by an intimate partner or a family member (UN Women, 2024). Less than 40 per cent of the women who experience violence seek help of any sort. And women who do seek help look to family and friends, and very few seek support from formal institutions, such as police and health services (UN Women 2024).

The arduous judicial process that follows the reporting of violence is mentioned in qualitative studies as a barrier for reporting (Broidy et al., 2016; Silván et al., 2015). Qualitative evidence shows how police officers and judicial actors may internalize and act upon harmful myths about victim credibility when dealing with VAW cases (Page, 2010; Jordan, 2011; O'Neal, 2019), increasing the risk of secondary victimization (SV). SV has been defined as the negative institutional or societal response to victims' disclosure of violence, which includes disbelief, trivialization, and stigmatization (UNODC, 1999; Orth, 2002). This phenomenon has been documented across a range of contexts, but it is particularly salient for IPV and sexual violence, where victims may encounter skepticism from police, prosecutors, or judges, as well as stigmatization and reputational damage in public discourse (Orth, 2002; Herman, 2005).

SV has been documented to have dramatic psychological consequences on victims who experience it (Campbell et al., 1999; Orth, 2002). At the same time, public exposure to SV might impact the behavior of victims and perpetrators. Anticipated SV can deter victims from reporting, adding to already low reporting rates (Miller and Segal 2019; Carrell and Hoekstra 2012), while sympathetic treatment has been shown to increase participation (Broidy et al. 2016; Rivera et al. 2012; Maddox et al., 2011). At the same time, dismissive judicial or public responses may weaken deterrence by signaling tolerance for violence, contributing to higher prevalence. Furthermore, SV

[1] Violence against women includes domestic violence, child marriage, forced pregnancy, "honor" crimes, female genital mutilation, femicide, sexual and other violence perpetrated by someone other than an intimate partner (also referred to as non-partner violence), sexual harassment (in the workplace, other institutions and in public spaces), trafficking in women and violence in conflict situations (UN Women 2024).

might impact social norms surrounding VAW, trivializing or normalizing this form of abuse. Nonetheless, SV has never been identified in the economic literature as a factor playing a role in VAW dynamics, and whether exposure to instances of SV affects the incidence and reporting of violence is still an open question.

This paper addresses this gap in the literature both theoretically and empirically. First, by developing, to my knowledge, the first economic model of SV of women victims of male violence. Any crime victim can be denied recognition by the actors she turns to, if only for want of evidence. What the model adds is why the denial is more likely for VAW victims: for them, victim status is not granted in proportion to the evidence presented, but discounted by a factor that reflects how fairly the system treats women. Observing that discount publicly is therefore a signal about the fairness of the system, which makes victims less likely to report and perpetrators more likely to exert violence. Second, by testing these predictions and showing that exposure to SV of IPV victims negatively impacts IPV reporting while exacerbating perpetrators' violent behaviors.

Estimating the causal impact of exposure to SV is inherently challenging, largely because judicial proceedings rarely enter the public domain. A notable exception is the defamation trial between actors Johnny Depp and Amber Heard, which offered an example of SV of an IPV victim, both from the judicial system and the public. The case, broadcast live and amplified across social media, drew global attention and became a paradigmatic instance of SV (Donegan 2022; Grady 2022a; Ledro 2023; Manne 2022). Features identified by the literature as exacerbating SV, including spectator presence, victim-blaming rhetoric, and extensive media harassment, were all present, magnified by online circulation (Orth 2002). A victim rights advocate reported hundreds of domestic abuse survivors retracting their court statements as a "direct response to the Depp-Heard trial" (Grady, 2022a).

My testing ground is the US, a context in which the trial had a particularly high resonance and where the incidence of IPV is still very high, with about 41 percent of women experiencing this form of violence during their lifetime (CDC 2024). I focus on IPV, the most common form of VAW and one of the settings in which SV has been most extensively documented. Beyond its direct harms to victims' physical and mental health (Campbell, 2002; García-Moreno et al., 2013), IPV reduces women's labour supply and earnings and imposes lasting costs on children (Heise and Garcia-Moreno, 2002; Adams et al., 2024). In the United States its annual cost has been estimated at $8.3 billion (Max et al., 2004), and the lifetime cost per female victim at over $100,000 (Peterson et al., 2018).

To measure IPV, I use data from the National Incident-Based Reporting System (NIBRS) on daily reported IPV offenses, which cover almost 67 percent of US police agencies. I distinguish

between IPV cases with minor or no injuries and those with major injuries (including fatalities), which I interpret as proxies for reporting and offending behaviors, respectively (García-Hombrados et al. 2024). I corroborate the NIBRS analysis with descriptive patterns from daily series of police calls for service in Seattle. To capture reporting, I rely on domestic violence (DV) calls that enter the system as DV cases and are confirmed as such; to capture variation in incidence, I define variables counting the cases that enter the system as disturbance and are initiated by someone other than the police, so likely reflecting a third-party call, and are classified at the end as DV. I further draw on a daily series of English-language tweets carrying case-related hashtags between April and June 2022, which measures both the intensity and the direction of public discussion of the trial.

To study the impact of exposure to SV through media coverage of the Depp v. Heard trial, I use a time Regression Discontinuity Difference-in-Differences (RD-DD) design (Farré et al. 2024). This design compares changes in IPV around the trial verdict date (June 1, 2022) to the same cutoff in non-trial years (2021 and 2023) and rests on the verdict date, fixed by the court's calendar, being independent of IPV dynamics and on seasonality being stable across years. By combining within-year variation around the cutoff date with across-year comparisons, the RD-DD framework allows me to isolate the local effect of the trial verdict on IPV.

In accordance with the model's predictions, I find that in the days following the verdict, IPV reporting decreased while IPV incidence increased. During the first 7 days following the trial, I estimate a 12.6 percent reduction in reporting due to exposure to SV, an effect comparable in size to the short-run responses documented for other high-salience emotional cues (Card and Dahl, 2011); I also estimates an approximate 34.2 percent increase in IPV incidence, although not statistically significant for the entire US. The effects are concentrated in more conservative states and in states where VAW is more prevalent. In these areas, both effects are statistically significant and larger in size.

To understand the magnitude of my findings, it is key to identify the context in which the trial occurred. In the post-#MeToo era, a strong backlash has emerged (Moro et al. 2023; Renström & Back 2024), particularly against victims of male abuse (Moro et al. 2023; Dickel and Evolvi 2023). The trial's verdict, widely interpreted as institutional validation of the idea that women may fabricate IPV claims (McFarland 2022), may have reinforced skepticism toward female accusers. The recognition that women may lie about male abuse may have triggered backlash behaviors, including renewed aggression toward intimate partners, amplifying the effect of the trial on IPV incidence in more conservative states.

To the best of my knowledge, no study so far has paid attention to the role of exposure to SV in determining IPV dynamics. Theoretical and empirical evidence from the present paper show that

exposure to SV is a key driver of both victims' and perpetrators' behaviors, highlighting a critical mechanism that, if addressed, could improve reporting rates while reducing violence. In line with my empirical findings, prior studies find that prosecutorial actions increase IPV reporting (Aizer and Dal Bó 2009) and decrease its prevalence (Black et al., 2023). A study by García-Hombrados et al. (2024) estimates that the opening of IPV specialized courts increases IPV reporting, while several papers show that increasing the female representation among police officers increases IPV reporting and decreases the likelihood of future victimization (Miller and Segal 2019; Ponce de Leon 2024).

By exploiting a temporal RD-DD design, the analysis captures sharp changes in reporting and incidence in the immediate aftermath of a salient judicial event. The findings, therefore, speak directly to the instantaneous impact of SV, rather than its persistence over time. Understanding these short-run dynamics is critical for policymakers and researchers, as even transient behavioral shifts can have life-threatening consequences and shape the incentives of both victims and perpetrators to engage with the justice system in the future.

2. Related Literature

Most IPV is never reported (Carrell and Hoekstra, 2012), and a body of work shows that how the justice system handles those who do come forward shapes both disclosure and subsequent violence. Prosecutorial and arrest policies affect reporting and recidivism (Aizer and Dal Bó, 2009; Dugan et al., 2003; Black et al., 2023); specialised courts raise the number of cases brought forward (García-Hombrados et al., 2024); and the gender composition of the police affects both whether victims report and whether they are victimized again (Miller and Segal, 2019; Ponce de Leon, 2024). In all of these, the institutional response is a treatment experienced by the victims who encounter it. I ask instead what happens when that response is observed by people who are not party to the case.

The literature shows that publicly observed events can move IPV dynamics. Colagrossi et al. (2023) and Cerqua et al. (2024) find that media coverage of femicides in Italy produces a spike in help-seeking by survivors in the following week, concentrated where coverage is broadest. Media exposure can also shift the underlying norm: the arrival of cable television in rural India reduced the reported acceptability of wife-beating (Jensen and Oster, 2009), and the #MeToo movement raised reporting of sexual crimes (Gauthier, 2022; Levy and Mattsson, 2020). This literature has documented events that make victims more likely to report, while this paper focuses on an event that might discourage reporting.

At the same time, exposure to SV might increase incidence. Sociological work on male backlash holds that violence can be used to reassert a masculine identity when prescribed gender roles are challenged (Hornung et al., 1981; Macmillan and Gartner, 1999). Related work in economics

explains why public narratives that validate suspicion toward a group lower the cost of acting on it (Glaeser, 2005; Bursztyn et al., 2020), and why agents derive utility from actions that affirm a social identity (Akerlof and Kranton, 2000). A verdict widely read as confirming that women fabricate abuse claims is a signal of this kind.

The only studies that examine SV directly lie outside economics and focus on the personal experience of SV through qualitative evidence. The arduous and traumatic judicial process that follows the reporting of VAW is commonly mentioned in surveys and qualitative studies by victims as an important barrier for reporting (Broidy et al., 2016; Rivera et al., 2012). In contrast, the presence of empathy from police officers has been positively correlated with victim-survivor participation and the chance of the case being taken to trial (Maddox et al., 2011). For victims who experience it, SV has been positively associated with posttraumatic stress reactions and a loss of self-esteem, faith in the future, trust in the legal system, and belief in a just world (Campbell et al., 1999; Orth, 2002).

3. Theoretical Framework

I model victim credibility as a function of structural gender bias and of the evidence a victim can present, and embed it in a two-period model in which a public episode of SV leads agents to revise their beliefs about how victims are recognized. Both victims and perpetrators act on the revision.

3.1 Secondary Victimization and Victim's Credibility

SV occurs when victims experience additional harm not from the initial offense but from the institutional or societal response to their disclosure (Council of Europe, 2006; Campbell, 2005). It is secondary in that it follows the primary victimization of the violence itself and is inflicted by the response rather than by the perpetrator. The concept originates in the criminology and sociology literatures, where it applies to the victim of any crime who is not recognized as such by the actors she turns to (Orth, 2002; Campbell, 2005, 2008; Campbell & Raja, 1999, 2005).

SV can arise both from the outcome of the reporting and from the treatment of the victim as a consequence of reporting.[2] SV by the outcome can manifests through insufficient punishment of the perpetrator or inadequate protection of the victim. As such, it primarily pertains to the criminal justice system,[3] a system that has been widely criticized for perpetuating SV (Orth, 2002), especially in the context of sexual violence and IPV (Hohl and Stanko, 2015; Rivera et al., 2012). SV by the treatment of the victim refers to the responses to the reporting that she received from all actors to

[2] This framework builds on Orth's (2002) definition of SV in judicial proceedings, which distinguishes between SV arising from the outcome of the trial and SV arising from the procedures of criminal proceedings.
[3] The criminal justice system includes both judicial system and law enforcement.

whom she discloses violence, including whether she is believed or supported, and, on the contrary, whether she is blamed, stigmatized or mocked. Both dimensions are therefore rooted in a common underlying object: the recognition of the victim status.

Nothing in it is specific to VAW, nor does it require an institution that treats victims unfairly: a victim whose account cannot be substantiated may be denied victim status by a system doing its job. What distinguishes VAW is that the denial is more likely, because credibility in these cases is not granted in proportion to the evidence presented. Credibility in VAW allegations is closely linked to gender norms and expectations and women who report violence often face heightened skepticism, whereas men accused of abuse are more readily granted sympathy (Banet-Weiser, 2021a; Manne, 2018). Empirical evidence supports this bias: juries evaluate testimony from male expert witnesses as more trustworthy than identical testimony from female experts (Larson and Brodsky, 2010), and students rate the same online professor as more competent when believed to be male rather than female (MacNell et al., 2015), reflecting the cultural coding of rationality as male and emotionality as female (Pavco-Giaccia et al., 2019). Consequently, in VAW cases, female victims often begin with a credibility disadvantage relative to their male perpetrators.

The recognition of the victim status also depends on the victim's characteristics relative to socially prescribed norms. Following Christie (1986), individuals who closely align with the "ideal victim" archetype, those perceived as passive, vulnerable, and blameless, receive higher credibility. Deviations from this ideal reduce credibility. Women face stricter standards for being recognized as legitimate victims as these credibility assessments intersect with prevailing gender norms, including expectations of female passivity and purity (Schneider, 2008; Banet-Weiser and Higgins, 2022). Agency is a key dimension: victims with greater autonomy are sometimes held partially responsible for remaining in abusive relationships, paradoxically reducing credibility (Randall, 2011; Schneider, 2008). As bargaining power or autonomy rises, courts and society are more likely to view victims as accountable for remaining in the relationship, regardless of financial or social constraints (Schneider 2008).[4]

Finally, offender characteristics further shape victim's recognition. Following Christie (1986), an "ideal offender", clearly culpable, unknown to the victim, and without social or economic power, maximizes the likelihood that the victim is recognized as ideal. Conversely, offenders who are known to the victim or possess social or economic power are non-ideal, lowering her credibility and making SV more likely.

[4] In IPV cases, this is frequently manifested in judicial proceedings through questions directed at victims regarding why they did not leave the abusive relationship earlier. For victims of SV, critiques often target their sexual freedom or independence, framing these as decisions that placed them in an unprotected situation.

Credibility, as defined here, does not refer narrowly to whether a victim's account of events is believed. Rather, it captures the probability that a woman who reports violence is recognized as a legitimate victim, that is, as someone deserving of institutional protection and societal support. This distinction matters because SV can arise even when the facts are not in dispute. A woman may be believed to have experienced violence yet denied victim status on the grounds that she provoked the abuse, chose to remain in the relationship, or otherwise contributed to her own harm. By considering the credibility of being a true victim rather than the credibility of the reported facts, the model encompasses both channels through which SV can operate: disbelief in the offense and blame directed at the victim.

Exposure to SV might have societal consequences that go beyond the individual experience. SV can lower the perceived costs of perpetration, by signaling weak judicial consequences and shifting social norms toward tolerance of VAW; simultaneously it can reduce victims' willingness to report, by highlighting the risk of disbelief and re-traumatization and eroding the trust in the criminal justice system. A perceived weaker response to IPV disclosure reduces women's bargaining power, potentially raising their risk of violence (Farmer & Tiefenthaler 1997; Aizer 2010).

3.2 The Model

Players and actions. Individuals are either female victims or male perpetrators. Individuals are matched in victim–perpetrator pairs and matches are taken as given. Throughout, i indexes the match. Perpetrators choose whether to exert violence, $a_i \in \{0,1\}$. Victims choose whether to formally report violence, $R_i \in \{0,1\}$.

Environment. Let $\theta_i \in [0,1]$ denote the credibility granted to a female victim reporting male abuse, which can also be interpreted as the true probability that a victim who reports violence is recognized as legitimate. Let $e_i \in [0,1]$ denote the evidence she presents, normalized so that $e_i = 0$ is no evidence and $e_i = 1$ is conclusive evidence.

In a fair society, credibility tracks evidence, meaning that the credibility granted to the victim directly mirrors the evidence presented in her favor: $\theta_i = e_i$. This paper models the empirically relevant case of a gender-biased society, where credibility departs from evidence, $\theta_i = \beta_i e_i$ with $\beta_i \in (0, 1]$. Therefore, in the unfair society modeled here, evidence raises credibility, but less than one-for-one. β_i is the credibility discount that applies when the environment is biased, and it has two parts. The first, $\bar{\beta}$, is structural gender bias: the discount in a victim's credibility that is applied receives before any evidence is weighed, common to all matches in the environment. The second, $k_i \in (0, 1]$, measures how far the match is from the ideal victim and ideal offender described above, and

so how exposed it is to that bias, with $\beta_i = 1 - (1 - \bar{\beta})k_i$. These characteristics vary across matches and are known within them. They operate only through the discount: a fair environment weighs the evidence and ignores them, and where there is no structural bias they cost nothing. Both β_i and e_i vary across pairs and their value is common knowledge within a pair.

A victim who reports is recognized with probability θ_i and suffers SV with probability $1 - \theta_i$. Weak evidence alone can leave her unrecognized, in a fair environment as in a biased one; what the credibility discount adds is disbelief her evidence does not warrant, raising that probability from $1 - e_i$ to $1 - \beta_i e_i$.

$\sigma_i > 0$ is the severity of the SV victim in match i suffers if she reports and is not recognized. It is the number of people and institutions a report would reach: police, prosecutors and courts on the institutional side, family, employer and community on the societal one, each contributing its own share of disbelief, blame and loss of standing when recognition fails. Severity is therefore a feature of the match, observable in principle and varying with how widely a report circulates. The public case sets the upper end of the scale: the trial studied here was watched by millions, so σ_0 is larger than any σ_i by orders of magnitude. I treat σ_i as common knowledge within the match.

Beliefs. The environment is not directly observed. Throughout, the discount (β_i) is a property of the environment (captured by its component $\bar{\beta}$) and of the match (captured by its component k_i), not of anyone's beliefs: the environment is biased, but agents do not know whether it is biased or fair. What they do know is what each environment would mean for their own case so their uncertainty is over which of the two they inhabit, not over what either would imply. Both members of match i attach probability π_i to the environment being fair. Since credibility tracks evidence in a fair environment, π_i is equivalently the probability she attaches to her evidence counting for exactly what it is worth, $\pi_i = Pr(\mu_i = e_i)$.

On average agents place most of their belief on fairness, so the fair environment is the benchmark they read the evidence against, and their error is one of optimism. The belief about the probability attributed to victim's recognition is therefore

$$\mu_i = \pi_i e_i + (1 - \pi_i)\beta_i e_i = e_i[\,\pi_i + (1 - \pi_i)\beta_i] \qquad (1)$$

which is increasing in both the beliefs about the environment being fair and the evidence the victim can present. For a given match, μ_i lies in $[\beta_i e_i\,, e_i]$: beliefs can move it no further than the two environments allow.

Both members of match i attribute probability μ_i to the victim being recognized, while the true probability is $\theta_i = \beta_i e_i$. They therefore overestimate it by $\mu_i - \theta_i = e_i \pi_i (1 - \beta_i)$, which is positive unless the environment is fair ($\beta_i = 1$) or they are certain it is not ($\pi_i = 0$), or the victim has no evidence to present ($e_i = 0$). The overestimation is increasing in the evidence the victim has: the stronger her case, the wider the gap between the belief about victim credibility and the credibility she receives. Averaging across matches, the mean overestimation is $E[e_i \pi_i (1 - \beta_i)]$.

The expected harm from reporting is $(1 - \theta_i)\sigma_i$, but she does not observe θ_i: the cost that enters her decision is $(1 - \mu_i)\sigma_i$, the same severity weighted by the probability she attributes to not being recognized. Since μ_i exceeds θ_i whenever $\pi_i > 0$, victims understate the harm they face. None overstates it: the most pessimistic belief available is that the environment is biased, which is the truth, so no belief falls below it.

Within a match the two members share the same information: the evidence e_i is a feature of the episode itself, so both know what can be shown; the discount β_i is built from characteristics each of them knows, hers and his, and from the environment they both inhabit; I finally assume π_i to be common to the match, which is plausible for two people who share a household, a social setting and the same sources of news, and which is what makes the perpetrator's anticipation of her reporting decision well defined without a signaling structure. Beliefs, evidence and match characteristics are jointly distributed across matches, and F denotes the distribution of μ_i they induce, with density $f > 0$ on $[0, 1]$.

Timing. There are two periods. In $t = 1$ a publicly observable case may arise and, if it does, ends either in recognition of the victim or not; agents observe the outcome and update their beliefs about the environment. Period $t = 2$ has two stages: the perpetrator first chooses whether to exert violence, anticipating the victim's reporting rule, and the victim then chooses whether to report, conditional on violence having occurred.

The public signal s records what happened in $t = 1$ and takes three values, $s \in \{1, 0, \emptyset\}$: $s = 1$ if a case arose and the victim was not recognized, $s = 0$ if a case arose and she was, and $s = \emptyset$ if no case arose. As shown in Appendix A.1, whether a case arises carries no information about the environment, and only its outcome does, so beliefs remain at the prior when $s = \emptyset$. An outcome $s = 0$ revises beliefs upward, but the revision is largest for agents who put least weight on fairness to begin with, and vanishes as beliefs approach certainty in it. Since most agents start close to that certainty, good news moves them far less than bad news does. I call $\mu_i(s)$ the belief held in match i after the signal realization s. The episode studied in this paper is $s = 1$.

Payoffs. Conditional on violence having occurred, the victim's expected utility from reporting is

$$E[U_i^V(R)] = \mu_i\, b - (1 - \mu_i)\, \sigma_i \tag{2}$$

The direct cost of primary victimization enters both the reporting and the non-reporting payoff and therefore nets out: the reporting decision depends only on beliefs about credibility, the benefit of receiving support, and the severity of SV. b indicates the benefit the victim receives if she reports violence and she is fully recognized as a victim (e.g., protection from future violence and psychological and medical support). b is common within an environment and it is common knowledge.

Costs and benefits of reporting are weighted by the probability the victim attaches to being recognized, μ_i. In the absence of reporting, $U_i^V(0) = 0$. A rational victim reports if the utility when $R_i = 1$ exceeds the outside option of not reporting, that implies

$$\mu_i \geq \frac{\sigma_i}{b+\sigma_i} \tag{3}$$

meaning when the probability of being recognized as a victim exceeds the cost of reporting relative to the benefits. The perpetrator's expected utility from exerting violence is

$$E[U_i^P(a, R)] = v_i - \mu_i \tag{4}$$

where v_i describes the utility gain from exerting violence. Perpetrator gains are distributed across matches according to G, with a density on its support, and G places positive mass above μ_i for a positive measure of matches, so that violence occurs for a positive mass of perpetrators in equilibrium. Unlike the victim's case, the gain from violence accrues only if the perpetrator chooses to act, so it does not cancel and drives the perpetration decision.

If the victim reports and is recognized as a victim, the perpetrator pays a penalty that bundles a formal component (legal sanction, litigation and separation costs) and an informal one (stigma, reputational damage, loss of standing). Absent a report neither is incurred. I normalize the size of that penalty to one, so that v_i is measured in units of it and the perpetrator's expected cost of violence is simply the probability that it is imposed, μ_i. Because μ_i is increasing in e_i, the reporting condition is easier to satisfy for victims who can present stronger evidence, and the expected cost faced by their perpetrators is correspondingly higher.

Because the game is sequential, the perpetrator moves first and the victim's reporting rule is common knowledge in equilibrium, the perpetrator conditions on the equilibrium-consistent reporting decision. Conditional on the victim reporting in equilibrium, a rational perpetrator exerts violence if

$$v_i \geq \mu_i \tag{5}$$

This arises for men who derive high utility from violence or for men who face low costs if the victim reports and she is recognized as a victim. If the reporting condition fails, no cost is incurred and he exerts violence whenever $v_i > 0$. The perpetrator's problem therefore has two regimes, and the deterrent disappears for any match in which the victim stops reporting.

Updating and equilibrium. The outcome $s = 1$ is more likely when the environment is biased, so observing it leads agents to revise π_i downward, and with it μ_i. How much they revise depends on the evidence presented in the observed case, e_0: a victim disbelieved despite conclusive evidence is a far sharper signal about the environment than one disbelieved with little evidence to offer, since the latter outcome is almost as likely under a fair environment as under a biased one (Appendix A.1). From reporting condition (3), when $\mu_i(1) \geq \frac{\sigma_i}{b+\sigma_i}$ the victim reports. Under complete information, the perpetrator knows whether the victim will report. From equation (5), after the signal the perpetrator exerts violence only if $v_i \geq \mu_i(1)$.

I study the subgame perfect equilibrium of the two-period game, with beliefs updated by Bayes' rule after the public signal. For generic parameter values it is unique in pure strategies. In societies where victims' credibility is positive, men with no taste for violence never exert violence in equilibrium (formal characterization and proofs in Appendix A.2).

Although the deterrent disappears discontinuously within a match, the aggregate response is continuous. Matches differ in μ_i according to F: a given downward revision in beliefs moves only those whose prior lay just above the reporting threshold, and the aggregate increase in violence is the mass of matches that cross it. Its size is therefore governed by the density of μ_i near the threshold, which is what the heterogeneity analysis in Section 8 exploits.

Empirical Predictions. When all agents observe an event in which compelling evidence of abuse is met with institutional skepticism and public mockery, this constitutes an informative signal about the environment. I call such an episode highly informative: one in which the evidence disregarded was strong enough that disbelief is far more likely under a biased environment than under a fair one. An episode in which little evidence was offered moves beliefs hardly at all. Bayesian updating implies

that the posterior belief adjusts downward from the prior. This shock simultaneously enters victims' reporting decisions and perpetrators' violence decisions in $t = 2$.

Prediction 1: After a highly informative episode of SV, victims become less willing to report. As beliefs revise downward from μ_i to $\mu_i(1)$, the expected benefit of reporting $\mu_i(1)b$ decreases while the expected cost of SV, $(1 - \mu_i(1))\sigma_i$, increases, and every victim whose posterior falls below $\frac{\sigma_i}{(b+\sigma_i)}$ ceases to report. At the aggregate level the same revision works in two directions: in matches where violence was already occurring it leads some victims to stop reporting, and in matches that remain above the reporting threshold it makes violent some perpetrators who were previously deterred, whose incidents are reported. Neither the number of reported incidents nor the share of violence that is reported declines for every distribution of beliefs and perpetrator gains; both decline when the mass of victims crossing the reporting threshold is large relative to the mass of perpetrators crossing theirs in matches that remain above it (Appendix A.3).

Prediction 2: After a highly informative episode of SV, violence increases. As beliefs revise downward from μ_i to $\mu_i(1)$, the expected cost of violence declines, and every perpetrator whose private gain lies between the new and the old threshold becomes violent. Unlike the reporting response, this one carries to the aggregate without further restrictions: the set of violent matches weakly expands for every distribution of beliefs and perpetrator gains, both in matches that remain above the reporting threshold and in those that fall below it, where the expected cost drops to zero.

Prediction 3: The two margins are linked in one direction. The signal lowers the expected cost of violence whether or not the victim changes her decision, but a victim who stops reporting removes that cost entirely for her own perpetrator. Part of the violence response is therefore generated by the reporting response itself, through perpetrators who were previously deterred and whose private gain is positive, $v_i \in (0, \mu_i)$. Two implications follow. First, where more victims cross the reporting threshold, more perpetrators are freed in this way, so the two responses should move together across environments. Second, where the reporting decline is the same in two environments but the violence response is not, the difference cannot be attributed to the reporting channel without assuming that perpetrator gains are distributed differently across them.

Predictions 1 and 2 are tested in Section 7; Prediction 3 is tested in Section 8, where I split states along measures of gender norms and the social cost of violence.

4. The Johnny Depp vs. Amber Heard Trial as Exogenous SV Shock

4.1 Background

Johnny Depp and Amber Heard married in 2015 and separated in 2016, when Heard obtained a temporary restraining order alleging domestic violence. The divorce was finalized in 2017 and, in 2018, *The Sun* published an article calling Depp a "wife beater." Depp sued the publisher, News Group Newspapers, and the editor, for libel. In November 2020 the High Court of Justice ruled against Johnny Depp finding that the majority of the alleged assaults had occurred (Grady 2022b; Depp v NGN and Wootton).[5]

Separately, in December 2018 Heard published an op-ed in *The Washington Post* describing herself as "a public figure representing domestic abuse" (Heard, 2018). [6] In February 2019, Depp filed a defamation lawsuit against Heard in Virginia and Heard counterclaimed for defamation over statements made by Depp's legal team. This trial, that is the focus of my empirical analysis, began on the 11th of April, 2022 and concluded on the 1st of June, 2022. The jury found that Heard's op-ed defamed Depp and awarded him compensatory and punitive damages ($10,35 millions). After post-trial motions and notices of appeal, the parties settled in December 2022. Heard agreed to pay $1 million to Depp, and both sides withdrew their appeals, ending the litigation. This US trial, nationally televised and massively amplified on social media, constitutes the signal $s = 1$ in the model.

This paper takes no position on the merits of the case. The analysis does not require that Heard's allegations be true; it requires only that the trial proceedings, the public discourse surrounding them, and the treatment of Heard as a reporting victim exhibited features that the literature identifies as SV, and on this point the evidence is extensive.

4.2 Secondary Victimization During the Trial

The trial provides a source of plausibly exogenous variation in exposure to SV. Its timing was determined by the legal process, specifically, the Virginia court's scheduling, and is orthogonal to pre-existing trends in IPV incidence or reporting. The national broadcast created a common shock whose intensity can be measured through viewership data, social media engagement, and geographic variation in media penetration.

Evidence and Victim Recognition. Heard's case carried an unusual volume of evidence: text messages, photographs of facial bruising, audio and video recordings of violent outbursts, medical

[5] *[2020] EWHC 2911 (QB)* available at: https://www.bailii.org/ew/cases/EWHC/QB/2020/2911.html

[6] *"I spoke up against sexual violence — and faced our culture's wrath. That has to change"* The Washington Post. December 2018. Accessed at: https://www.washingtonpost.com/opinions/ive-seen-how-institutions-protect-men-accused-of-abuse-heres-what-we-can-do/2018/12/18/71fd876a-02ed-11e9-b5df-5d3874f1ac36_story.html

records of injuries, documentation of coordinated online harassment, and evidence of Depp's substance abuse (Palin, Brook, and Rosenberg 2022). To this can be added a judicial finding: Depp had lost a parallel defamation action in the United Kingdom, a jurisdiction whose law places the burden of proving truth on the defendant and was therefore more favorable to Depp than the American one.[7] In the notation of the model, the evidence in this case, e_0, is close to its upper bound.

Despite the high volume of evidence, she was not recognized as a legitimate victim. The jury believed that violence occurred yet denied her victim status on the grounds that she provoked the abuse. The case was framed as an instance of "mutual abuse" despite the fact that domestic violence experts regard mutual abuse as a blame-shifting device without empirical support: one partner typically holds power and control over the other (National Domestic Violence Hotline 2025), as the bargaining model of the household implies (Farmer and Tiefenthaler, 1997; Aizer, 2010).

The distance between the evidence presented and the credibility granted is the credibility discount β_0, and Section 3.1 names three determinants of it, which the trial exposed. First, the gender credibility bias: identical behavior was read asymmetrically. Heard's emotional displays were taken as irrational where Depp's were received as signs of vulnerability; similarly, despite both being famous actors, she was accused of acting in court and faking her emotions while his testimony and emotional displays were interpreted as authentic (Ledro, 2023). Second, Heard departed from the ideal victim on several dimensions: her economic and social independence, and instances of physical retaliation contributed to the reverse victim-blaming narrative surrounding the trial. Third, Depp's fame, public persona and disclosure of childhood trauma placed him far from the ideal offender (Christie, 1986), generating sympathy that eroded Heard's credibility further.[8]

What makes the episode informative is the configuration rather than any single element of it: the weight of the evidence, and the fact that it was disregarded in court and in public alike. A victim presenting evidence of this weight is recognized with probability e_0 in a fair environment and $\beta_0 e_0$ in a biased one; that Heard was not recognized is therefore far more likely if the environment is biased. This is what the likelihood ratio records (Appendix equation A1): L is close to zero, and the episode is highly informative precisely because the evidence disregarded was strong.

Exposure to the Trial and Public Response. The SV Heard experienced was σ_0, the size of the public that followed the trial. The audience following the trial was unprecedented for an IPV case.

[7] In the UK, the defendant typically bears the burden of proving that a statement is true, while in the U.S., defamation law provides broader protections for speech, and plaintiffs must prove the falsity of the statement and harm resulting from it (Johnson, 2016; Baughman 2023).

[8] According to 2022 estimates from HNGN, Amber Heard's net worth of approximately $8 million was about 8 percent of Johnny Depp's $100 million (HNGN 2022).

Three networks broadcast the proceedings live on YouTube; over six weeks the Law & Crime Network broadcast alone accumulated 83.9 million hours watched, peaking at 3.5 million concurrent viewers as the verdict was announced (Streams Charts 2022; Appendix Figure B1, Panel a). Figure 1, Panel (a) plots English-language tweets carrying case-related hashtags: nearly 900,000 were posted on the day of the verdict. Mentions on Twitch rose through the trial, reaching 24,000 for Depp on June 1 (Appendix Figure B1, Panel b).

Public reaction was overwhelmingly one-sided. Media coverage ran strongly in Depp's favor (Rothschild and Fischer, 2022), as did several public figures.[9] Among tweets that took a side, those supporting Depp outnumbered those supporting Heard by roughly ten to one over the proceedings, and by more on the day of the verdict; support for Heard peaked during her own testimony and even then remained an order of magnitude below the opposing volume. Heard was not merely disbelieved but ridiculed. Social media recast Heard as a fame-seeker and subjected her to viral mockery and demeaning caricature (Baughman, 2023; Goldberg, 2022). Her testimony, including allegations of sexual assault, was characterized as staged, with memes and videos mocking the plausibility of her account (Watercutter 2022); countless videos skewered her on social media and influencers reported that pro-Heard content lost followers while pro-Depp content went viral and generated revenue (Ligon, 2023; Winter 2022).[10]

The signal $s = 1$ is the failure of a public victim to be recognized. Recognition can be withheld by institutions and by the public, and in this case, both withheld it: the harassment campaign that ran through the trial and the verdict that followed were refusals to credit the same account, reached on the same evidence. The model does not separate them. What agents observe is that a victim presenting this much evidence was not believed, and the coincidence of the two refusals is what makes the episode informative of the environment rather than of one courtroom.

The cultural climate. The trial took place during a widespread backlash to #MeToo (Moro et al. 2023; Grady 2023; Renström & Back 2024).[11] The movement had initially raised the credibility of survivors and reporting rates (Gauthier 2022; Levy & Mattsson 2020), but counter-narratives soon emerged portraying men as frequent victims of false accusations (Banet-Weiser 2021b), diffusing

[9] "Believe all women, except Amber Heard," Chris Rock joked (Murphy 2022). A "Saturday Night Live" turned one of Depp's wildest accusations against Heard into a skit, treating her as a figure of ridicule and him as a charming scamp (Itzkoff 2022).

[10] Misinformation was also widely shared, with researchers identifying bots artificially spreading content to discredit Heard (Romano 2022). News organizations also played a role: *The Daily Wire* reportedly spent nearly $50,000 promoting misleading narratives on social media (Winter 2022; McCool & Narayanan 2022).

[11] Originating in late 2017 with high-profile allegations against film producer Harvey Weinstein, the #MeToo rapidly expanded as women, and later, men, publicly shared experiences of sexual harassment and assault across industries. The movement had substantial consequences, including dismissals, resignations, criminal prosecutions, civil suits, and the introduction of federal legislation aimed at curbing workplace misconduct and strengthening survivor protections.

widely online and especially among young men (Renström & Back 2024). Survey evidence tracks the shift: the share of respondents agreeing that "when it comes to giving women equal rights with men, things have gone far enough" rose from 33 percent in 2019 to 40 percent in 2024, reaching 47 percent among men, nearly half of whom also agreed that women's equality efforts now discriminate against men (Ipsos 2019, 2024). Commentators place the trial alongside other 2022 developments pointing the same way, including the reversal of Roe v. Wade (Donegan 2022; Goldberg 2022).

The argument the trial provoked was not just about Depp and Heard. It was about whether women who report violence should be believed, a question that preceded the case and found in it a focal point (Dickel & Evolvi, 2023): mentions of Depp on Twitch peaked at 14,000 on May 27, the final day of hearings, coinciding with a closing argument by his lawyer on the importance of fair investigation of domestic violence against men (Streams Charts 2022), a day that also marks a peak of pro-Depp tweets in Figure 1. Such reverse-victimization claims connect to the "mutual abuse" framing and to the debate over ideal and non-ideal victims that #MeToo had sought to settle in survivors' favor (Gill 2022). What audiences took from the case was therefore not only a judgment about Heard but also a statement about how institutions and the public respond to women who report violence, which is what the model requires of a signal about the environment.

Where these narratives run strongest, the verdict may do more than lower the cost a perpetrator expects to pay. It publicly vindicates the belief that women's accusations are false and that men are the ones treated unfairly, and the violence that follows such a vindication is an act of anger rather than a recalculated risk. Section 8.2 returns to this possibility.

5. Data

Descriptive statistics of all the variable used in the paper are included in Table 1.

5.1 National Incident-Based Reporting System Data

To measure violence, I use information on reported cases of IPV from the National Incident-Based Reporting System of the Uniform Crime Reporting Program (UCR), a collection of incidents reported to individual police agencies administered by the Federal Bureau of Investigation (FBI). The NIBRS provides detailed information on victims, known offenders, and relationships between victims and offenders for each reported crime incident. The data are based on the voluntary submissions of crime information by local law enforcement agencies. In 2022, the NIBRS was covering about 66.5 percent of the universe of US agencies, covering 73 percent of the US population (U.S. Department of Justice 2022).

Because NIBRS is a voluntary system, the set of reporting agencies changes over time, creating an unbalanced panel in which a zero-incident cell may reflect either the absence of crime or the absence of transmission. To address this, I apply a sequence of filters designed to retain only agencies that report consistently throughout the estimation window (details in Appendix C.1). Starting from 14,044 agencies that submitted at least one incident between 2021 and 2023, I first balance the panel by requiring activity in all 36 months (6,893 agencies) and drop 607 with missing population. I then apply five sample-selection criteria that exclude agencies whose reporting begins too late or ends too early relative to the estimation window, and those exhibiting sharp volume discontinuities indicative of changes in transmission rather than in underlying crime. This screening approach is consistent with standard practice in the NIBRS literature (RTCI 2024). Finally, to reduce the percentage of zeros, agencies covering a population smaller than 50,000 individuals are dropped from the sample. The final balanced sample includes 779 agencies and 853,005 observations for the period 2021-2023. Data are aggregated at the day-agency level. Summary statistics are presented in Table 1.

To code IPV, I select all cases in which the victim is female, and the perpetrator is the male spouse (including common-law spouse), boyfriend, ex-spouse, or ex-boyfriend of the victim. The offenses considered are murder/nonnegligent manslaughter, negligent manslaughter, justifiable homicide, kidnapping/abduction, rape, sodomy, sexual assault with an object, fondling, aggravated assault, simple assault, and intimidation.

Following the literature (García-Hombrados et al. 2024), I disentangle the effect of the trial on violence reporting and incidence by coding two types of violence: IPV with minor injuries (representing reporting) that do not need urgent medical attention, and IPV with major injuries (representing incidence) that need urgent medical attention, including fatalities. I code as IPV with major injuries all cases that involve broken bones, possible internal injury, loss of teeth, severe laceration, unconsciousness, other major injury, and death. IPV with minor injuries are offenses in which the victim had no or minor injuries. Incidents involving major injuries require urgent medical attention and therefore come to the attention of the authorities through hospitals, third parties and attending officers, largely independently of whether the victim chooses to report. Their count tracks violence rather than disclosure, which is what allows the two margins to be separated. For the same reason the two series proxy for reporting and incidence rather than measuring them: their magnitudes are not comparable, and the comparisons that follow are across groups within an outcome rather than across outcomes.

In the main sample, there are on average 1.2 cases IPV with minor injuries per agency and day and 0.07 cases IPV with severe injuries per agency and day (Table 1). Figure 2 plots the monthly

weighted average of IPV reporting and incidence across treated and control years, showing closely parallel trajectories.

5.2 Twitter Data

For the social media component, historical data from X (formerly Twitter) are accessed through the X API. Since I focus on the US, I only retrieved English tweets for the trial time window. Location is not used as a filter, since post-level geolocation is optional and unavailable for a large share of posts.

To construct the search terms, I first defined a set of keywords intended to capture as much discussion of the case as possible while minimizing unrelated posts. These include the full names "Amber Heard" and "Johnny Depp," posts containing both "Amber" and "Johnny," posts containing both "Heard" and "Depp," and the phrases "Depp Heard," "Depp vs Heard," and "Depp v Heard." Terms such as Amber, Johnny, Heard, or Depp are not used on their own because they may capture unrelated people or discussions.

The search strategy also separately specifies case-related hashtags, since keyword searches may not capture them. I classify the hashtags as either neutral, such as #DeppVsHeard, when they identify the case without clearly indicating support for either side, or as sided hashtags expressing support for Johnny Depp or Amber Heard, such as #JusticeForJohnnyDepp and #JusticeForAmberHeard. The hashtag list was compiled using hashtags identified in previous studies, hashtags appearing in the initial test data, and additional variants identified through a scan of archived Twitter trends[12] (Jin and Bouvier, 2025; Robinson and Hiltz, 2024; Nelson, 2025). The list of hashtags used is include in Appendix C.2.[13]

5.3 911 Calls for Service

I complement the incident-level NIBRS analysis with daily series of police calls for service in Seattle.[14] Calls for service record an earlier stage of the reporting process than an incident report. They capture what the caller told the dispatcher, what the responding officer concluded on scene, and

[12] https://archive.twitter-trending.com/

[13] Because X imposes a character limit on individual search queries, it is not possible to include every candidate hashtag in the combined specification. The core hashtags identified in the literature or clearly specific to the case are retained. For additional hashtags and name variants, I estimated the marginal number of posts each one adds around the verdict and prioritized those with the largest additional volume. Very generic hashtags that could capture substantial unrelated discussion, such as #MenToo and #BelieveAllWomen, are excluded from the hashtag list, even though they were also used in discussions of this case.

[14] Seattle is the only city whose CAD dictionary supports the decomposition used in this section. It records an initial and a final call type drawn from the same vocabulary, that vocabulary contains a generic domestic-disturbance category alongside the violence-coded ones, and the classification is stable across the whole sample period.

how the event was ultimately classified. The data come from the city's open-data portal, the Seattle Police Department Call Data. I keep 2021--2023, the same window as the main analysis, and collapse to the city level to a balanced daily series of 1,095 days with no gaps. Records that duplicate an existing event are removed, as are system test records. After cleaning, I observe 961,644 events.

The system assigns a call type twice: once at intake, when the call is received, and once at closure, after the responding unit has been on scene and also flags officer-initiated events. My outcomes are built from these three fields. [15] To capture reporting, I rely on DV calls that enter the system as DV cases and are confirmed as such. I code two variables based on the severity of the violence. Severe DV defines calls whose initial and final classification is an DV incident of severe physical violence, including critical DV assaults and DV cases imposing mandatory arrests. Less severe DV includes all DV cases except the severe ones. To capture variation in incidence, I define variables counting the cases that enter the system as domestic or general disturbance/complaint and are initiated by someone other than the police, so likely reflecting a third-party call, and are classified at the end as DV. I build two variables from this definition based on the severity of the violence reported at arrival, mirroring the definition used above.

5.4 Additional Variables

County-level weather information on daily temperature and precipitation are from PRISM which is part of the Northwest Alliance for Computational Science and Engineering (NACSE), at the College of Engineering at Oregon State University.

To measure internet access across counties, I use broadband coverage data from the FCC Broadband Data Collection (released 19 June 2025), reporting availability as of June 2022. I construct an indicator for whether at least 90 percent of broadband serviceable locations in the policed territory have fixed service at the FCC 100/20 benchmark (for details on variable construction see Appendix D).

To compute the heterogeneous analysis, I measure the prevalence of lethal violence against women as the rate of females murdered by males in single victim–single offender incidents per 100,000 female residents, pooled over 2015–2019 and computed from the FBI Supplementary Homicide Reports as compiled in the Violence Policy Center's annual *When Men Murder Women* reports. Pooling over five pre-pandemic years serves two purposes. It keeps the measure predetermined with respect to the verdict and free of pandemic-period disruptions, which matter here because lockdown stringency varied across states and has been linked to IPV. And it reduces sampling

[15] As the system do not contain information on the relationship between victim and perpetrator, I use cases of DV instead of IPV.

noise: female homicides are few in small states, so a single year is an imprecise measure of the underlying rate precisely in the states that determine a median split. Beyond 2020 the series cannot be extended: the Violence Policy Center suspended its state-level rankings from the 2021 data onward, when the retirement of the Summary Reporting System left 11 states with fewer than 60 per cent of their agencies reporting. Alabama and Florida do not submit Supplementary Homicide Report data and are therefore absent in every year.

6. Empirical Strategy

My identification strategy exploits the timing of the trial, which was plausibly orthogonal to pre-existing trends in IPV. The key empirical challenge is the duration of the trial. On June 1, 2022, the jury verdict officially dismissed Heard's abuse allegations as defamatory, rendering the gender credibility gap explicitly observable to the public. June 1 also coincided with the peak broadcast audience of 3.5 million concurrent viewers (Streams Charts, 2022) and with the maximum of daily public discussion of the trial (Figure 1). The verdict was therefore both the starkest institutional statement of the proceedings and the one delivered under the widest public attention, and it is the combination that makes it the sharpest signal in the sense of the model. I therefore use June 1 as the primary cutoff date, at which I expect the sharpest downward revision in beliefs from μ_i to $\mu_i(1)$, and test Predictions 1 through 3 against this date.

Nonetheless, the public mockery of Heard and media bias in favor of Depp accumulated throughout the trial, implying that beliefs may have begun updating prior to the verdict. To test for this, I implement an RD-DD specification that excludes the trial period entirely, comparing post-verdict outcomes to a pre-trial baseline. To further disentangle the verdict effect from intra-trial dynamics, I estimate the main specification using pre-verdict hearing dates as alternative cutoff dates, tracing whether the effect emerges progressively or concentrates at the verdict.

A pure RD design around June 1 is unfeasible for two reasons. First, the continuity-of-outcomes assumption is unlikely to hold at this specific date: IPV exhibits well-documented end-of-month spikes driven by the timing of fixed income payments (Hsu, 2017; Santantonio and Masi, 2024), introducing discontinuities at month boundaries that are unrelated to the trial. More broadly, the post-verdict period overlaps with seasonally elevated IPV rates in summer months, violating the smooth potential outcomes assumption required for RD validity. Similarly, a pure Difference-in-Differences design is not available because there is no plausible untreated control group: the trial was nationally broadcast and virtually the entire United States was exposed to the SV signal.

I therefore adopt a time Regression Discontinuity Difference-in-Differences design (RD-DD; Farré et al., 2024), which addresses both problems simultaneously. The RD-DD compares changes

in IPV outcomes around the June 1 verdict date to changes around the same calendar date in non-trial years (2021 and 2023), so that the control group consists of observations from the same seasonal window in counterfactual years (Persson and Rossin-Slater, 2024). This absorbs both the end-of-month payment cycle and the summer seasonality that would confound a pure RD. As validation, I report simple RD estimates alongside the main results; these yield coefficients of the same sign and comparable magnitude with larger standard errors, consistent with the RD-DD successfully absorbing the noise sources described above.

The dependent variable is a non-negative count with a large mass at zero, which motivates a multiplicative specification of the conditional mean. I estimate it by Poisson pseudo-maximum likelihood, which is consistent provided only that the conditional mean is correctly specified, whatever the true distribution of the outcome (Gourieroux et al., 1984; Santos Silva and Tenreyro, 2006), and which retains this property with fixed effects in panels of this kind (Wooldridge, 1999).

To avoid contamination from Covid-19 disruptions to IPV reporting behavior and judicial activity, I restrict the sample to 2021--2023, retaining two non-trial years as controls. Seasonality in IPV is controlled by the design itself: by comparing outcomes around June 1 in the trial year to the same calendar window in 2021 and 2023, the RD-DD differences out any systematic seasonal variation that is stable across years, without requiring additional seasonal fixed effects. In time-based RD designs, covariates serve to prevent bias rather than merely improve precision, because time-varying factors correlated with the running variable can confound the local comparison even within a narrow bandwidth (Hausman and Rapson, 2018). Two sources of bias require explicit controls. First, because time spent at home is positively associated with IPV (Dugan et al., 2003), daily weather conditions may shift IPV rates independently of the trial; I therefore include weather controls in all specifications. Second, weekends and public holidays exhibit systematically elevated IPV rates (Vazquez et al., 2005); each regression includes a dummy variable for these dates. I estimate the following RD-DD specification:

$$E[IPV_{a,d,t}] = \exp[\,\alpha_1 Post_d + \alpha_2 Treat_t + \alpha_3 Center_d + \alpha_4 Post_d \cdot Treat_t + \alpha_5 Center_d \cdot Post_d + \alpha_6\, Center_d \cdot Treat_t + \alpha_7 Center_d \cdot Post_d \cdot Treat_t + \delta_a + \zeta_t + \gamma' X_{a,d,t} + \log(pop_{a,t})] \qquad (6)$$

where *a* indexes the agency, *d* indexes the day, and *t* indexes the year. $IPV_{a,d,t}$ is the number of IPV cases in agency *a,* day *d*, and year *t*. The running variable is calendar day *d,* with the cutoff date $c = June\ 1$. $Post_d$ is an indicator equal to 1 if $d \geq c$ and zero otherwise; $Treat_t$ equals 1 for the treatment year ($t = 2022$) and 0 for the control year. $Center_d$ is the distance in days from day *d* to the cutoff *c* (June, 1). δ_a denotes agency fixed effect, and ζ_t are year fixed effects (for control years, the 2022 fixed effect is captured by $Treat_t$). Standard errors are clustered at the agency level. $X_{a,d,t}$ is

a vector of time-varying county-level covariates, including daily temperature, daily precipitation, and vacation days dummies (weekends and holidays). The exposure variable included in all regression is the population covered by each agency ($\log(pop_{a,t})$).

A linear specification constrains the effect to a constant slope on either side of the cutoff. I also estimate a specification quadratic in the running variable, which allows the effect to decay, and report it at the wider bandwidths, where the constant-slope restriction is least tenable. At narrow bandwidths the additional parameters leave too few degrees of freedom for the quadratic to be informative so the linear specification is preferred there.

The main specification excludes day-of-week fixed effects. With a bandwidth smaller than 8 days on each side of the cutoff, the linear trend and the day fixed effect are the same object. From 8 days per side the block retains a free dimension and the slope is identified within it; at 14 it retains 7, and the weekday profile may in addition be allowed to vary across years. I accordingly calculate weekday-adjusted estimates at 14 days obtaining coefficients in line with the main specification.

Bandwidths are selected using MSE-optimal RD procedures and their suitability assessed within the time-based RD-DD framework. In a regression discontinuity in time the running variable is the calendar itself, so any other determinant of IPV that moves with the date is a potential confounder, and the scope for such confounding grows with the width of the window (Hausman and Rapson, 2018). This argues for estimating as locally as possible. I therefore select a maximum bandwidth of 20 days on each side of June 1 and present the estimated coefficients across the entire set of bandwidths from 5 to 20 days. Because the RD estimand is local, its magnitude depends on the bandwidth over which it is estimated, and a small set of columns cannot reveal whether an estimate is sensitive to that choice. Plotting the estimate as a function of the bandwidth makes the dependence observable and separates variation in the point estimate from variation in its precision. A detailed discussion of bandwidth selection and related identification concerns is provided in Appendix E. The preferred specification, used to interpret the results, uses a window of one week on each side of the cutoff since this is the narrowest window that balances the calendar days on both sides of the cutoff.

The calls-for-service estimates use the same design as the main specification, with two differences due to the nature of the data. The regression uses heteroskedasticity-robust standard errors and the bandwidth ranges from ±10 to ±20 days, as bandwidths below ±10 days leave fewer than twenty observations per cell due to the limited number of observations.

6.1 Identifying Assumptions

The RD component of the design requires that, absent the trial, potential IPV outcomes would have evolved smoothly through 1 June, conditional on controls and fixed effects. The verdict date itself

was fixed by the court's scheduling calendar. Two threats bear on this assumption. The first is a pre-existing trend: if IPV dynamics were already shifting before the proceedings opened, the estimate would capture that trend rather than the effect of the verdict. The second concern is contamination: behavior may have started adjusting during the proceedings before June 1, as the public SV channel accumulated. This assumption is tested in Section 7.2 through a test for anticipatory responses before the opening of the trial, and a comparison of estimates over a sample including the trial proceeding and one excluding it.

The difference-in-differences component additionally requires that seasonal patterns in IPV around 1 June are stable across trial and non-trial years. Figure 2 plots monthly weighted averages of both outcomes in the three years: levels and seasonal shape track one another closely, which is the stability the design relies on.

In classical regression discontinuity designs the McCrary (2008) density test checks for sorting around the cutoff; in a time-based design the test is logically irrelevant, since time cannot be manipulated (Hausman and Rapson, 2018).

Overall exposure to the trial in my sample was high: 95 percent of included agencies are in urban counties, where broadband access is near-universal (91 percent, against 87 percent in rural areas; US Census Bureau, 2024). Nonetheless differences in exposure across counties exist, so I test the validity of the design through heterogeneous analysis by internet speed.

7. Results

7.1 Main Results

Figure 3 plots the estimated effect of the verdict across bandwidths of 5 to 20 days, while coefficients are reported in Table 2. Panels (a) and (b) show the effects for IPV reporting under a linear and a quadratic specification respectively. Panel (a) shows an estimated reduction of roughly 11 to 13 percent for windows up to about one week, which attenuates beyond 8 days and settles near 5 percent at the widest bandwidths. Panel (b) of Figure 3 shows that this attenuation is a feature of the linear fit rather than of the effect. Under the quadratic specification the point estimate remains close to 10 percent at every bandwidth from 5 to 20 days, significant at conventional levels and comparable in magnitude to the linear estimates at the narrow bandwidths, where the constant-slope restriction is least binding.

At the preferred bandwidth of 7 days on each side of the cutoff, the estimated reduction in IPV incidents involving minor or no injuries is 12.6 percent. Absent the trial, the agencies in the sample would have recorded approximately 7,960 incidents in the week following the verdict, against

the 6,929 observed: the verdict accounts for roughly 1,031 reports that were not made,[16] a net figure, since incidents newly generated in matches that still report enter the count with the opposite sign (Appendix A.3).

Taken together, these estimates confirm *Prediction 1*: exposure to a highly informative episode of SV depresses reporting. Because reported incidents fall only when the victims silenced by the verdict outweigh the perpetrators it makes violent in matches that remain above the reporting threshold, the estimated decline also establishes which of the two aggregate effects dominates. The magnitude of the effect is large but not without precedent for a high-salience media event. Card and Dahl (2011) find that an unexpected loss by the local NFL team raises police reports of at-home male-on-female violence by roughly 10 percent in the hours that follow.

Panels (c) and (d) of Figure 3 repeat the exercise for IPV incidents involving major injuries. Panel (c) shows an estimated increase of roughly 40 percent at 5 days, which declines steadily as the window widens and is indistinguishable from zero beyond 12 days.[17] Panel (d) of Figure 3 confirms again that the quadratic specification does not attenuate in the same way: the estimate remains between 25 and 40 percent for windows of 6 to 13 days before decaying at the widest bandwidths. The estimates are positive at every bandwidth up to 12 days and under both specifications, and at the preferred window the implied increase is economically large (34.2 percent). None of them, however, is distinguishable from zero at conventional levels. Incidents involving major injuries are far rarer than those involving minor or no injuries, and the standard errors in Panel (c) are roughly four times those in Panel (a) at every bandwidth. The consistency of the sign across windows and specifications is therefore the informative feature of these estimates, and I read them as evidence on the direction of the incidence response rather than on its magnitude, confirming *Prediction 2*, without precisely quantifying the effect.

The sign of these estimates carries more weight than their precision, because it discriminates between the two readings of the reporting result. Had the verdict reduced the underlying incidence of IPV rather than the propensity to report it, incidents involving major injuries would have fallen alongside those involving minor or no injuries. These results are therefore inconsistent with the interpretation that the fall in reported minor-injury IPV reflects a fall in violence.

[16] The baseline specification includes an interaction between the running variable and the treatment within the post-verdict window, so the estimated effect on day k after the cutoff is $\beta + \gamma k$. The slope γ is small and imprecisely estimated for both outcomes (t = 0.75 for minor-injury and t = 0.25 for major-injury incidents), so the data are uninformative about the profile of the effect within the week. I therefore hold the effect constant over the window when computing counterfactual totals. The resulting coefficients are -0.139 and 0.287, against -0.135 and 0.294 at the cutoff (Appendix Table F1).

[17] Because major-injury incidents are rare, the set of agencies entering the incidence regressions widens with the bandwidth, from 435 at 5 days to 651 at 20: agencies recording no severe incident within the window do not contribute to the Poisson likelihood and are dropped. Restricting every bandwidth to the 435 agencies that enter at the narrowest window, so that the sample is constant by construction, leaves the series essentially unchanged (Appendix Figure F1).

In counterfactual terms, absent the trial the agencies in the sample would have recorded approximately 277 incidents involving major injuries in the week following the verdict, against the 369 observed: roughly 92 additional incidents. This is a substantial figure given that the outcome captures only the most serious cases, including fatalities. Consistent with the imprecision of the underlying coefficient, however, it is not distinguishable from zero at conventional levels, and I read it as an order of magnitude rather than as an estimate of the number of incidents caused.

The comparison across outcomes also speaks to the mechanism. A compositional shift, with perpetrators escalating from minor to major violence, would raise severe incidence while mechanically reducing the count of minor-injury incidents, and could in principle generate part of the estimated decline without any change in victims' willingness to come forward. Comparing the two margins requires estimating them on the same agencies. Because incidents involving major injuries are rare, agencies recording none within the estimation window do not contribute to the Poisson likelihood and are dropped. The major-injury regressions are therefore estimated on 501 of the 773 agencies at the preferred bandwidth, a set that nonetheless accounts for 86 percent of minor-injury incidents and 80 percent of the covered population. Restricting both to the common set leaves the reporting estimate essentially unchanged, at -12.3 percent against -12.6 percent on the full sample (Appendix Table F2). On that common set of agencies, the counterfactual for minor-injury incidents is 6,828 against 5,988 observed, so the verdict accounts for 840 reports that were not made; the counterfactual for major-injury incidents is 277 against 369 observed, so it accounts for 92 additional severe incidents. Severity reclassification can therefore explain at most 11 percent of the reporting decline. The reduction in reporting is more plausibly driven by a decline in victims' willingness to come forward than by a change in the composition of violence.

7.2 Identification and Validation

Identification. To assess whether pre-verdict exposure to the trial contaminates the post-verdict estimates, I estimate an RD-DD model excluding the full trial period (April 11--May 31). As shown in Figure 4 (and Appendix Table F3 for the coefficients), these estimates are directionally and sizably consistent with the main results: at the 7-day bandwidth the reduction in minor-injury IPV reporting is 9.9 percent, while the increase in major-injury incidence is 21.9 percent. Neither difference with the main coefficients is statistically significant (p-value 0.55 for reporting and p-value 0.65 for incidence). Contamination of the pre-period is therefore not a concern for my design.

A further concern is anticipation. Figure 5 plots estimates for the ten days preceding the opening of the trial (March 12--21). Both reporting and incidence show two isolated spikes, on March 13 and March 20. Both are Sundays, when IPV is typically higher, and both coincided with major

sporting events; March 13 also coincided with intensive coverage of the reauthorization of the Violence Against Women Act. Because incidence rises alongside reporting, I cannot say which margin moved, and the pattern is consistent with transitory, date-specific shocks of the kind documented by Card and Dahl (2011) rather than with a systematic pre-trend. In any case, these spikes do not threaten identification: the cutoff falls on a weekday in all three years (Tuesday, Wednesday and Thursday), so a Sunday pattern cannot generate a discontinuity at the threshold, and all specifications include weekend fixed effects.

Design and Sample Validity. The validity of the RD-DD design rests on the assumption that the verdict date is exogenous to potential IPV outcomes and that, conditional on fixed effects and covariates, no other shock occurs precisely at the June 1 cutoff. As discussed in the empirical strategy, the continuity assumption required by a pure RD design may be violated in this context due to evidence linking fixed payment timing to IPV (Hsu, 2017; Santantonio & Masi, 2024), pronounced seasonal patterns in the outcome, and the high share of zeros in daily agency-level counts. Appendix Figure F2 presents the RD-only estimates for reference: coefficients are similar in sign and magnitude to the RD-DD baseline but exhibit larger standard errors, consistent with the RD-DD framework absorbing variation that a standard RD cannot.

The effect is not driven by the choice of the control years: when I add 2019 to my sample (Table 2, column 8) coefficients stay consistent in direction and size with the baseline results. Because NIBRS coverage was thinner in 2019 (many agencies moved to incident-based reporting after January 2021) rebalancing the panel on 2019 retains only 70 percent of agencies, and selects against later adopters; I therefore use the 4-year sample as a robustness check rather than as the main specification.

The model design also relies on the assumption that treatment passes through exposure to the trial. Broadband coverage is not evenly distributed across the territory these agencies police. I ask whether the effect of the trial on IPV differs between agencies whose policed population has high-speed service and those whose does not, using an indicator for whether at least 90 percent of broadband serviceable locations in the policed territory have fixed service at the FCC 100/20 benchmark. Table 3 reports the results. For IPV reporting, among agencies below the benchmark the effect on minor injuries is indistinguishable from zero. Among those above there is a significant effect of -0.141, and the difference of -0.163 is significant at the 10 percent level. The effect is concentrated where the policed population has broadband coverage, and such areas are typically urban: across the counties in my sample coverage averages 0.979 in urban areas against 0.854 in rural ones, and the rural share of serviceable locations negatively correlates with county-wide broadband coverage. For

major injuries the effect is positive in both groups consistent with the paper's main estimates but their difference is non-significant likely due to the test having little power to discriminate.

Falsification Tests. To assess whether the estimated discontinuity is unusual, I re-estimate the main specification placing the cutoff at every other admissible date of the year. Admissible dates exclude the trial period, the verdict and their estimation windows. Each placebo estimate uses the same 7-days bandwidth, controls, fixed effects, exposure, and clustering as the main specification. The permutation p-values are 0.03 for minor-injury IPV and 0.1 for major-injury IPV (Figure 6), indicating that 3 and 10 percent of placebo estimates, respectively, are as large as the true estimates. [18] I also re-estimate equation (6) on the two non-trial years alone, treating one of them as if it were the treated year, finding overall null results (Figure 7).

Estimating equation (6) for three property crime categories (larceny/theft, fraud, and a composite of other offenses: arson, extortion/blackmail, embezzlement, stolen property, and bribery) yields uniformly null results (Figure 8), supporting the interpretation that the estimated effects are specific to IPV rather than reflecting a general post-verdict shift in criminal activity.

7.3 Timing of the Response

Public attention to the trial was not constant. Figure 1 shows that daily discussion volume peaked sharply at the verdict, exceeding twice the volume of any other day of the proceedings, including the opening of testimony and the closing arguments. The comparison of the estimates with and without the trial provides a rough quantification of the relative importance of the verdict compared to the trial proceedings. The baseline specification captures the discontinuity at the verdict. Because the pre-verdict side of this window includes the trial period, the baseline estimate reflects primarily the additional effect at the verdict itself. The specification that removes the trial weeks, by contrast, captures the total effect of the trial-and-verdict episode relative to normal times. The fact the difference between the two estimates for both incidence and reporting is small, non-statistically significant and likely due to noise in the estimation suggest that the verdict is the main driver of the estimated effect. The verdict cannot be read as a purely institutional signal as public engagement peaked on the same day with concurrent viewership reaching 3.5 million, higher than on any other

[18] Other random events might have an impact on IPV at the local level. For example, on October 3, some US states (especially Florida) were severely affected by Hurricane Ian, with the power fully coming back on the 7 of October (the Florida Power & Light reported restoring power to 99 percent of customers, with full restoration expected by the end of the day, on that date). The literature shows a strong association between natural disaster and IPV (Sekhri and Storeygard, 2014; Díaz and Saldarriaga, 2023). The same reasoning can be applied for example to major layoffs events characterizing the second half of 2022 (Anderberg et al. 2016) or sport events (Card and Dahl 2011).

day of the proceedings. The estimate at the cutoff therefore bundles the institutional signal with the societal reaction it triggered.

I further descriptively explore potential transitory behavioral changes during the trial proceeding by re-estimating equation (6) using every active trial day as an alternative cutoff date. [19] [20] Because the trial continued beyond each of these points and media coverage evolved continuously, these dates do not represent sharp discontinuities; the estimates should therefore be interpreted as descriptive evidence on the timing of the behavioral response. As an attempt to keep the windows as cleaned as possible, each regression excludes the trial period preceding the cutoff. Results are presented in Figure 9 for one-week bandwidth. For reporting, I detect some negative significant effects for few placebo cutoff dates, showing that there were some small and short-lived changes in victims' behavior during the trial. By contrast, the increase in IPV incidence is estimated with much more noise, with coefficients imprecisely estimated with no consistent sign and no significant effect detected. This evidence suggests that the incidence response is disproportionately concentrated at moments of institutional validation and massive social response while the reporting response emerges with the onset of public reactions.

Comparing the engagement timeline in Figure 1 with the intra-trial placebo estimates in Figure 9 reveals a striking correspondence: the trial dates at which significant effects on reporting emerge coincide with peaks in online engagement. The reporting decline becomes statistically significant during the second and final weeks of hearings, precisely the periods that registered the sharpest surges in viewership and social media mentions, although these intra-trial responses are smaller and short-lived, and the discontinuity at the verdict remains the dominant one. This co-movement provides direct evidence that the treatment operates through media exposure rather than through a diffuse "trial awareness" effect: it is not the mere passage of time during the proceedings that suppresses reporting, but the intensity of public engagement with the trial at specific moments. The pattern is also consistent with the model's mechanism: each engagement peak corresponds to a moment in which a large audience simultaneously observed the public discrediting of a victim, triggering a downward revision in beliefs about the probability of being recognized as a victim.

7.4 Suggestive Evidence from 911 Calls in Seattle

[19] All the trial transcripts can be found at *Reporting Depp vs Heard* website. Accessed at: https://reportingdeppvheard.net/depp-v-heard-2022/transcripts/.

[20] The trial lasted from the 11th of April to the first of June 2022. The week from the 9th to the 15 of May, the court was completely inactive. On a weekly basis, the court was active Monday through Thursday, with the exception of the day of the closing argument, May 27, which was a Friday.

The interpretation of the NIBRS results is strengthened by the analysis of the 911 Calls for Services in Seattle presented in Figure 10 (and Appendix Table F4). These estimates rest on narrow samples by construction (60 to 120 observations depending on the window). I therefore read the results as informative about the direction of movement and its pattern across margins, rather than as precise magnitudes, and do not attach a causal interpretation to any single coefficient.

Panels (a) and (b) isolate calls that arrive already labelled as DV at intake which proxy for reporting. Panel (b) shows a decline in reported less severe DV of 28 to 42 percent, in accordance with the NIBRS results. Panels (c) and (d) show results for calls that enter the system as disturbance and are recorded as DV only once an officer has attended. By construction the caller in these cases is not reporting DV, and the channel is predominantly third-party reports. Panel (c) shows that the verdict is followed by more than a doubling of third-party calls to 911 for severe DV.

Three mechanisms could generate a rise in third-party calls resolving as severe DV. The first is a general rise in public attentiveness following the trial, that would raise the flow of generic disturbance calls as a whole and not only the fraction of them that turns out to involve DV. Appendix Figure F3, panel (a), shows that it does not. Heightened attentiveness would also be expected to increase the less severe tier first, since low-severity incidents are those whose reporting is most discretionary, but estimates show an opposite pattern. A second hypothesis is a change in reporting by victims, with third-party calls substituting for direct ones. The data do not support it: the reporting channel for severe DV in Panel (a) does not fall at any bandwidth. None of these effects is explained by a variation in the volume of 911 calls in general. Appendix Figure F3, panel (b) reports the same exercise on the volume of non-DV calls showing small non-significant coefficients at any bandwidth.

8. Mechanisms

8.1 Gender Norms

Prevailing gender norms plausibly move both $\overline{\beta}$ and b. Structural bias $\overline{\beta}$ is more severe where women are granted less credibility before any evidence is weighed. The benefit of reporting b is lower where institutions and communities are less responsive to victims. A lower b raises the reporting threshold $\frac{\sigma_i}{b+\sigma_i}$, so the victims who still report are concentrated just above it and a given revision in beliefs removes a larger share of them. A more severe $\overline{\beta}$ acts on both margins. It lowers β_i and with it μ_i, shifting the distribution of beliefs down relative to the threshold, so more matches sit close to it. It also widens the revision itself: from Appendix A.1, $\mu_i - \mu_i(1) = e_i(1-\beta_i)[\pi_i - \pi_i(1)]$, which is increasing in $1-\beta_i$. By Prediction 2, the perpetrators who become violent are those with $v_i \in [\mu_i(1), \mu_i)$, so a wider revision converts a larger set of them, in matches that continue to report and

without any victim changing her decision. By *Prediction 3* a further group is freed by the victims who do fall silent. Both margins should therefore be strongest where stated norms are least egalitarian.

I measure stated norms with the Gender Progressivity Index of Kleven (forthcoming), built from General Social Survey responses to three questions on gender roles over the 2010–2020 decade, so that it is predetermined with respect to the verdict; higher values denote more egalitarian norms. Results are reported in Figure 11 and Appendix Table F5. Below the median of the index, reporting falls by 15.0 percent and incidence rises by 57.6 percent in the week around the verdict; above it, neither coefficient is distinguishable from zero. The two margins move together, as the first implication of *Prediction 3* requires, and they move where the model locates them.

8.2 Levels of Lethal Violence

The second split uses the prevalence of lethal violence against women. The rate is not a measure of norms: it is an outcome, and it summarizes where the deterrent facing perpetrators has already failed. A high rate can arise from entrenched traditional norms, but also from environments in which women's autonomy has grown enough to destabilize male authority without yet being established as the prevailing norm, a configuration the literature associates with elevated rather than reduced violence (Schuler et al., 2018; Williams et al., 2024; Gómez-Casillas et al., 2023). A median split on the rate therefore pools states whose violence is generated by different mechanisms, which is why the split along stated norms is the primary test. The data are consistent with this: of the 48 states for which the homicide rate is available, 30 are classified consistently by the two splits (below the median of the index and above the median of the rate, or the reverse) against the 24 that independent classifications would produce by chance (Figure F4).

Since the rate carries little of the variation in stated norms, in the terms of the model it is a split on b and on G, the distribution of perpetrator gains, the one feature of the environment that the model takes as given and never restricts across environments. The model therefore makes opposite predictions for the two margins. Reporting should respond more strongly: where violence more often ends in death the protection obtained by reporting is smaller, which lowers b and raises the threshold. Incidence should respond less strongly, since violence is deterred only for perpetrators whose private gain falls short of the expected cost, and where lethal violence is already common that group is small, so the same revision converts a smaller share of an already large violent population.

Results are reported in Figure 12 and Appendix Table F6. The two margins behave differently. At the preferred bandwidth the reporting response is of similar size in the two groups, 11.7 percent where lethal violence is high and 13.5 percent where it is low, although the profiles differ: the estimate

is stable across bandwidths in the first group and confined to the narrowest windows in the second. The incidence response reaches 75.9 percent where lethal violence is high, significant at the five percent level and positive through ten days, while where it is low the estimate is indistinguishable from zero at every window.

None of the model's channels accounts for the concentration of the incidence response. The cost channel predicts the opposite, as set out above. The direct channel cannot either: the perpetrators converted without any change in reporting are those with $v_i \in [\mu_i(1), \mu_i)$, and that interval widens only where the credibility discount is more severe, which this split does not track. Nor can the channel running through reporting: the violence it generates is the mass freed by matches that fall silent (Appendix equation A6), and the observed reporting decline is of the same size in the two groups. The most plausible candidate is an emboldening effect. Where men already hold strong priors that women's allegations lack credibility, a court publicly disbelieving a victim despite substantial evidence does more than revise a probability: it validates a belief, and acting on a validated belief is cheaper than acting on a private one. Identity economics gives this a formal footing, with agents deriving utility from actions that affirm a social identity (Akerlof and Kranton, 2000, 2010), as does the political economy of hatred, where public narratives confirming suspicions about an out-group lower the cost of acting on them (Glaeser, 2005); the literature on precarious manhood points the same way, treating masculinity as a status requiring public validation and aggression as one form its expression can take (Vandello and Bosson, 2013; Bosson et al., 2009). This mechanism lies outside the model, which contains expected costs and nothing else, and I offer it as an interpretation of the residual rather than as a tested prediction.

9. Discussion and Conclusion

This paper develops the first economic model of SV of victims of VAW and provides causal evidence that public exposure to a highly informative SV event reshapes the behavior of both victims and perpetrators. The credibility-updating channel is activated more often for VAW victims than for the victims of any other crime, because recognition in these cases is not granted in proportion to the evidence presented. When agents observe an episode in which a victim's evidence is met with institutional skepticism and public ridicule, they revise downward their beliefs about the probability that future victims will be recognized as victims. This revision simultaneously raises the expected cost of reporting for victims and lowers the expected cost of violence for perpetrators, generating two complementary behavioral responses.

Exploiting the 2022 Depp v. Heard defamation trial as a source of quasi-random variation in exposure to SV, I find that IPV reporting declined by approximately 12 percent in the days following

the verdict, while severe IPV incidence increased by approximately 34 percent, although imprecisely estimated. Both effects are concentrated in more conservative environments, as the model predicts, and the incidence response is further concentrated where lethal violence against women is already high, a pattern none of the model's channels accounts for, and which I read as evidence of an emboldening mechanism operating through the verdict's validation of pre-existing skepticism toward victims. The timing of effects reveals that the response of both victims and perpetrators is driven by the verdict. Nonetheless, the public response to the trial proceeding induced some transitory and more modest changes in victims' behavior coinciding with the peaks in online social media engagement with the trial.

Empirically, this paper establishes SV as a determinant of violence in its own right rather than a private harm. One episode, in which none of the women in the sample took part, cost roughly a thousand reports in a single week and loosened the deterrent facing their perpetrators. What the literature has measured as damage to the woman who reports turns out to shape the behavior of everyone who watches.

The theoretical framework makes several contributions. First, it models SV, derives testable predictions for the reporting and incidence margins, and accounts for why SV falls disproportionately on victims of gender-based violence: any victim can go unrecognized for want of evidence, but only here is credibility discounted rather than granted in proportion to what she can show, so recognition fails even when the evidence is strong. Second, the model formalizes two features of SV that the literature has only described so far. Severity is the number of people and institutions a report reaches, so it rises with the actors a case draws in; and an episode is the more informative the more evidence it disregards, so a victim disbelieved despite a strong case moves beliefs further than one with little to show. Third, it offers a sequential structure in which the bargaining and backlash hypotheses coexist rather than compete: bargaining power is protective before reporting, through the victim's outside option, while the credibility penalty that accompanies agency makes it harmful after. The most empowered women are therefore better able to leave a violent relationship but less able to be granted the status of victim.

Several limitations should be noted. The identification strategy captures short-run effects in the immediate aftermath of the verdict; whether these behavioral shifts persist or attenuate over time remains an open question. Incidence is measured with error as the majority of IPV incidents are still not reported to the authorities; the proxy used isolates rarer events so that the effect is imprecisely estimated for the whole sample. Severity is taken as given, though a victim could in principle limit how far her report travels; modelling that choice would require making the benefit of reporting a function of it too, since both rest on the same actors. The restriction is conservative: a less visible

report still enters the data as a report. Finally, the model treats the signal as common and homogeneous; in practice, exposure intensity varies across individuals and platforms.

The policy implications of these findings extend well beyond high-profile media events. The Depp–Heard trial provides a uniquely clean empirical setting but the mechanism it reveals operates at every scale. The credibility-updating channel is activated whenever a woman's report of violence is questioned: in a courtroom, a police station, a workplace, or a family. Each of these episodes constitutes an SV signal that, however local, updates beliefs about the cost of reporting and the cost of perpetrating. The results of this paper suggest that such everyday encounters with secondary victimization -- a dismissive officer, a disbelieving relative, a hostile online comment -- may cumulatively produce the same chilling and emboldening effects documented here, precisely because the underlying mechanism is the same. If anything, the aggregate impact of routine, small-scale SV may exceed that of rare, high-profile events, because the former is pervasive and the latter exceptional. Policies aimed at reducing violence against women must therefore address not only the institutional response to reported violence, but the entire discourse surrounding violence against women. Moreover, the results underscore that the institutional treatment of individual cases has aggregate consequences: when a court publicly discredits a victim, it does not merely resolve a private dispute but shifts the equilibrium behavior of victims and perpetrators across the population.

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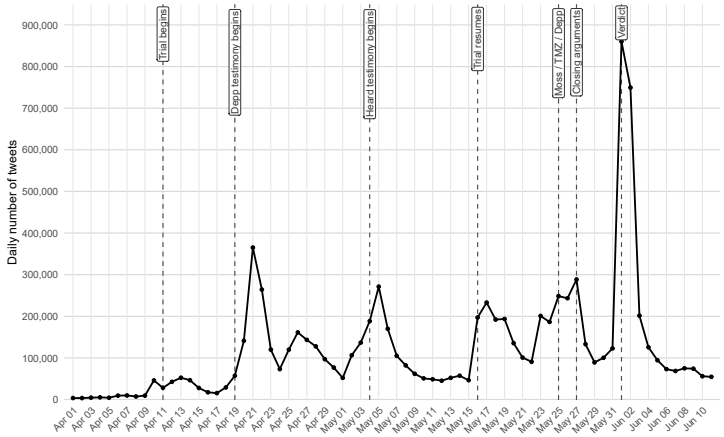
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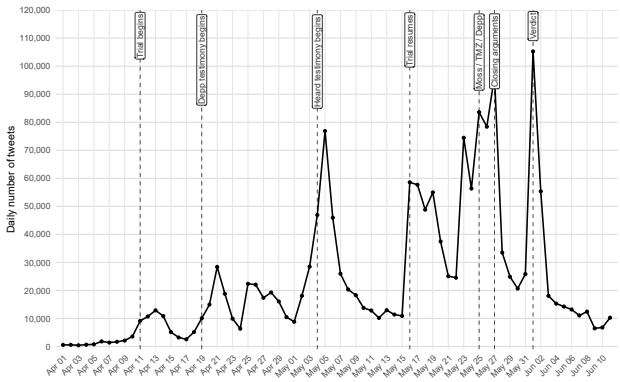
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Figure 1. Tweet Counts Timeline During the Trial

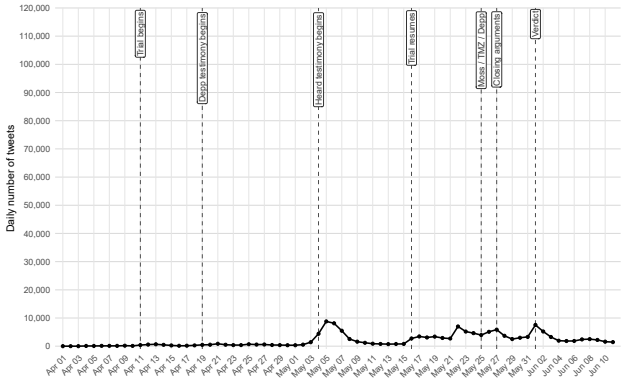
Panel (a): Tweets -- Total



Panel (b): Tweets – Johnny Depp Sided



Panel (c): Tweets – Amber Heard Sided

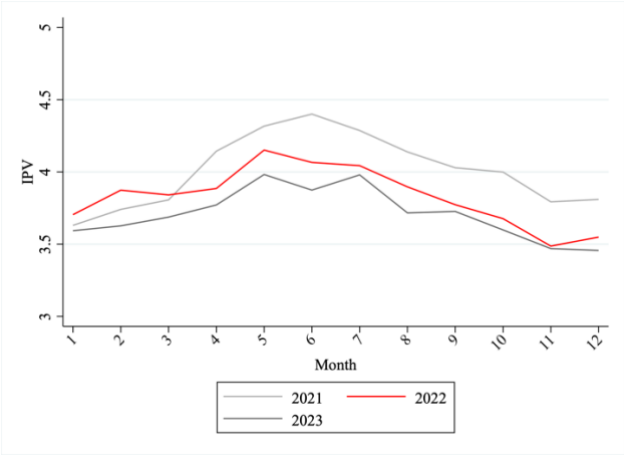


Notes: The figure plots the daily number of English-language tweets containing case-related keywords and selected hashtags related to the Depp v. Heard trial. The sample covers April 1 through June 11, 2022, inclusive. Retweets are included in the counts. Dashed grey lines indicate some of the key events during the trial. The light grey shaded area indicates the scheduled trial break from May 9 to May 15, 2022. Panel (a) includes tweets containing hashtags related to the trial, that are both neutral or in support of one of the two parties; Panel (b) includes tweets containing hashtags expressing support for Johnny Depp; Panel (c) includes tweets containing hashtags expressing support for Amber Heard.

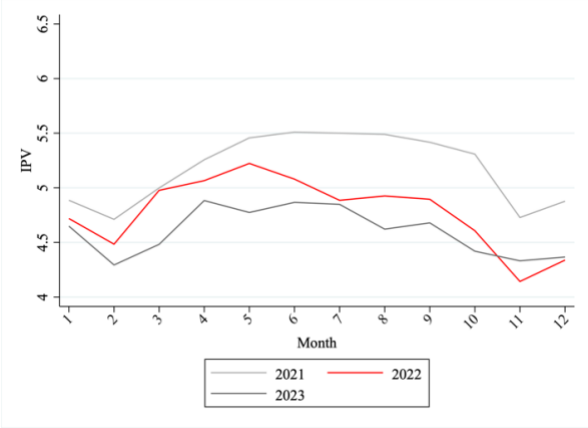
Figure 2. Time Trends in IPV, NIBRS, 2021, 2022 and 2023

IPV Reporting

Panel (a): Week Days

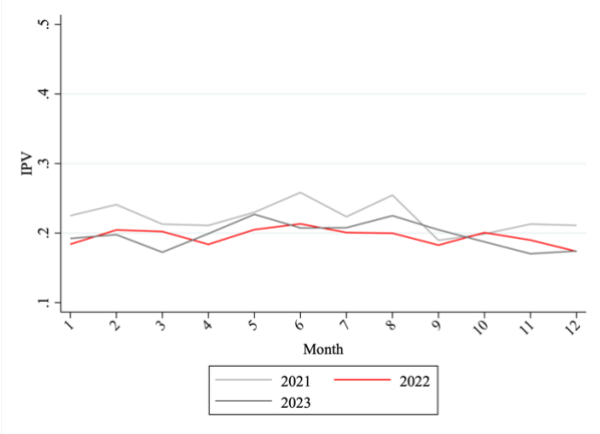


Panel (b): Weekends

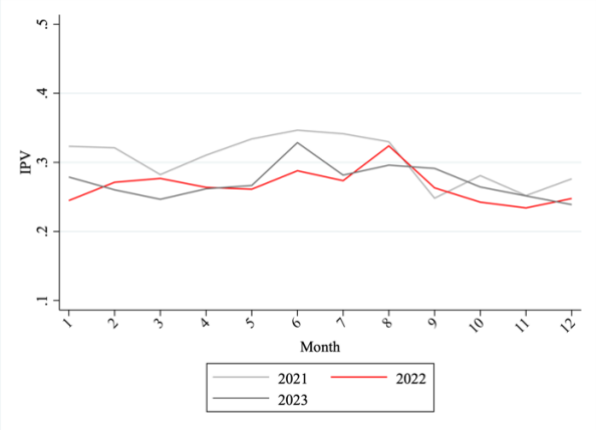


IPV Incidence

Panel (c): Week Days



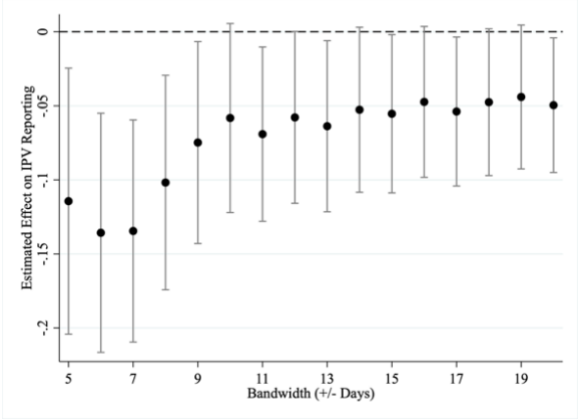
Panel (d): Weekends



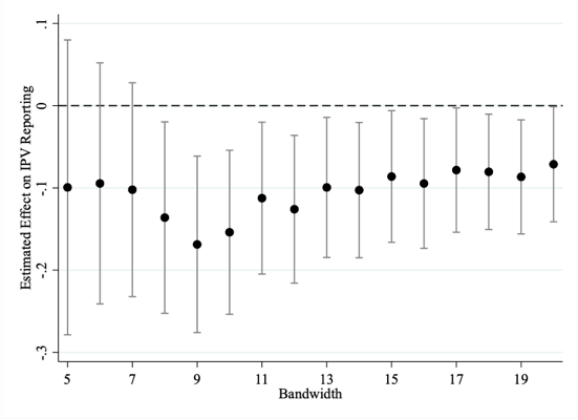
Note: Monthly weighted average IPV reported cases across years. Data are from the National Incident-Based Reporting System from 2021 to 2023. Police agencies that cover a population lower or equal to 50,000 individuals are dropped from the sample.

Figure 3. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence by Bandwidth Sizes, NIBRS 2021-2023

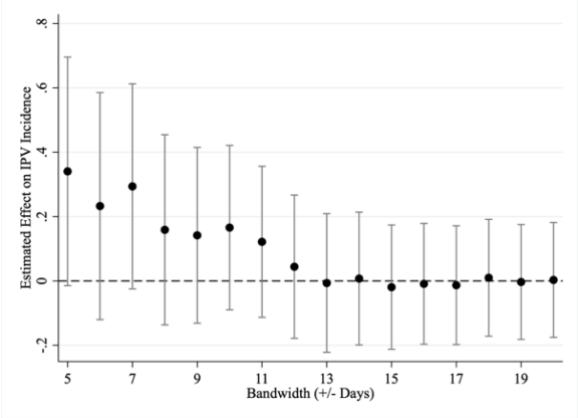
Panel (a): IPV Reporting



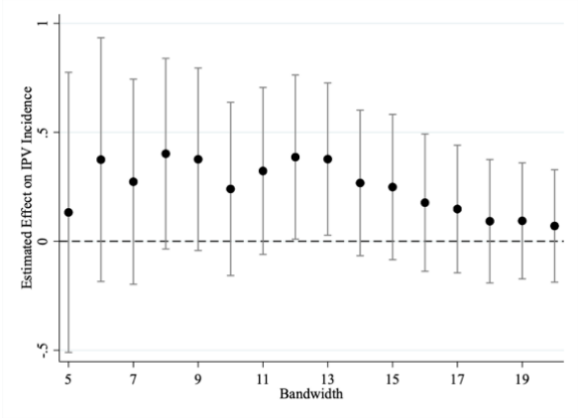
Panel (b): IPV Reporting, Quadratic Model



Panel (c): IPV Incidence



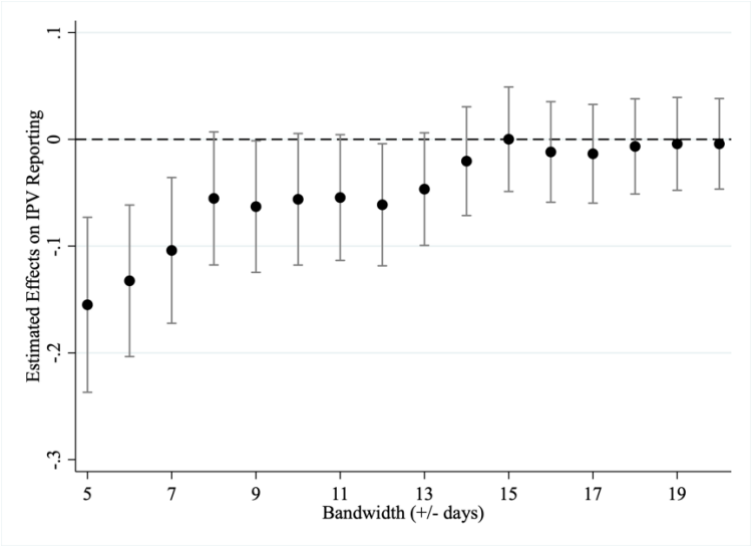
Panel (d): IPV Incidence, Quadratic Model



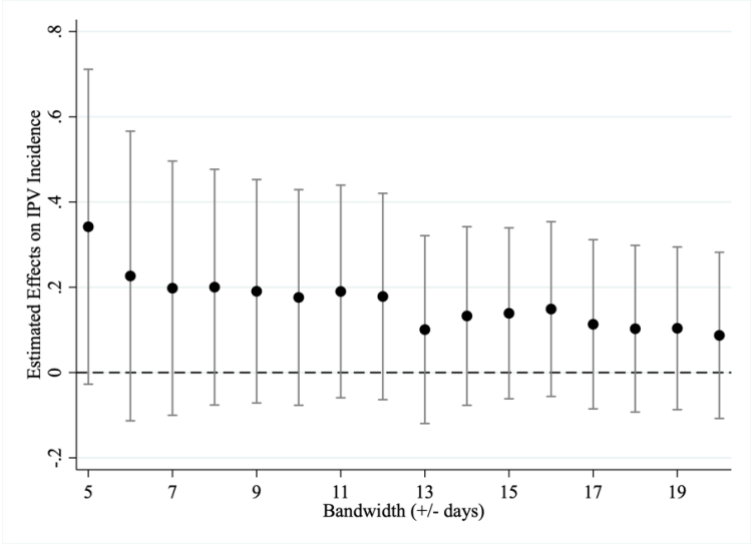
Note: Estimated effect of the trial on IPV across bandwidths using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

Figure 4. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence by Bandwidth Sizes. Remove Weeks of Trial, NIBRS 2021-2023

Panel (a): IPV Reporting



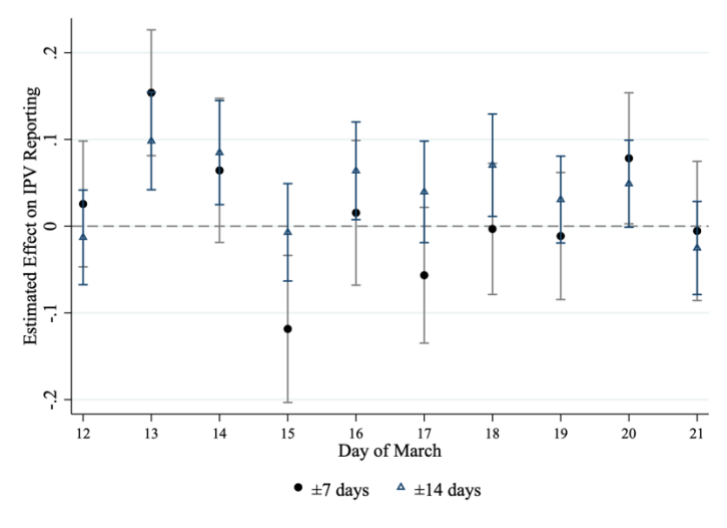
Panel (b): IPV Incidence



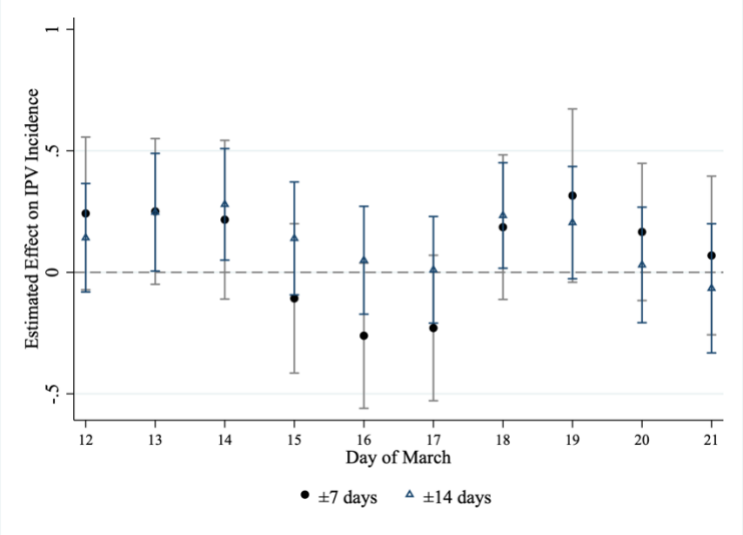
Note: Estimated effect of the trial on IPV across bandwidths using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

Figure 5. Test for Anticipatory Effects, NIBRS 2021-2023

Panel (a): IPV Reporting



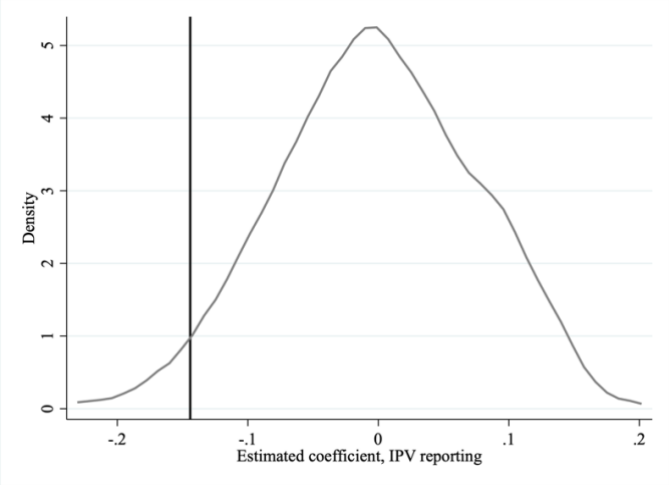
Panel (b): IPV Incidence



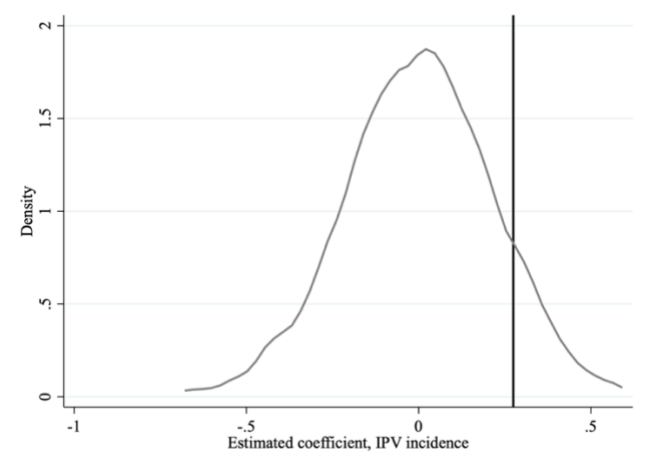
Note: Estimated effect of the pre-trial dates on IPV using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

Figure 6. Permutation Test, NIBRS 2021-2023

Panel (a): IPV Reporting



Panel (b): IPV Incidence

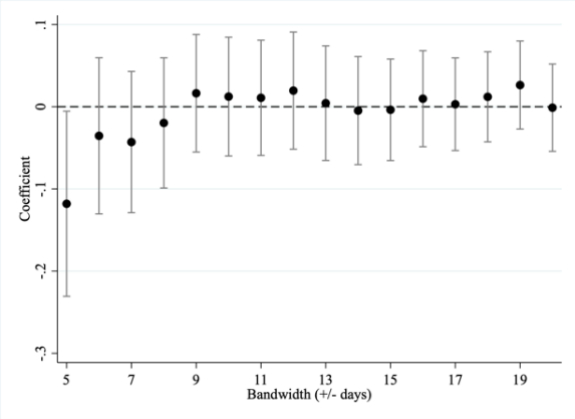


Note: Data are from the National Incident-Based Reporting System from 2021 to 2023. Police agencies that cover a population lower or equal to 50,000 individuals are dropped from the sample. The estimation uses a 7-day bandwidth. The vertical line represented the coefficient estimated in the main specification.

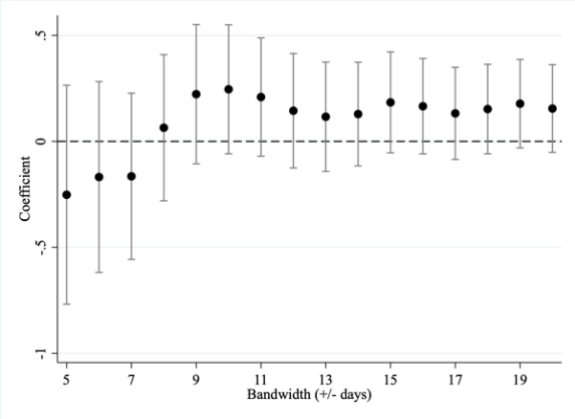
Figure 7. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence by Bandwidth Sizes, NIBRS 2021 & 2023. Placebo year

Treated Year = 2021

Panel (a): IPV Reporting

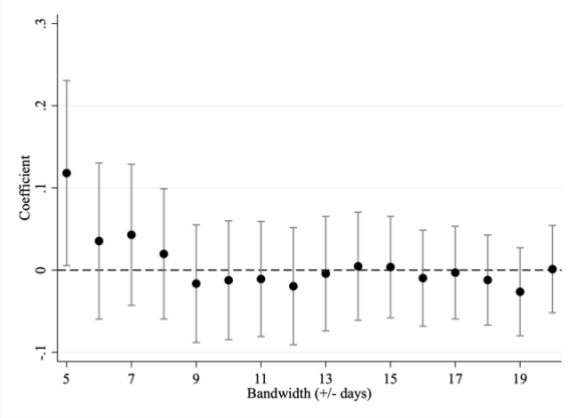


Panel (b): IPV Incidence

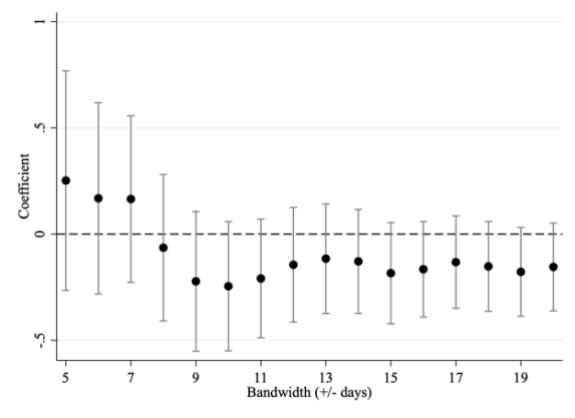


Treated Year = 2023

Panel (c): IPV Reporting



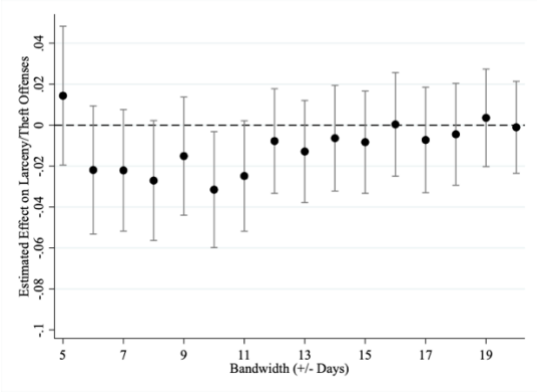
Panel (d): IPV Incidence



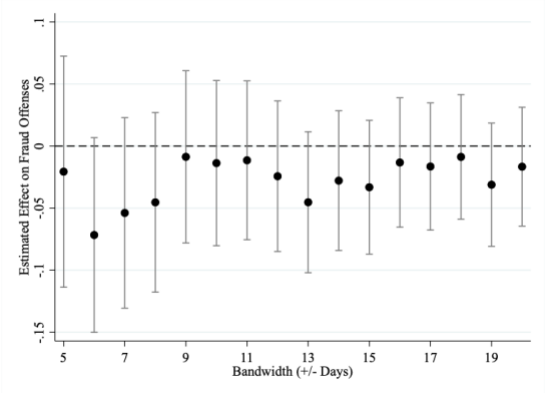
Note: Estimated effect of 1st of June on IPV in control years using a RD-DD model and data from the National Incident-Based Reporting System 2021 and 2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level

Figure 8. Effect on Property Crimes, NIBRS 2021-2023

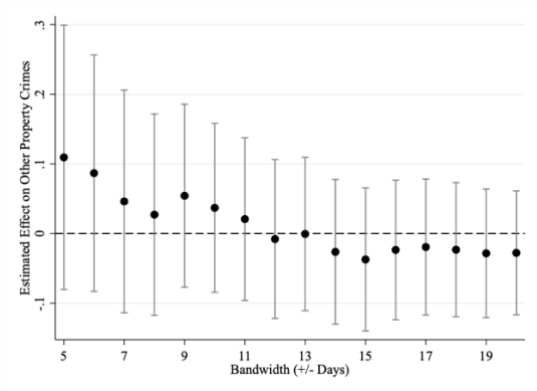
Panel (a): Larceny/Theft Offenses



Panel (b): Fraud Offenses



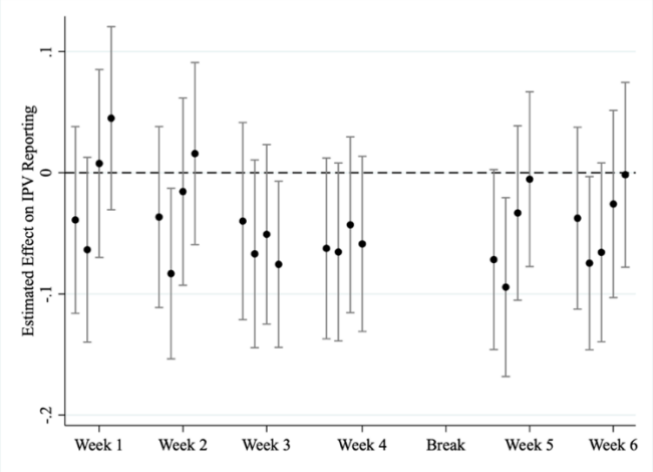
Panel (c): Other Property Crimes



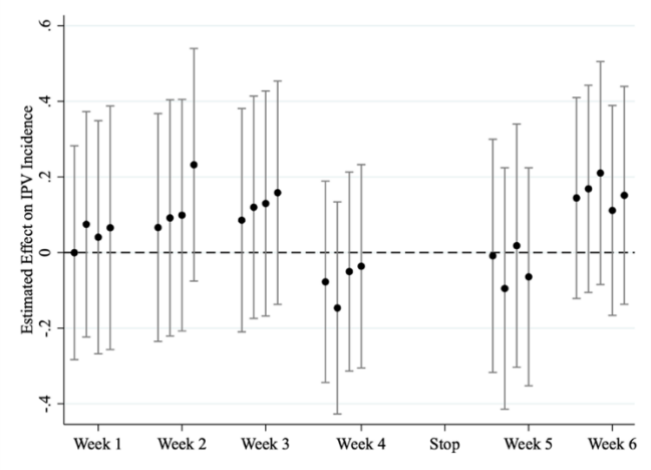
Note: Estimated effect of the trial on property crimes across bandwidths using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level. Other property crimes include arson, extortion/blackmail, embezzlement, stolen property offense, and bribery.

Figure 9. Intra-trial Placebo Cutoffs

Panel (a): IPV Reporting



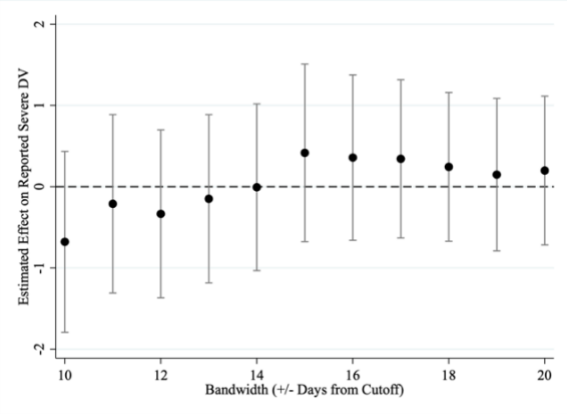
Panel (b): IPV Incidence



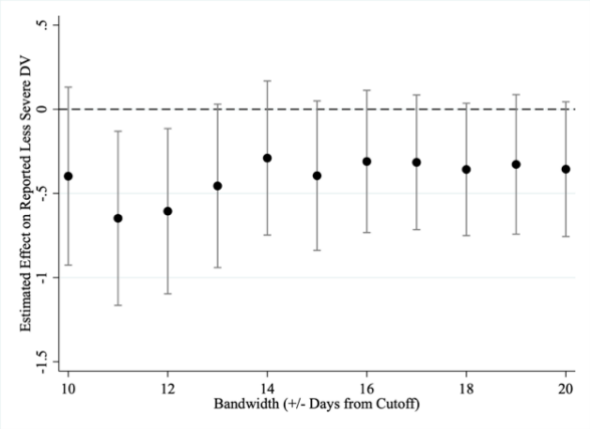
Note: Estimated effect of placebo intra-trial cutoff dates on IPV using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Coefficients are estimated over a 7-day window on both sides of the cutoff. Standard errors are clustered at the agency level.

Figure 10. RD-DD Estimates of the Impact of the Trial Decision on DV 911 Calls, Seattle. Years 2021-2023

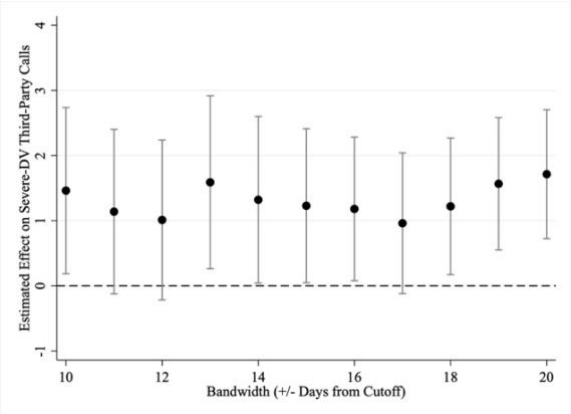
Panel (a): Reported DV – Severe



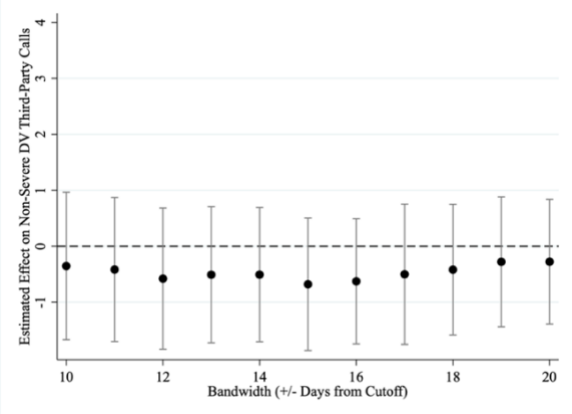
Panel (b): Reported DV– Less Severe



Panel (c): Third-Party Reported - Severe DV



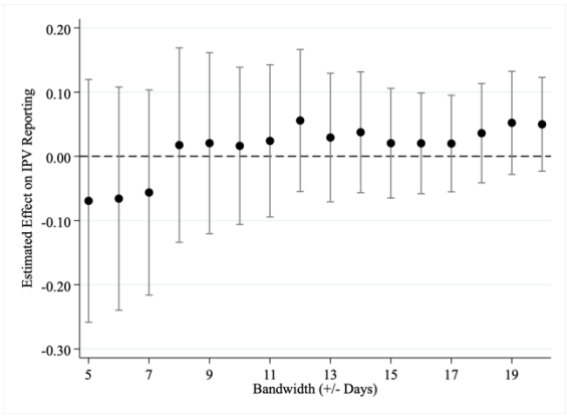
Panel (d): Third-Party Reported - Less Severe DV



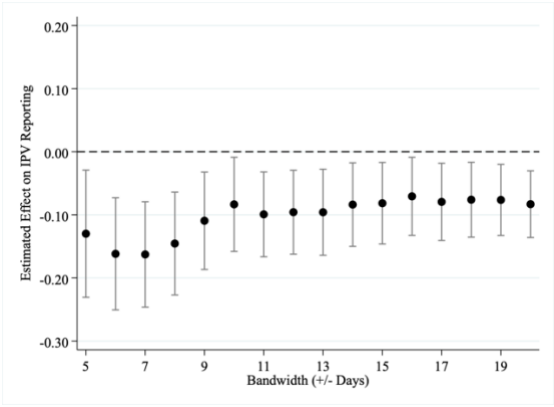
Note: Estimated effect of the trial on IPV across bandwidths using a RD-DD model and data from the Police calls for service in Seattle 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects.

Figure 11. Heterogeneous Effects by Kleven Gender Progressivity Index, NIBRS 2021-2023

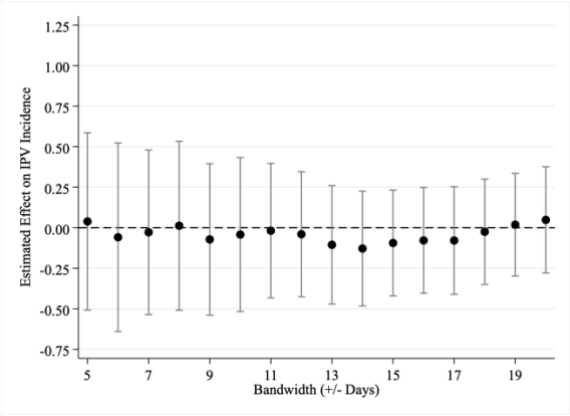
Panel (a): IPV Reporting, More Egalitarian



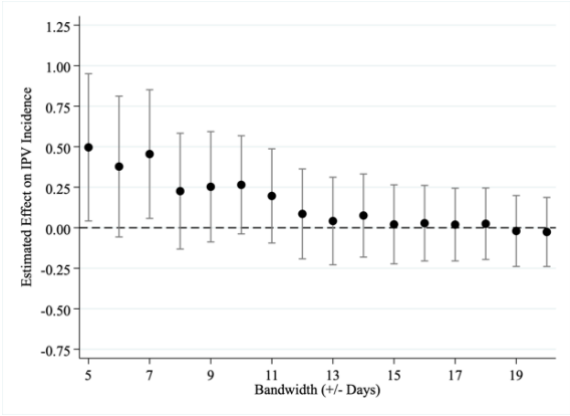
Panel (b): IPV Reporting, Less Egalitarian



Panel (c): IPV Incidence, More Egalitarian



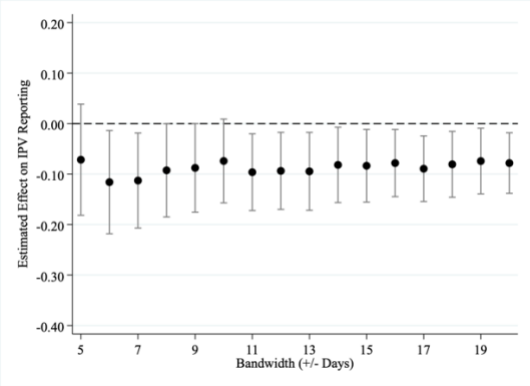
Panel (d): IPV Incidence, Less Egalitarian



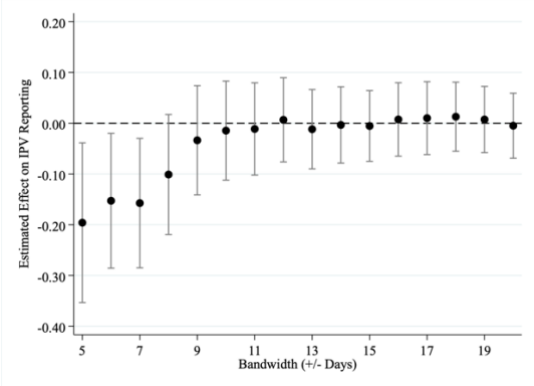
Note: Estimated effect of the trial on IPV across bandwidths using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level. The Gender Progressivity Index is from Kleven (forthcoming).

Figure 12. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence by Bandwidth Sizes, Heterogeneity by Female Homicide Rate, NIBRS 2021-2023

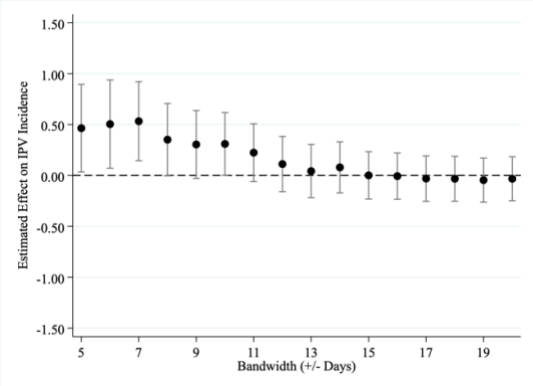
Panel (a): IPV Reporting, High Female Homicides



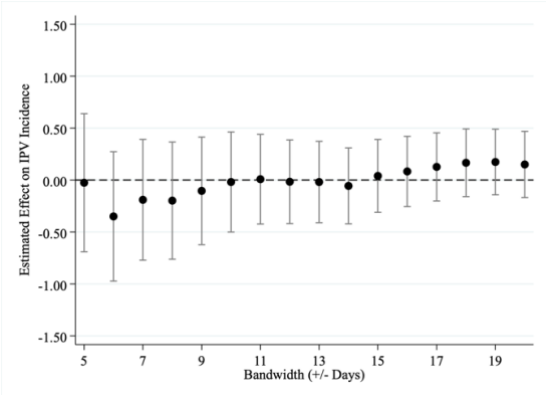
Panel (b): IPV Reporting, Low Female Homicides



Panel (c): IPV Incidence, High Female Homicides



Panel (d): IPV Incidence, Low Female Homicides



Note: Estimated effect of the trial on IPV across bandwidths using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level. Data on female homicides are from the FBI Supplementary Homicide Reports 2015—2019.

Table 1. Descriptive Statistics

	Mean	SD	Min	Max	N
Panel I: Main Sample 2021-2023					
NIBRS					
IPV Minor Injuries (Reporting)	1.219	2.839	0	82	853,005
IPV Major Injuries (Incidence)	0.068	0.316	0	11	853,005
Agency Population	148,348.9	200,054.4	50,001	2,290,746	853,005
Larceny/Theft Offenses	9.415	20.481	0	393	853,005
Fraud Offenses	1.342	2.849	0	205	853,005
Other Property Crimes	0.278	0.799	0	27	853,005
Additional variables					
Temperature	57.008	18.483	-25.4	106.3	848,625
Precipitation	0.111	0.322	0	10.64	848,625
Panel II: 911 Calls for Service, Seattle 2021--2023					
Reported Severe DV	3.03	1.79	0	10	1,095
Reported Less Severe DV	6.40	2.60	0	16	1,095
Third-party reported Severe DV	1.28	1.16	0	6	1,095
Third-party reported Less Severe DV	1.14	1.11	0	6	1,095
Disturbance	71.06	11.62	18	109	1,095
Non-DV	844.90	95.34	216	1140	1,095

Sources: National Incident-Based Reporting System; Seattle Police Department Call Data; PRISM Weather Data.

Table 2. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence, NIBRS 2021-2023

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bandwidths	-/+ 5 Days	-/+ 1 Week	-/+ 10 Days	-/+ 2 Weeks	-/+ 10 Days	-/+ 2 Weeks	-/+ 1 Week	-/+ 1 Week
Specification	Eq. (6)	Eq. (6)	Eq. (6)	Eq. (6)	Quadratic Model	Quadratic Model	Exclude Trial Period	Add 2019
					Panel (a): IPV Reporting			
Treat*Post	-0.114**	-0.135***	-0.058	-0.053	-0.154**	-0.103**	-0.104**	-0.106*
	(0.055)	(0.046)	(0.039)	(0.034)	(0.061)	(0.050)	(0.041)	(0.056)
Observations	23,160	32,466	46,380	64,932	46,380	64,932	32,382	23,884
					Panel (b): IPV Incidence			
Treat*Post	0.341	0.294	0.166	0.007	0.240	0.268	0.198	0.268
	(0.216)	(0.194)	(0.155)	(0.125)	(0.242)	(0.203)	(0.181)	(0.246)
Observations	13,050	21,042	33,660	50,904	33,660	50,904	21,672	17,990
Year & Agency FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimated effect of the trial on IPV using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

Table 3. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence by Broadband Coverage, NIBRS 2021-2023

	(1)	(2)
	IPV Reporting	**IPV Incidence**
Treat*Post if not exposed	−0.021	0.455
	(0.102)	(0.422)
Treat*Post if exposed	−0.141***	0.344
	(0.053)	(0.224)
Difference	−0.163*	−0.111
	(0.096)	(0.408)
N	25,289	15,004
FE	Yes	Yes
Covariates	Yes	Yes

Note: Estimated effect of the trial on IPV using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023 and from the FCC Broadband Data Collection. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Coefficients are estimated over a 7-day window on both sides of the cutoff. Standard errors are clustered at the agency level.

Appendix

Appendix A: Theoretical Model

A.1 Signal Technology and Belief Updating

The object of inference is the state of the environment, $\omega \in \{fair, biased\}$. A victim's true probability of recognition depends on it, $\theta_i(fair) = e_i$ and $\theta_i(biased) = \beta_i e_i$ so the prior on ω induces a two-point prior on $\theta_i(\omega)$ with masses π_i and $1 - \pi_i$. Throughout, μ_i and $\mu_i(1)$ denote the probability of recognition an agent assigns before and after the signal, obtained by averaging $\theta_i(\omega)$ over her beliefs about ω.

In $t = 1$ a publicly observed episode of SV may or may not occur. Conditional on the state, two things are random. First, whether a case arises at all: with probability $\lambda \in (0,1)$ a case becomes publicly observable, and λ is independent of the state, since whether a high-profile dispute reaches trial reflects the litigants and the court calendar rather than how the system treats victims. Second, conditional on a case arising, how it ends. The public case carries evidence e_0. In a fair environment its victim is recognized with probability e_0 and disbelieved with probability $1 - e_0$; in a biased environment she is recognized with probability $\beta_0 e_0$ and disbelieved with probability $1 - \beta_0 e_0$. Disbelief is more likely under a biased environment whenever $\beta_0 < 1$, and the signal is then informative provided $e_0 > 0$.

Three events are therefore possible. If no case arises, nothing is learned and beliefs remain at the prior, because non-arrival is equally likely in either state. If a case arises and the victim is recognized as such ($s = 0$), beliefs revise upward. If a case arises and the victim is disbelieved, the event denoted $s = 1$, beliefs revise downward. Only the last is relevant for what follows: the trial studied in the empirical analysis is a case that arose and ended in SV, while the control years contain no comparable episode. Write $\mu_i(s)$ for the belief held at the time of the t = 2 decisions, where s indexes the realization of the $t = 1$ event. When no case arises, $\mu_i(s) = \mu_i$. The analysis below concerns $s = 1$, a case that arises and ends in SV. The strength of the signal is governed by the likelihood ratio

$$L \equiv \frac{\Pr(s = 1 \mid fair)}{\Pr(s = 1 \mid biased)} = \frac{(1 - e_0)}{(1 - \beta_0\, e_0)} \leq 1 \qquad \text{(A1)}$$

where β_0 is the discount that applies to the observed case. L depends on that case alone and is therefore the same for every agent: the signal is common even though the discount is not. L is decreasing in the evidence presented in the observed case. A victim disbelieved despite conclusive evidence cannot be

dismissed as a case the system got right: as e_0 approaches one, L approaches zero, since a fair environment would almost surely have recognized her. A victim disbelieved with no evidence to offer carries no information at all: as e_0 approaches zero, L approaches one, because a fair environment would have disbelieved her too. The informativeness of an episode of SV is therefore not a free parameter but a property of the case observed. When $s = 1$, agents revise their beliefs about the environment downward. Bayes' rule gives

$$\pi_i(1) \equiv Pr(fair|\, s = 1) = \pi_i \frac{(1 - e_0\ )}{[\,\pi_i\,(1 - e_0\,) + (1 - \pi_i)(1 - \beta_0 e_0\,)]} = \frac{\pi_i\, L}{\pi_i\, L + (1 - \pi_i)} \tag{A2}$$

The arrival probability λ cancels from numerator and denominator, since it does not depend on the environment: only the outcome of the case is informative.

The posterior can be expressed as $\pi_i(1) = L\,\pi_i\ /\ [1 - (1 - L)\,\pi_i]$. Since $L < 1$ whenever the observed case carried some evidence, it follows directly from (A2) that $\pi_i(1) < \pi_i$ for interior priors: observing SV shifts beliefs about the environment downward. The revision is monotone in L: $\partial\pi_i(1)/\partial L > 0$, so episodes carrying stronger evidence, which lower L, produce sharper downward revisions.

The revision $\Delta(\pi_i) \equiv \pi_i - \pi_i(1)$ vanishes at $\pi_i = 0$ and at $\pi_i = 1$ and attains a single interior maximum at $1/(1 + \sqrt{L})$. Since $L \leq 1$ this maximum never falls below one half, and it approaches one as the episode becomes more diagnostic: a strongly informative episode moves most those who were most confident that the environment was fair. Only beliefs that are already degenerate are immune.

When $e_0 \to 1$ the signal is fully revealing and $\pi_i(1) \to 0$: agents become certain the environment is biased. Conversely, when $L \to 1$, because the observed case carried little evidence, or because β_0 is close to one, the signal is uninformative and $\pi_i(1) \to \pi_i$: beliefs do not move.

The environment is the only thing that links the public case to the victim's own: conditional on ω, her outcome is independent of the public event, both of whether a case arose and of how it ended. This rules out a shared court or community that would make the public outcome informative beyond ω, and it rules out a verdict that changes how the next victim is treated rather than how she expects to be treated. The assumption is what makes the posterior usable. Conditioning on the state and summing over it, $\mu_i(1) \equiv \Pr(recognized \mid e_i, s = 1) = \sum \omega \Pr(recognized \mid \omega, e_i, s = 1) \Pr(\omega \mid s = 1)$. The first factor still conditions on the public event, about which the sampling model says nothing. Independence removes it, $\Pr(recognized \mid \omega, e_i, s = 1) =$

$\Pr(recognized \mid \omega, e_i) = \theta_i(\omega)$, and the second factor is the posterior derived in (A2). What remains is the posterior mean of $\theta_i(\omega)$:

$$\mu_i(1) = \sum \omega\, \theta_i(\omega)\, Pr(\omega \mid s = 1) = e_i[\pi_i(1) + (1 - \pi_i(1))\beta_i] \quad \text{(A3)}$$

Since μ_i is increasing and affine in π_i for given e_i, the shape of the revision carries over from beliefs about the environment to beliefs about recognition. Comparing with the same construction under the prior, $\mu_i = e_i[\pi_i + (1 - \pi_i)\beta_i]$, gives $\mu_i - \mu_i(1) = e_i(1 - \beta_i)[\pi_i - \pi_i(1)]$, which is strictly positive whenever $\beta_i < 1,\ L < 1$ and the prior is interior: the signal lowers the belief relevant to the $t = 2$ decisions. In the fully revealing limit, when $e_0 \to 1$ and $\pi_i(1) \to 0$, her belief about her own recognition does not vanish with it but converges to $\beta_i e_i$, the probability of being recognized in a biased environment.

The fair environment is nested rather than excluded. At $\beta_i = 1$, an environment with no structural bias, we have $\mu_i = e_i$ for every π_i and $L = 1$, so the posterior equals the prior and no decision changes: a fair environment cannot produce an outcome that is diagnostic of anything. A victim may still fail to be recognized when $\beta_i = 1$, since e_i may be below one, and the SV she then suffers is no less severe, so σ_i continues to operate. What vanishes is not the harm but the share of it due to bias: relative to a biased environment, the probability of suffering it falls from $1 - \beta_i e_i$ to $1 - e_i$, and the difference $e_i(1 - \beta_i)$ is the SV attributable to bias, as distinct from the disbelief the evidence itself warrants.

The two-state specification is a special case of a continuous prior over fairness, obtained by placing the prior on $\{1, \beta_i\}$. Because the prior sits on only two points, the most pessimistic belief available is that the environment is biased. No episode, however strong the signal, can push $\mu_i(1)$ below $\beta_i e_i$. The continuous version delivers the same comparative statics, the revision is proportional to the variance of the prior and vanishes at degenerate beliefs, at the cost of a posterior that no longer admits a single likelihood ratio.

A.2 Equilibrium

The subgame perfect equilibrium of the $t = 2$ game is solved by backward induction. Throughout, write $\tau_i = \frac{\sigma_i}{b + \sigma_i}$ for the reporting threshold. In $t = 2$, the perpetrator first decides whether to exert violence; the victim then decides whether to report, conditional on violence having occurred. An equilibrium consists of (a) a strategy for each player that maximizes expected utility at every information set, given beliefs and the other player's strategy, and (b) beliefs about the environment

which are updated from π_i to $\pi_i(1)$ via Bayes' rule, with $\mu_i(s)$ following from (1), upon observing the signal in $t = 1$, and are common to both players in $t = 2$. Substituting the updated belief $\mu_i(1)$ into both conditions yields the post-signal equilibrium.

Victim's best response. Given that violence has occurred, the victim reports if and only if $\mu_i(s) \geq \tau_i$. The best response is unique except at $\mu_i(s) = \tau_i$, where the victim is indifferent. Because $F(\cdot|e_i, \beta_i, \sigma_i)$ has a density, no single value of μ_i carries positive probability within a cell; the victim is therefore indifferent only on a set of measure zero, and the best response is unique almost everywhere. Denote the equilibrium reporting decision by $R_i^*(s)$.

Perpetrator's best response. The perpetrator anticipates $R_i^*(s)$ and chooses a_i to maximize expected utility. If $R_i^*(s) = 1$, his payoff from violence is $v_i - \mu_i(s)$, and he is violent if and only if $v_i \geq \mu_i(s)$. If $R_i^*(s) = 0$ the expected cost is zero, his payoff from violence is v_i, and he is violent if and only if $v_i > 0$. Both are unique best responses except on measure-zero boundaries.

Uniqueness. Four mutually exclusive regions cover all generic parameter values:

(i) $\mu_i(s) \geq \tau_i$ and $v_i \geq \mu_i(s)$: violence occurs and is reported.

(ii) $\mu_i(s) \geq \tau_i$ and $v_i < \mu_i(s)$: no violence, since the expected cost exceeds the gain.

(iii) $\mu_i(s) < \tau_i$ and $v_i > 0$: violence occurs and is not reported.

(iv) $\mu_i(s) < \tau_i$ and $v_i \leq 0$: no violence, since the gain is not positive even with no expected cost.

For generic parameter values there exists a unique equilibrium in pure strategies.

Corollary (non-abusive men). A man with $v_i \leq 0$ derives no utility from violence. He falls in region (iv) when the victim does not report, and in region (ii) when she does, since reporting requires $\mu_i(s) \geq \tau_i > 0$ and therefore $v_i < \mu_i(s)$. In both $a_i = 0$ is optimal, strictly so whenever $v_i < 0$. Men with no taste for violence are never violent in equilibrium.

Proposition 1 (reporting threshold). In $t = 2$ a victim reports violence if and only if $\mu_i(s) \geq \tau_i$, where $\tau_i = \frac{\sigma_i}{b + \sigma_i} \in (0,1)$. The threshold is increasing in σ_i and decreasing in b: a victim whose report would reach more actors faces a higher bar, while greater institutional support lowers it. Since σ_i varies across matches, so does the threshold, and victims facing more actors are both less likely to report at any belief and closer to the margin at which a revision in beliefs silences them.

Proposition 2 (violence threshold). In $t = 2$, if the victim does not report, every perpetrator with $v_i > 0$ is violent. If the victim reports, perpetrators are violent if and only if $v_i \geq \mu_i(s)$. The signal lowers $\mu_i(1)$ relative to the prior and therefore lowers the minimum private gain required for violence.

Since $\mu_i(1) < \mu_i$, the reporting condition becomes harder to satisfy and the violence condition easier: the signal moves both margins, in opposite directions. The mechanism can be seen directly in the utility comparison across signal realizations. In the absence of the episode, the perpetrator's expected utility from violence is $v_i - \mu_i$, where μ_i reflects the prior probability of incurring some cost. After the signal $(s = 1)$, this becomes $v_i - \mu_i(1)$ with $\mu_i(1) < \mu_i$, unambiguously increasing the perpetrator's net gain from violence. Symmetrically, the victim's expected utility from reporting shifts on average from $\mu_i b - (1 - \mu_i)\sigma_i$ to $\mu_i(1)b - (1 - \mu_i(1))\sigma_i$, which is strictly lower. Both agents' incentives shift in the same direction: perpetrators toward violence, victims away from reporting.

A.3 From Individual Decisions to Observed Counts

The model delivers individual decisions but the data are aggregated counts. This appendix states the mapping between them. Let F be the distribution of μ across matches, $F(\cdot\,|e_i, \beta_i, \sigma_i)$ the distribution among those sharing a given e_i, β_i, and σ_i, and G the distribution of perpetrator gains. Write $\mu^*(e_i, \beta_i, \sigma_i)$ for the belief at which the post-signal belief equals τ_i, so that among matches sharing a given e_i, β_i and σ_i, a victim reports after the signal if and only if $\mu_i \geq \mu^*(e_i, \beta_i, \sigma_i)$. The aggregate switching mass is the average of $F(\mu^*(e_i, \beta_i, \sigma_i)|e_i, \beta_i, \sigma_i) - F(\tau_i|e_i, \beta_i, \sigma_i)$ over the joint distribution of evidence, discounts and severities. The observed reported count is the mass of matches in which violence occurs and is reported,

$$R = E[\int \{\mu \geq \tau_i\}(1 - G(\mu))dF(\mu|e_i, \beta_i, \sigma_i)] \tag{A4}$$

while the observed violence count is the mass of matches in which violence occurs, whether reported or not,

$$V = R + E[\int \{\mu < \tau_i\}\big(1 - G(0)\big)\, dF(\mu|e_i, \beta_i, \sigma_i)] \tag{A5}$$

The signal moves three groups. Let S denote the mass of matches with $\mu_i \geq \tau_i > \mu_i(1)$ in which violence occurs, $v_i \geq \mu_i$: those whose incidents were reported before the signal and are not after.

Matches with $\mu_i(1) \geq \tau_i$ remain above the reporting threshold, and among them perpetrators with $v_i \in [\mu_i(1), \mu_i)$ become violent; let A denote this mass. Matches that stop reporting free perpetrators with $v_i \in (0, \mu_i)$; let B denote this mass. Then

$$\Delta R = -S + A, \qquad \Delta V = A + B \tag{A6}$$

Two restrictions follow. First, the sign of the reported count is not determined: a downward revision removes reporters but also creates violence in matches that remain above the reporting threshold, and those incidents enter the reported count, so $\Delta R < 0$ if and only if $S > A$. The model predicts the composition of the observed change, not its sign. Second, the violence count always moves up relative to the reported count: $\Delta V - \Delta R = B + S \geq 0$. Equivalently, the gap between the two counts is violence that goes unreported, and the signal enlarges the set of matches in which violence goes unreported.

The same ambiguity affects the share of violence that is reported, R/V. When $\Delta R \leq 0$ that share necessarily falls, since its numerator weakly declines while its denominator weakly rises. When $\Delta R > 0$ it need not: in the limiting case in which no victim crosses the reporting threshold, every incident the signal generates is reported and the share rises.

The first restriction is what makes a second outcome necessary. The magnitude of A is already restricted by the model: the signal cannot make violent a man who derives no utility from violence, since by the Corollary such men are never violent in equilibrium, so A is confined to men whose private gain is positive and who were previously deterred by the expected cost. It is bounded empirically as well: since $A \geq 0$ and the estimated ΔR is negative, the gross reporting loss $S = A + |\Delta R|$ is bounded below by $|\Delta R|$. An upper bound would require an estimate of the violence response on all incidents, not only on those involving major injuries.

Proportional and Absolute Responses. The aggregate reporting response can be measured in two ways, and they behave differently. The absolute number of victims who fall silent within the set of matches sharing a given e_i, β_i and σ_i is the switching mass, $F(\mu^*(e_i, \beta_i, \sigma_i)|e_i, \beta_i, \sigma_i) - F(\tau_i|e_i, \beta_i, \sigma_i)$, where $F(\cdot\,|e_i, \beta_i, \sigma_i)$ is the distribution of beliefs in that cell. This mass is non-monotone in the reporting threshold: where τ_i is high few victims report at all and few remain to switch, while where τ_i is low most remain above the threshold after updating. Estimation is by Poisson, however, so the coefficient is a proportional change, and the relevant quantity is the switching mass relative to the mass reporting at baseline, $\frac{F(\mu^*(e_i,\beta_i,\sigma_i)|e_i,\beta_i,\sigma_i) - F(\tau_i|e_i,\beta_i,\sigma_i)}{1 - F(\tau_i|e_i,\beta_i,\sigma_i)}$, that is, the probability that a victim who was reporting before the signal falls silent after it. This is the hazard of

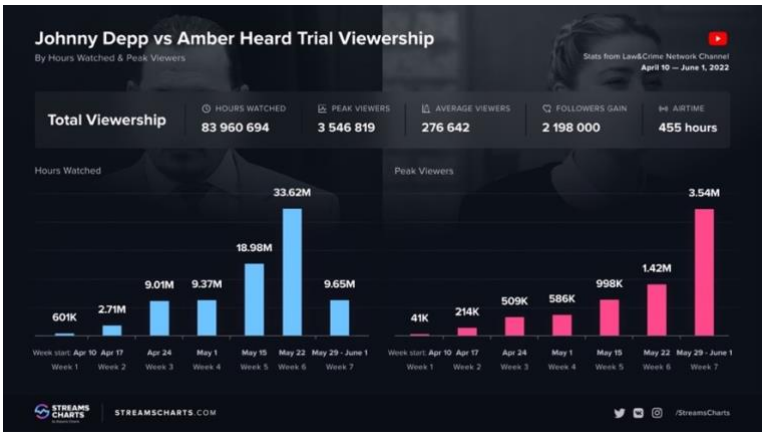
$F(\cdot\,|e_i, \beta_i, \sigma_i)$ over $\left[\tau_i, \mu^*(e_i, \beta_i, \sigma_i)\right)$. If the hazard rate $f/(1-F)$ is increasing, and provided the switching interval lies below the belief at which the revision is largest so that it does not narrow as the threshold rises, the ratio is increasing in τ_i. The aggregate response is the average of these cell-level ratios, weighted by the mass reporting at baseline in each, and inherits the same comparative static.

The same distinction applies on the perpetrator side, with the opposite sign. The proportional incidence response is the newly violent mass relative to the mass already violent. Where violence is common the denominator is large and the proportional response correspondingly smaller. A concentration of the incidence response in environments where violence is already common therefore cannot be produced by the expected cost of violence alone. An upper bound would require an estimate of the violence response on all incidents, not only on those involving major injuries. The model contains a single type of violence, whereas the data distinguish incidents by severity, so reading the major-injury count as V requires that the detection of severe incidents, which operates largely outside the victim's decision, be unaffected by the signal itself.

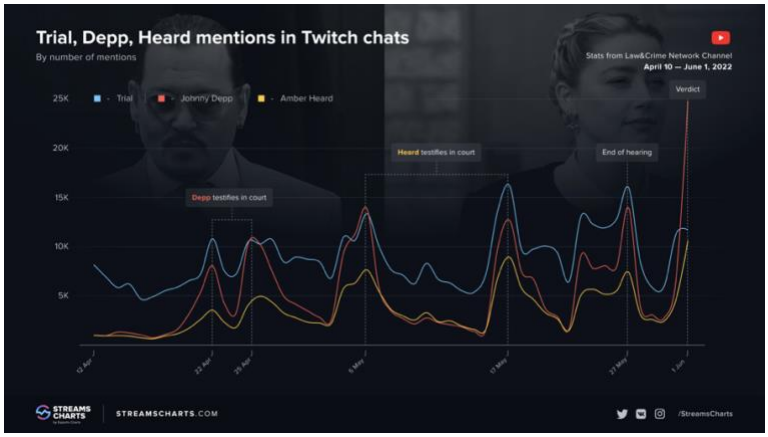
Appendix B: The Trial

Appendix Figure B1. Public Exposure to the Depp vs Heard Trail

Panel (a) Trial Viewership on Law&Crime Network



Panel (b) Trial Mentions on Twitch Chat



Note: Reprinted from Streams Charts (2022)

Appendix C. Data

C.1 NIBRS Dataset Cleaning

NIBRS is a voluntary reporting system, and the set of agencies that submit data changes continuously. Because participation is decided agency by agency the panel is unbalanced in a way that is not random: agencies join, drop out, or temporarily suspend submission, and when they resume they may do so under a different reporting protocol. A count of zero incidents in an agency-day cell is therefore ambiguous, since it can mean that no crime was reported to the police or that the agency was not transmitting to the federal system that day. Three issues arise. An agency that begins submitting in the middle of the period contributes structural zeros before its entry date, and one that stops

contributes structural zeros after its exit; an agency that is nominally active can still report anomalously little for one or more months, either because of a systems migration or because a subset of its offences stopped flowing through; and an agency whose annual volume doubles or halves between consecutive years has almost certainly changed what it transmits rather than experienced a change in underlying crime of that magnitude.

The first of these is harder to detect than it appears, because NIBRS dates each record by the date of the incident rather than of transmission: an agency going live in a certain period that uploads its still-open caseload produces records dated to the preceding spring, so the months before entry appear covered when they are only a retrospective residue, and a residue that is not reduced reporting but the absence of reporting plus a set of records tilted toward precisely the offences the design measures. The pattern follows from the 2021 transition, when the FBI retired the Summary Reporting System and only 63 per cent of agencies submitted any data for that year and 52 per cent a full year, the lowest participation since 1979 (FBI 2022a); the NIBRS specification permits first-time submitters to backfill incidents to their conversion date, which generates the retrospective dates, and formally distinguishes a zero-report from a non-submission (FBI 2022b). Excluding these agencies is standard practice rather than a choice specific to this paper: the Real-Time Crime Index applies the same two filters, monthly completeness and severe under-reporting (RTCI 2024), and the Bureau of Justice Statistics imputes partial years rather than using them (BJS 2022), an option unavailable within a daily regression discontinuity.

The dataset used in the analysis is therefore built as follows. The starting point is the full set of 14,044 agencies that submitted at least one incident of any offence type to NIBRS between 2021 and 2023. As first cleaning step, the panel is balanced by retaining only agencies that recorded at least one incident, of any offence type, in each of the 36 months of the period. This step leaves 6,893 agencies. A further 607 are dropped because the source file records no population served, and population enters the Poisson specification as the exposure variable. As explained above, this balancing rule is necessary but not sufficient: an agency that transmits a handful of long-investigation cases per month while its ordinary caseload never reaches the federal system satisfies the rule and enters the panel indistinguishable from an agency reporting in full.

Five criteria define the baseline sample. The first two concern the endpoints of an agency's reporting record. I define an agency's first active month as the earliest month in 2021---2023 in which its incident count exceeds twenty-five per cent of its own median monthly volume, and its last active month as the latest such month. An agency is excluded if its first active month falls after March 2021 or if its last active month precedes June 2023. As the empirical design uses a maximum bandwidth of twenty days around each of the two dates of interest, estimation draws on 22 March to 1 May and

on 12 May to 21 June in each of the three years. All screening criteria are therefore evaluated on that window rather than on the full period. An agency that begins reporting in February 2021 is admissible, because the first month used in estimation is March 2021; similarly, an agency that stops reporting in August 2023 is admissible, because the last month used is June 2023.

The remaining three criteria concern the interior of the record: an agency is excluded if its volume doubles or halves across years[23], if any month within the window falls below 10 percent of its own median monthly volume, or if two consecutive months within the window fall below twenty-five per cent of that median. The two latter thresholds are deliberately asymmetric. A single month is flagged only when volume falls below 10 percent of the agency's own median, because isolated low months arise routinely from seasonal and idiosyncratic variation and only an extreme drop is informative about a break in transmission. Two consecutive months are flagged at the milder 25 percent threshold, because persistence is itself evidence: ordinary fluctuation rarely produces two adjacent months that are both well below an agency's typical volume. Of the 6,286 agencies in the balanced panel, 166 do not report at a normal level in January 2021 and 122 have stopped by December 2023; 170 exhibit a doubling or halving of annual volume between consecutive years. The sample construction is described in Table C1.

An alternative sample applies the same five baseline criteria to all 36 months instead of the estimation window and it is used for the permutation-based p-value test that uses every admissible date in 2022 as placebo cutoff.

Appendix Table C1. Construction of the Estimation Sample, NIBRS 2021–2023

	Agencies	Removed
Agencies submitting at least one incident, 2021–2023	14,044	—
… reporting in all 36 months	6,893	7,151
… with non-missing population served	6,286	607
… passing the five screening criteria	6,029	257
… covering at least 50,000 residents in every year	779	5,250

Note: Each row reports the number of agencies remaining after the step described and the number removed at that step. The five screening criteria are described in the text; each is evaluated on the estimation window, 22 March to 21 June in each of the three years, and each is defined relative to the agency's own median monthly volume. The population restriction is imposed last and removes the largest number of agencies but a small share of incidents, since the agencies it excludes are small by construction. The final sample contains 853,005 agency-day observations.

[23] An agency is recorded as exhibiting a level change only if two conditions hold at once. The first is measured on annual totals: the ratio of one year's incident count to the previous year's must exceed 2 or fall below 0.5, that is, the volume must double or halve; the same ratio, computed on March-to-June totals only, must again exceed 2 or fall below 0.5.

C.2 Twitter Data

Hashtags used in the count search to retrieve Tweets related to the trial:

Neutral/case-related:

#AmberHeard
#JohnnyDepp
#DeppVsHeard
#JohnnyDeppVsAmberHeardTrial
#JohnnyDeppAmberHeardTrial
#JohnnyDeppAmberHeard
#DeppVHeard
#JohnnyDeppVsAmberHeard
#JohnnyDeppTrial
#DeppVHeardTrial
#DeppVsHeardTrial
#DeppHeardTrial

Johnny Depp support / anti-Amber:

#JusticeForJohnnyDepp
#JusticeForJohnny
#IStandWithJohnnyDepp
#JohnnyDeppIsInnocent
#AmberHeardIsALiar
#AmberHeardIsAnAbuser
#AmberHeardLsALiar
#AmberHeardLsAnAbuser
#AmberTurd
#StandWithJohnnyDepp
#IBelieveJohnnyDepp

#JohnnyDeppWon
#JohnnyWon
#TeamDepp
#JohnnyDeppGotJustice
#JusticeServedForJohnnyDepp
#AmberIsALiar
#AmberHeardIsAPsychopath

Amber Heard support / anti-Johnny:

#JusticeForAmberHeard
#IStandWithAmberHeard
#StandWithAmberHeard
#AmberHeardIsIconic
#JohnnyDeppIsAWifeBeater
#IStandWithAmber
#IBelieveAmberHeard
#AmberHeardIsInnocent
#JohnnyDeppIsAnAbuser

Appendix D. Broadband Coverage

Coverage data come from the FCC Broadband Data Collection (released 19 June 2025), reporting availability as of June 2022. I keep county-level records for residential service over the technology group "All Wired and Licensed Fixed Wireless", which covers fiber, cable, copper and licensed fixed wireless while excluding satellite, whose latency makes it a poor substitute, and unlicensed wireless, which is interference-prone.

The underlying variable, *speed_100_20*, is the share of broadband serviceable locations (addresses or structures that could receive service) for which providers report residential fixed service at 100 Mbps download and 20 Mbps upload or better. This has been the FCC's benchmark for fixed broadband since March 2024. I also construct the share of a county's serviceable locations that is rural, from the same file's location counts.

Outcomes are measured per agency while coverage is measured per county, and within a county different agencies police different territory: the county sheriff covers the unincorporated

remainder, municipal departments cover the incorporated towns. Assigning everyone the county average would credit a city department with the poor coverage of the countryside around it (in Spokane, for example, 0.916 for all six agencies instead of 0.422 for the sheriff, who polices 167,512 residents, and 0.996 for each municipal department). I therefore split each county's serviceable locations into rural and urban and compute *speed_100_20* separately on each.

Agency type is read off the ORI code, whose last four digits are 0000 for the county-level agency. That suffix is not sufficient on its own: it is also used by large city departments and by city police in county-equivalent jurisdictions, whose populations rural coverage does not describe. For each county-level agency, I compare the population it polices with the county's rural population, approximated as county population times the rural share of locations. Agencies policing more than five times that figure are treated as urban-policing and assigned the urban value; the rest keep the rural one. The rule reclassifies 58 county-level agencies, moving 30 agencies covering 9.9 million residents out of the not-exposed group.

The exposure indicator equals one when the assigned coverage is at least 0.90, that is when at least nine in ten serviceable locations in the policed territory have service at the FCC benchmark.

Appendix E. Bandwidth selection

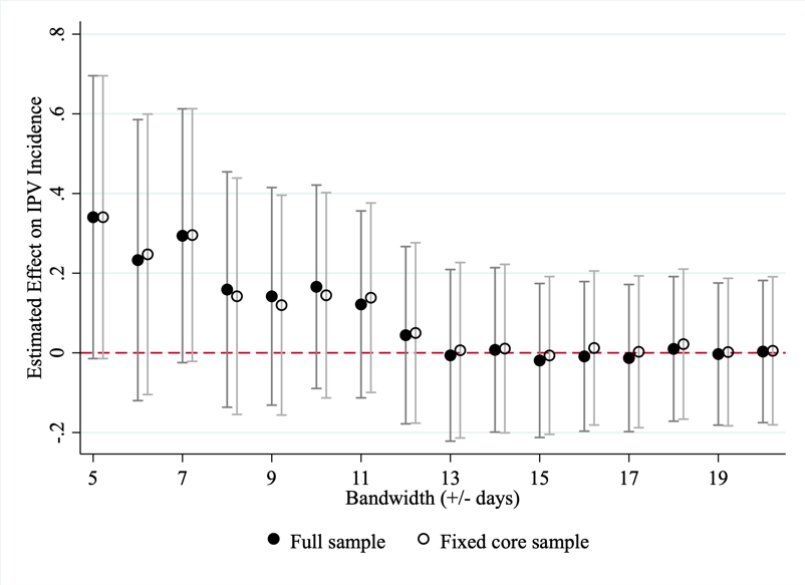
To select the optimal bandwidths range, I first estimate MSE- and CER-optimal bandwidths using the STATA command `rdbwselect`. The command cannot be used across different years, so I estimated it separately for each year. Since in this framework, I am estimating a difference in treatment effects at the cutoff across time and not just a jump in level at the cutoff in one year, this approach might be problematic, especially when focusing on the treatment year, and potentially leading to an overestimation of the optimal bandwidth. While standard RD designs rely on smoothness assumptions around the cutoff within a single cross-section, RD-DD estimates treatment effects by comparing discontinuities across time. Using bandwidth selectors like `rdbwselect` on only the treated year implicitly assumes that the functional relationship is smooth absent treatment, which leads to an underestimation of bias due to the presence of a treatment-induced jump. This in turn results in overly wide bandwidths that may incorporate non-comparable observations and undermine identification. Across the years, the optimal bandwidths are consistently ranging between 20 and 30 days on each side of the cutoff.

Furthermore, to choose an "optimal" bandwidth for this framework, some potential confounders need to be considered: (1) having time as a running variable, and (2) the potential confounding effect of events that happened in the second half of 2022. The former issue suggests that focusing on smaller bandwidths might reduce the bias (Hausman and Rapson 2018). Concerning the

latter, it is worth mentioning that less than a month after the sentence, Roe vs Wade was overruled, taking away the constitutional right to abortion in the US. Right after, many states ban abortion altogether, with potential repercussions on domestic violence victimization. Previous literature estimates that restrictions on abortion access have a sizable effect on IPV (Muratori 2025; Roberts et al. 2014). Also, the overruling might lead to important cultural shifts, in terms of decreasing gender equality, which may in turn impact violence. Additionally, in August 2022, a major tech company layoff started (CNBC 2022), leading to a shock in the level of employment. The existing literature shows robust evidence of the causal association between unemployment and IPV (Anderberg et al. 2016). In general, expanding the time window in a time RD framework increases the probability of bias, as data are added away from the cut-off (Hausman and Rapson 2018). For these reasons, I prioritize results obtained using smaller bandwidths, focusing on the effect in the first week after the trial's outcome. Anyhow, results are shown up to 20 days post-trial to give some evidence of persistence. Furthermore, the use of small bandwidths around the cutoff rules out the possibility that long-term cultural changes drive both the dependent and the independent variable.

Appendix F. Empirical Analysis

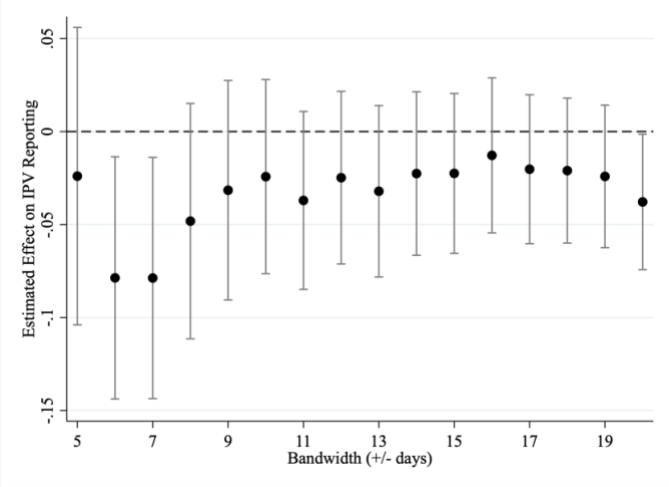
Appendix Figure F1. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence Incidence by Samples, NIBRS 2021-2023



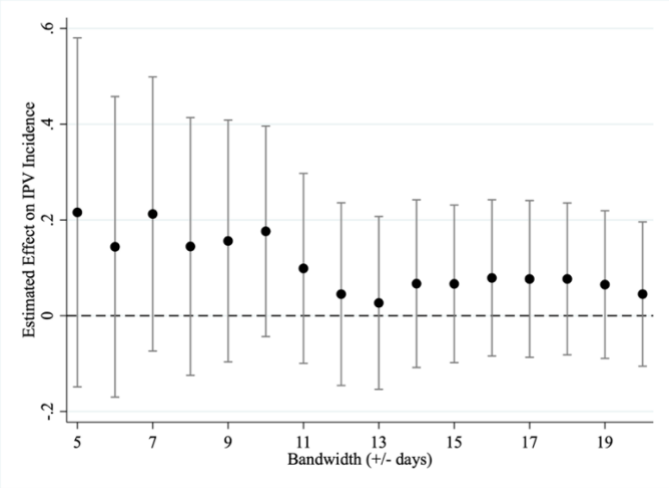
Note: Estimated effect of the trial on IPV across bandwidths using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

Appendix Figure F2. RD Estimates of the Impact of the Trial Decision on Intimate Partner Violence by Bandwidth Sizes, NIBRS 2022

Panel (a): IPV Reporting



Panel (b): IPV Incidence



Note: Estimated effect of the trial on IPV across bandwidths using a RD model and data from the National Incident-Based Reporting System 2022. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

Appendix Table F1. RD-DD Estimates Holding the Treatment Effect Constant within the Post-Verdict Window, NIBRS 2021–2023

	(1)	(2)
	IPV Reporting	IPV Incidence
Treat*Post	−0.139***	0.287
	(0.045)	(0.190)
Effect at cutoff (Table 2)	−0.135***	0.294
	(0.046)	(0.194)
Observations	32,466	21,042
Year & Agency FE	Yes	Yes
Covariates	Yes	Yes

Note: The specification is the baseline model of Table 2 excluding the interaction between the running variable, the treated-year indicator and the post-verdict indicator, so that the treatment effect is constant within the seven-day window and the coefficient corresponds to the average effect over that window rather than to the effect at the cutoff. All other terms, controls, fixed effects, exposure and clustering are as in Table 2. Standard errors clustered at the agency level in parentheses. Data are from the National Incident-Based Reporting System, 2021–2023; police agencies covering a population lower than 50,000 individuals are dropped from the sample. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table F2. Counterfactual Quantities under a Constant Within-Window Effect, Full and Common Samples, NIBRS 2021–2023

	(1)	(2)	(3)
	Reporting	Reporting	Incidence
	Full sample	*Common sample*	*Full/Common sample*
Treat*Post	−0.139***	−0.131***	0.287
	(0.045)	(0.049)	(0.190)
Effect at cutoff (Table 2)	−0.135***	−0.125**	0.294
	(0.046)	(0.049)	(0.194)
Percentage effect	−12.6	−12.3	+33.3
Observed incidents	6,929	5,988	369
Counterfactual incidents	7,960	6,828	277
Attributed to the verdict	−1,031	−840	+92
Agencies	773	500	501
Observations	32,466	21,000	21,042
Year & Agency FE	Yes	Yes	Yes
Covariates	Yes	Yes	Yes

Note: All columns report a specification identical to the baseline model of Table 2 except that the interaction between the running variable, the treated-year indicator and the post-verdict indicator is excluded, so that the treatment effect is constant within the seven-day window and the coefficient corresponds to the average effect over that window rather than to the effect at the cutoff. Columns (1) and (2) use each outcome's own estimation sample. Agencies recording no incident of the relevant type within the window do not contribute to the Poisson likelihood and are dropped, which leaves 501 of 773 agencies for major injuries; columns (3) and (4) restrict both outcomes to that common set, so that the two margins refer to the same population. Standard errors clustered at the agency level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table F3. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence, Excluding Trial Weeks, NIBRS 2021-2023

	(1)	(2)	(3)
Bandwidths	-/+ 5 Days	-/+ 1 Week	-/+ 10 Days
		Panel (a): IPV Reporting	
Treat*Post	-0.155***	-0.104**	-0.0561
	(0.0498)	(0.0415)	(0.0375)
Observations	23,070	32,382	46,320
		Panel (b): IPV Incidence	
Treat*Post	0.342	0.198	0.176
	(0.225)	(0.181)	(0.154)
Observations	12,990	21,672	34,260

Note: Estimated effect of the trial on IPV using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. The sample exclude the weeks of the trial (April 11 to May 31). Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

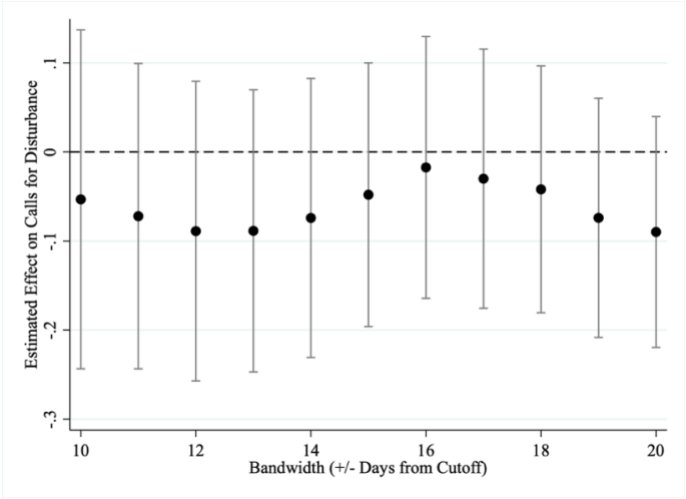
Appendix Table F4. RD-DD Estimates of the Impact of the Trial Decision on DV 911 Calls, Seattle Years 2021—2023

	(1)	(2)	(3)
	-/+ 10 Days	-/+ 15 Days	-/+ 20 Days
		Panel (a): Reported DV -- Severe	
Treat*Post	-0.679	0.416	0.198
	(0.677)	(0.664)	(0.557)
		Panel (b): Reported DV - Less Severe	
Treat*Post	-0.398	-0.395	-0.356
	(0.321)	(0.270)	(0.243)
		Panel (c): Third-Party Reported Severe DV	
Treat*Post	1.462*	1.229*	1.714***
	(0.776)	(0.719)	(0.601)
		Panel (d): Third-Party Reported Less Severe DV	
Treat*Post	-0.353	-0.680	-0.276
	(0.801)	(0.719)	(0.677)
Observations (days)	60	90	120

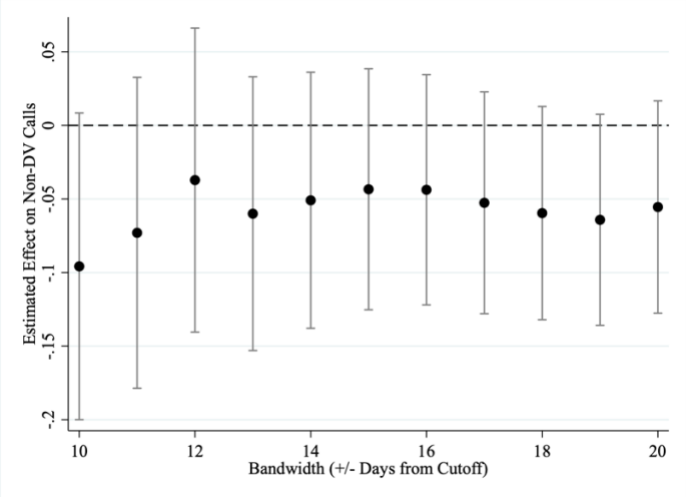
Note: Estimated effect of the trial on IPV using a RD-DD model and data from the Police calls for service in Seattle 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level.

Appendix Figure F3. RD-DD Estimates of the Impact of the Trial Decision on DV 911 Calls, Seattle Years 2021-2023

Panel (a): 911 Calls for Disturbance



Panel (b): 911 Calls – No DV



Note: Estimated effect of the trial on 911 calls across bandwidths using a RD-DD model and data from the Police calls for service in Seattle 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects.

Appendix Table F5. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence, Heterogeneity by Kleven Gender Progressivity Index, NIBRS 2021-2023

	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidths	-/+ 5 Days	-/+ 1 Week	-/+ 10 Days	-/+ 5 Days	-/+ 1 Week	-/+ 10 Days
	More Egalitarian			**Less Egalitarian**		
			Panel (a): IPV Reporting			
Treat*Post	-0.069	-0.056	0.016	-0.130**	-0.163***	-0.083*
	(0.115)	(0.097)	(0.074)	(0.061)	(0.051)	(0.045)
Observations	7,710	10,836	15,480	15,450	21,630	30,900
			Panel (b): IPV Incidence			
Treat*Post	0.039	-0.028	-0.042	0.496*	0.455*	0.265
	(0.332)	(0.309)	(0.289)	(0.276)	(0.241)	(0.184)
Observations	4,650	7,266	10,920	8,400	13,776	22,740
Year & Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimated effect of the trial on IPV using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level. The Gender Progressivity Index is from Kleven (forthcoming).

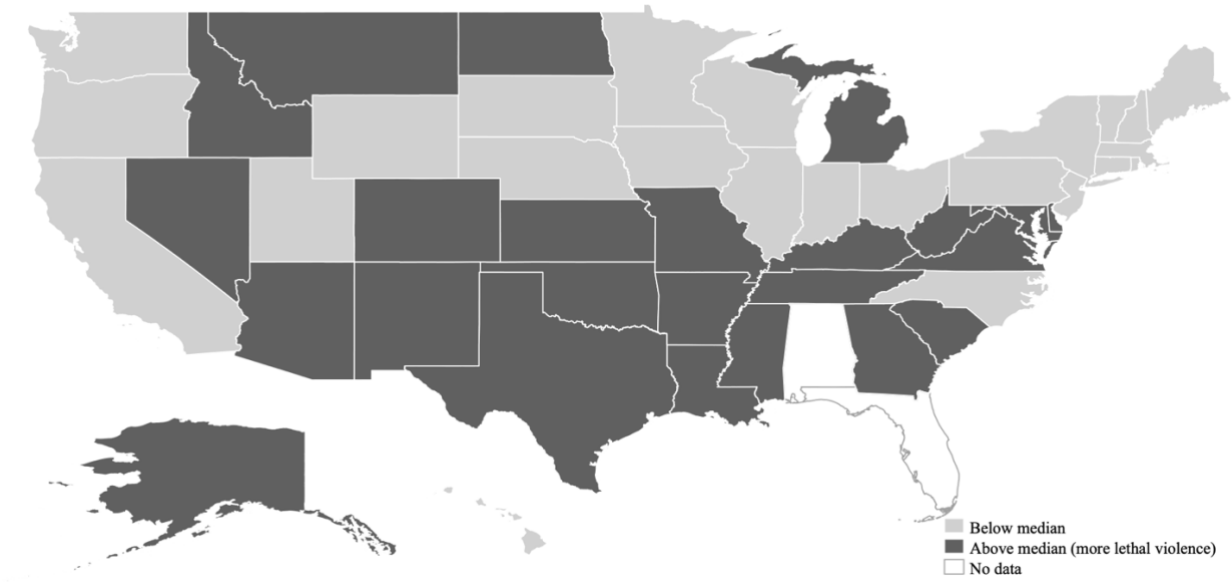
Appendix Table F6. RD-DD Estimates of the Impact of the Trial Decision on Intimate Partner Violence, Heterogeneity by Female Homicides, NIBRS 2021-2023

	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidths	-/+ 5 Days	-/+ 1 Week	-/+ 10 Days	-/+ 5 Days	-/+ 1 Week	-/+ 10 Days
	High Female Homicides			**Low Female Homicides**		
			Panel (a): IPV Reporting			
Treat*Post	-0.089	-0.124**	-0.074	-0.174*	-0.145*	-0.017
	(0.066)	(0.055)	(0.048)	(0.098)	(0.084)	(0.070)
Observations	11,970	16,800	24,000	10,770	15,078	21,540
			Panel (b): IPV Incidence			
Treat*Post	0.407	0.565**	0.334*	0.133	-0.194	-0.052
	(0.275)	(0.248)	(0.190)	(0.361)	(0.322)	(0.286)
Observations	7,560	11,844	18,780	5,250	8,694	14,100
Year & Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

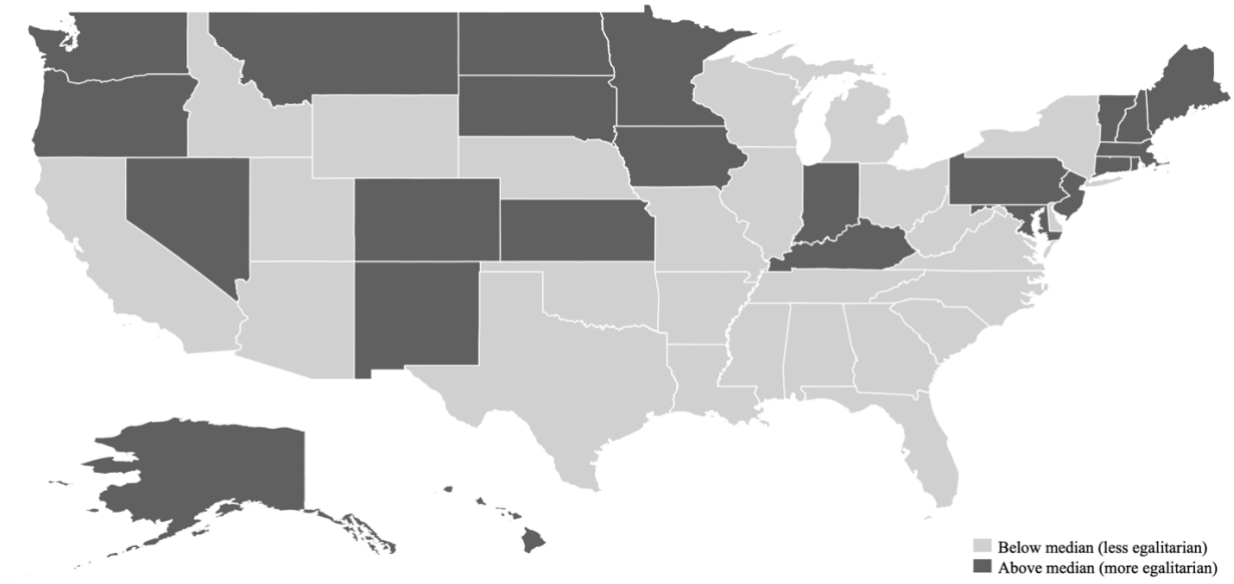
Note: Estimated effect of the trial on IPV using a RD-DD model and data from the National Incident-Based Reporting System 2021--2023. Each regression controls for daily average temperature, daily average precipitation, vacation days dummies, agency and year fixed effects. Police agencies that cover a population lower than 50,000 individuals are dropped from the sample. Standard errors are clustered at the agency level. Data on female homicides are from the FBI Supplementary Homicide Reports 2015—2019.

Appendix Figure F4. Heterogeneities across US States

Panel (a): Female Homicides



Panel (b): Gender Progressivity Index



Note: The Gender Progressivity Index is from Kleven (forthcoming). Data on female homicides are from the FBI Supplementary Homicide Reports 2015—2019.