

Deterrence or Backlash? Arrests and the Dynamics of Domestic Violence

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Abstract

How should police officers respond to domestic violence (DV) incidents to reduce the likelihood that battered women will be victimized again? There is a vigorous debate on whether arrests will deter future abuse or create a retaliatory backlash. We study the question of arrests and the dynamics of DV using detailed administrative data for 183,000 DV emergency calls (999 calls) for West Midlands, the second most populous county in England. We take advantage of the conditionally random assignment of officers to a case by call handlers, combined with the systematic differences across police officers in their propensity to arrest suspected batterers. We find that arrests reduce future 999 DV calls in the ensuing year by 26 percentage points, which translates to a 50% drop. Using both (i) victim versus third-party reporting of repeat 999 DV calls and (ii) measures of repeat severity, we conclude the reduction is not driven by reduced reporting, but instead a decline in repeat victimization. Arrest also increases the likelihood a suspected batterer is charged with a crime and retained in custody during an investigation. Our findings argue against recent calls for a decriminalization of domestic violence and suggest the optimal police response is to lower the threshold for arrests.

Keywords: Domestic violence, arrest, deterrence, revictimization

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1 Introduction

Domestic violence (DV) is a pervasive threat to the well-being of women worldwide, with one third of women reporting some form of physical or sexual violence from a partner during their lifetime (WHO, 2021). A key aspect of DV is that it is seldom a one-time occurrence, with women frequently experiencing repeat abuse by the same partner (Tjaden and Thoennes, 2000; Aizer and Dal Bo, 2009). Despite the prevalence and seriousness of DV, the question of how best to police this crime so as to break the cycle of DV is still largely unresolved.

This paper asks how arrest affects the dynamics of DV. Long-standing and controversial debates abound on this question. Proponents contend that in addition to temporarily incapacitating offenders, arrest deters men from future abuse by signaling a high cost for repeat offenses (Berk, 1993). Opponents raise concerns about backlash effects (Schmidt and Sherman, 1993; Goodmark, 2018), arguing that while arrest offers short-term relief, it triggers an escalation of DV in the longer term as abusers retaliate against their partners. Both sides also make arguments related to the reporting of future DV, with proponents claiming that arrest empowers women and opponents saying that arrest discourages future calls for police help.

Estimating the consequences of arrest on future victimization is challenging for two reasons. The first is the scarcity of large-scale datasets which include information on DV incidents and police responses. In most datasets, DV is not directly identified as a crime category, but must be inferred based on the characteristics of the incident. Moreover, researchers often only observe cases where a criminal charge is filed, but in DV cases, the victim is often reluctant to file a formal charge. And finally, it is difficult to link repeat victimization to a prior incident, as victim identities are generally withheld for confidentiality reasons. And even in datasets which track victim identities, they are usually only recorded if there is a documented criminal charge.

The second estimation challenge is that arrest is endogenous, leading to selection bias. The problem is that there are likely to be characteristics of a case which are observable to police officers, but not the researcher. For example, if cases which result in arrest are (unobservably) more serious and hence more likely to be positively related to the underlying risk of repeat violence, OLS will underestimate any potential deterrent effect or possibly yield a positive estimate even if there is no backlash.

We address both the data and endogeneity challenges in the context of emergency 999 calls in the United Kingdom (similar to 911 calls in the US). On the data front, we observe the universe

of all 999 emergency calls for the second largest municipality in the UK over 10 years. Due to an increased priority placed on domestic abuse in the UK in recent years, DV is singled out as a separate category by call handlers. We can follow an incident from the time the call is placed until the police officers arrive and complete their on-scene investigation (including whether they arrest an suspected offender on the spot). We can merge these records with the UK National Crime Database, allowing us to observe any additional sanctions imposed on the offender (including whether they are held in custody or charged with a crime). To create a linked panel of DV incidents over time, we exploit information on the precise geo-location of where the incident occurred. This takes advantage of the fact that most DV occurs at home and is reported via a 999 emergency call (Britain, 2014).¹ The key benefit of our novel approach is that it allows us to track repeat DV even if there is not a formal charge filed. For this reason, we are likely to pick up a much higher fraction of repeat cases compared to other panel datasets which are limited to formal charges. We provide several checks showing that the scope for misclassification errors using this measure (e.g., residential moves after being exposed to DV) is limited.²

To address endogeneity, we exploit the conditionally random assignment of police officers to 999 DV calls combined with heterogeneity in officers' propensity to arrest.³ Since it is difficult to precisely predict when and where demands for police resources will emerge, the availability of patrol officers which can be dispatched to a DV incident is as good as random after conditioning on time, geography, and the priority level assigned by the call handler. Since some officers are more likely to arrest than others, the average arrest propensity in other cases can be used as an instrument for arrest in the current case. As multiple patrol officers can be dispatched to an incident (there are usually 2 officers in a patrol car), we use a weighted average arrest rate of dispatched team members to DV calls.⁴ As we show, the instrument is highly predictive of arrest in the current cases, but uncorrelated with observable case characteristics.

¹Britain (2014) shows that 9 out of 10 domestic violence incidents are reported via 999.

²For the subsample of cases which result in a charge (and hence for which we observe victim ID), we show that an arrest does not impact whether an observation is identified as a repeat DV incident using geo-location. We also show that our geo-location based results are robust to focusing on areas with single family homes as opposed to multifamily units.

³Our IV strategy is inspired by the quasi-random assignment of criminal cases to judges to investigate the consequences of juvenile incarceration (Aizer and Doyle Jr, 2015), adult incarceration (Kling, 2006; Bhuller et al., 2018, 2020), pre-trial detention (Dobbie et al., 2018), and electronic monitoring (Di Tella and Schargrotsky, 2013). In contexts other than crime, quasi-random assignment of caseworkers or judges has been used to examine the effects of disability insurance receipt (Maestas et al., 2013; Dahl et al., 2014; French and Song, 2014; Autor et al., 2019), child protection (Doyle Jr, 2007, 2008), and consumer bankruptcy (Dobbie and Song, 2015).

⁴We use the initially dispatched officers to construct the instrument, as additional officers may be called to the scene if backup is needed or if the initially dispatched officers are not able to respond.

Our first finding is that arrest reduces the probability of a repeat DV emergency call within the ensuing 12 months by 26 percentage points. This amounts to a sizable 55% reduction relative to the control mean. In sharp contrast, OLS finds a precisely estimated zero effect. The implication is that not accounting for selection bias would lead one to erroneously conclude that arrest has no affect on DV call trajectories.

Whether the reduction in repeat DV 99 emergency calls after an arrest is good or bad from a policy perspective depends on whether it is driven by a reduction in incidence or a reduction in reporting. To better understand the incentives to report, we develop a simple threshold reporting model. In the model, women who experience backlash from an arrest raise the level of abuse they are willing to tolerate before reporting in the future. In contrast, women who are empowered due to the deterrent effect of arrest will report future abuse with a lower threshold. Using a measure of repeat severity in our dataset, we find that reporting thresholds drop on average after an arrest. Through the lens of the model, this implies the decline in future 999 DV calls is not driven by a change in reporting behavior, but rather a reduction in abuse. As a second exercise, we use differences in victim versus third-party reported repeat 999 DV calls. The idea behind this comparison is that any reluctance to report a repeat case due to fears of retaliation should be substantially larger for the victim than a third party. Yet we find roughly similar reductions in DV calls for both groups, consistent with a drop in actual abuse.

To better understand our findings, we next turn to mechanisms. Scepticism about the deterrent effect of arrest frequently arises in relation to its weak “dosage” of punishment. The main concerns are that few arrested offenders are handcuffed, spend much time in custody, or are a jailed overnight, and most are not prosecuted (National Research Council, 1998). Contrary to this view, we find that arrest leads to consequential legal sanctions against suspected batterers. When all on-the-scene work has been completed, patrol officers hand over the incident to an investigative officer. We find that arrest increases the likelihood the investigative officer will both place the suspect in custody and charge him with a crime.

We also find evidence that part of the effect is driven by short-term incapacitation. We find a reduction in DV calls in the 3 days after an arrest, when offenders are most likely to be placed in temporary custody. This accounts for one-fourth of the total effect.

Our paper contributes to several lines of research. Much of what we currently know about the impact of arrest on domestic assault comes from the 1981 Minneapolis Domestic Violence

Experiment (Sherman and Berk, 1984) and its replications in Omaha (Dunford et al., 1990), Charlotte (Hirschel and Hutchison III, 1992), Milwaukee (Sherman et al., 1992), Dade County (Pate and Hamilton, 1992), and Colorado Springs (Berk et al., 1992). In these social experiments, the research design called for one of three treatments to be randomly enforced by patrol officers encountering a DV situation: arrest, separate, or advise. Despite being highly innovative at the time, the significance of these trials is limited in that they produced inconclusive findings⁵, had to contend with small samples, and suffered from flawed designs. A major concern was non-compliance with random assignment: patrol officers frequently deviated from the treatment called for by random assignment, and their delivered treatments had a substantial behavioral component (Angrist, 2006).

In the 2000s, researchers set out to assess the effects of mandatory arrest laws and no-drop policies on DV. The state-level analysis of Iyengar (2009) shows that intimate partner homicides increased in states with mandatory arrest laws, which might be explained by an escalation of intra-family violence triggered by reduced reporting by victims. No-drop policies compel prosecutors to continue with prosecution even if DV victims request that charges be dropped. Aizer and Dal Bo (2009) find that the adoption of such policies in the US resulted in a reduction of male homicides by intimate partners and an increase in reporting of DV. This finding can be rationalized in a model in which (i) women in violent relationships have time inconsistent preferences and (ii) no-drop policies offer women a cheaper commitment device to end violent relationships than murder.

In the past decade, empirical research exploring the drivers of DV has burgeoned. For developed countries, scholars have linked DV to gender-specific local labor market conditions (Aizer, 2010; Anderberg et al., 2016; Tur-Prats, 2021), to divorce laws (Stevenson and Wolfers, 2006), and to emotional cues (Card and Dahl, 2011). For developing countries, drivers of DV include ancient cultural and anthropological factors (Alesina et al., 2021), colonial history (Guarnieri and Rainer, 2021), resource inequality (Cools and Kotsadam, 2017), cash-transfers to women (Bobonis et al., 2013; Hidrobo et al., 2016), and male alcohol consumption (Angelucci, 2008). In a recent study, Bhalotra et al. (2021) merge Brazilian employer-employee registers with administrative data on DV cases brought to criminal courts, which allows them to estimate the impacts of job loss and unemployment benefits on DV.

⁵In Omaha, Charlotte, and Milwaukee, offenders assigned to the arrest group showed higher levels of repeat offending while in the other three jurisdictions, a modest reduction in recidivism was found among batterers assigned to arrest (Schmidt and Sherman, 1993).

Taken together, our findings provide a cautionary tale for recent policy debates calling for a decriminalization of DV. Goodmark (2018) argues that the criminal legal system has been ineffective in deterring DV and has had detrimental consequences for victims, offenders, and their communities. Arrest in particular has been singled out as having negligible effects on DV recidivism. Indeed, our OLS estimates show no association between arrest and future violence. However, our causal estimates indicate a strong deterrent effect of arrest for domestic assault. Our results argue against recent calls to decriminalize DV and suggest instead that the optimal police response is to deal more strictly with batterers and lower the threshold for arrests.

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