Public Services Access and Domestic Violence

Lessons from a Randomized Controlled Trial*

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November 23, 2020

Abstract

We conducted a randomized controlled trial of an intervention designed to assist victims in accessing non-police support services. The intervention led to a 22% decrease in the provision of statements by victims to police, but no significant change in criminal justice outcomes against perpetrators. This is consistent with the treatment response in statements coming from victims for whom a statement was relatively less effective for pursuing criminal sanctions, in part explained by a 84% decrease in withdrawn statements for the treatment group. Over a two-year period, reported domestic violence outcomes do not differ significantly between the treatment and control group.

Keywords: Public services, domestic violence, RCT, allocative efficiency

JEL Codes: I18, J12, H75

*We are grateful for comments provided by Anna Aizer, Sofia Amaral, Dan Anderberg, Orazio Attanasio, Herb Emery, Gianni De Fraja, Esther Duflo, Mark Hoekstra, Pamela Jervis, Magne Mogstad, Krzysztof Karbownik, Melanie Khamis, Tom Kirchmaier, Bettina Siflinger, Chris Wallace, Tanya Wilson, and seminar participants at the Asian Meeting of the Econometric Society, the Bristol Workshop on Economic Policy Interventions and Behaviour, the Essen Health Conference, IZA Workshop on Family and Gender Economics, Royal Economic Society Meeting, Royal Holloway UL, SITE Stockholm, and the Warsaw Ce^2 workshop. Eliot Button, Corinne Crosbourne, Pooja Pattni, and Chloe Rawson provided excellent research assistance. We thank Leicestershire Police, the Office of the Leicestershire Police and Crime Commissioner, and Leicester City Council for enabling the evaluation as a randomized controlled trial and for data access. We gratefully acknowledge funding from the UK Ministry of Justice PCC Fund. AEA RCT Registry No. AEARCTR-0000537.

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1 Introduction

Domestic violence[1] is a problem of first order importance across the world. In 2018, there were 1.2 million reports of domestic incidents to police in England and Wales, leading to approximately one-third of all arrests made by police forces (Home Office 2019, ONS 2019). These numbers do not include the many more cases never reported to the police; it is estimated that up to one in three women experience domestic abuse in their lifetime. Of those cases reported to police, less than six percent resulted in the conviction of a perpetrator. While the scale of the problem of DV is vast, progress in identifying interventions to reduce the incidence of DV and improve the well-being of victims has been modest.

The large number of DV cases is partly due to repeat incidents involving the same households, so that a small number of repeat-violence households disproportionately contribute to the overall number of police reports. This comes amid the availability of a number of public and private non-profit organizations to help victims of domestic violence to address long-term abuse by providing services and opportunities for change of personal circumstances.[2] One potential problem that has received a great deal of attention by practitioners is that victims are unaware of these services, or find it difficult to access them.[3]

In this paper, we provide randomized evidence on whether improving victim access to existing services will lead to better DV outcomes. For this purpose, we worked with a large UK police force to implement a randomized controlled trial (RCT) of an intervention specifically designed to improve access to non-police support services.[4] The trial focused

[1]Domestic violence (DV), as used here, refers to both intimate partner violence and violence between family members.

[2]These include the provision of temporary housing and women's shelters, help with permanent housing, designing of and helping to implement escape plans to leave a violent partner, legal aid, among other forms of support to leave a perpetrator.

[3]This was highlighted in a report on the policing of domestic violence by the UK police watchdog (HMIC, 2014) and Fugate et al. (2005) show that information barriers are a significant deterrent for victims of domestic violence in the United States.

[4]The programme is known as *Project 360*, reflecting the role of the caseworker in taking a 360-degree look at victims' needs and the corresponding available services.

on households that have experienced multiple reports of DV over a period of 365 days. The intervention provided victims of police-reported DV with a caseworker who offered information about and assistance in accessing support services. The trial lasted for six months and randomization took place at the level of individual victims. The final sample of over one thousand households constitutes one of the largest RCTs on DV to date. Our analysis is based on a unique dataset that we constructed by linking information from local police administrative records, the UK Police National Database, and a victim follow-up survey. These extraordinarily rich data, collected over a two-year period, allow us to follow the lifecycle of every case in our sample, from the time a case is opened to the time a perpetrator is sentenced in court.

Three important findings come forth from this study. First, treatment group victims were significantly less likely to provide police with a statement.[5] We find that the intervention led to an intention-to-treat effect of a 6.5 percentage point decrease (21.7% relative to the control group mean) in statement provision. Using information on the precise timing of when statements are provided to police, we show that while there is no difference in the number of statements provided during the initial police callout (before treatment status is assigned), the number of statements made after the treated group received the intervention differs significantly. Using data collected through a victim survey one month following the intervention, we also find that the treatment group is more likely than the control group to report using non-police support services. Taken together, these results are consistent with the victims in this study using non-police support services and police services as substitutes.

Second, despite the decrease in statements, we do not find an effect of the intervention on criminal sanctions against a perpetrator (specifically, arrest by the police, charges by the Crown Prosecution Service, and sentencing by the courts). This is surprising, as the

[5] In our context, providing a statement to police would be analogous to what is commonly called "pressing charges" in the North American context.

correlation between the provision of a statement and criminal sanctions against a perpetrator is positive and strong. This suggests that for victims who forgo making a statement in treatment, the effect of their statement on criminal sanctions is low relative to other victims. One plausible mechanism underlying this result is statement retraction by victims. Relative to the control group, treatment group statements are 10.3 percentage points, or 84%, less likely to be retracted. We interpret this result as the intervention increasing the efficiency of police service utilization by removing ineffective statements from the service load of police officers.

Third, different from previous secondary responder programmes, of which some reported an increase in repeat victimization, we do not find a significant effect on repeat police-reported household violence over a two-year period. Specifically, treatment group households are as likely as control group households to experience a repeat police callout (77.8% and 74.9% respectively), and have approximately the same number of repeats (3.0 and 2.7 on average). Furthermore, duration analysis suggests that the timing of repeat reporting does not significantly differ between the two groups. Across several measures that reflect the severity of future cases, we find suggestive evidence that treatment group cases are less severe. For example, the average risk assessment score assigned to the treatment group is 9.5% lower for the treatment group than for the control group. While this difference is only marginally statistically significant, taken together with the other measures of incident severity, this provides evidence consistent with the intervention leading to an increase in willingness of victims to report future incidents to police.

This interpretation is supported by estimates using supplementary data specifically collected through a victim survey one month following the intervention to study outcomes not found in administrative records. Using the victim survey data, we find an increase in the willingness to report any future incidents to police. We also find that the treatment group has an overall lower risk of repeat victimization. In particular, we find that individuals in the treatment group are more likely to report no longer being in contact with the perpetrator. Finally, we find that the treatment group is more likely than the control group to report using non-police support services. Despite these positive findings of the intervention on the safety of the victim, we find that reported measures of stress for treatment group individuals are higher than for control group victims one-month after the initial incident, possibly indicating the increase in stress associated with changes in the personal circumstances of the victim engaging with DV services.

The theoretical basis for the intervention design is based on a household bargaining model (Aizer, 2010; Anderberg and Rainer, 2013; Anderberg, Rainer, Wadsworth and Wilson, 2016; Bobonis, Gonzalez-Brenes, and Catro, 2013; Farmer and Tiefenthaler, 1996). If support services improve the outside options available to victims of household violence, we expect that making these support services easier to access will lead to a decrease in violence. As it is not possible to observe violence in the household directly, we must attempt to infer changes based on reporting behaviour and our survey response. Our results are consistent with the intervention leading to an increase in willingness to report future incidents, with the number of reported incidents increasing and the severity of reported incidents decreasing.

This paper contributes to two strands of literature. The first is a literature studying the role of public policy to improve domestic violence outcomes. In particular, there is a number of studies using experimental designs to analyse *secondary responder* programmes, in which police officers or officer/social worker teams follow-up on households after an initial report of violence (Davis and Taylor, 1997; Davis, Weisburd and Hamilton, 2010; Hovell, Seid and Liles, 2006; Casey, et al, 2007; Stover, Poole and Marans, 2009; Stover, Berkman, Desai and Marans, 2010). These studies unanimously find that secondary responder programmes, at best, do not lead to a change in household violence and may even increase household violence. The intervention studied here differs from previous research in a number of important ways. The intervention was designed with the primary goal of making support services more accessible to victims. Steps are taken to ensure this is done without involving the perpetrator. The intervention caseworkers were domestic abuse specialist, with extensive local knowledge and experience with accessing existing services, rather than police officers as in previous secondary responder programmes. Because the caseworkers were embedded within the police, they still benefited from access to police intelligence. The study also differs from previous work in ways that improve our ability to infer programme effects. First, the RCT design here does not allow for any override of random assignment to treatment. Previous RCTs (Davis and Taylor, 1997; Davis, Weisburd and Hamilton, 2010) involved a small number of treatment assignment overrides by the police, which may have lead to estimation biases. Second, the RCT studied here has a sample size more than double that of previous RCTs. This allows us to estimate significantly smaller effects that previous studies may have missed. Third, the dataset built for our analysis is based primarily on police administrative records over a two-year period. These records provide us with a standardized measure of the severity of each incident. This allows us to answer the question whether the intervention reduced repeat incidents and/or changed reporting patterns by looking at the severity of the attended subsequent incidents.

This paper both contributes to and is informed by a second strand of the literature studying "nudges" to overcome information barriers in general service choice. Examples from previous work find that simplifying information on public school performance leads parents to select higher performing schools for their children (Hastings and Weinstein, 2008), that providing information on the cost and benefits of education changes students' intention to stay in non-compulsory education (McGuigan, McNally, and Wyness, 2016), that personalized prescription drug plan information makes Medicare users more likely to switch to lower cost plans (Kling, Mullainathan, Shafir, Vermeulen, and Wrobel, 2012), and that assistance for filling out complex college aid applications leads to a significant increase in college enrolment (Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012). These studies demonstrate that even small bureaucratic barriers or costs to obtain and process information lead to distortions in choice relative to what is chosen absent these barriers. Our study is similar in spirit, considering a relatively simple and inexpensive change to the way that victims of DV receive assistance following a police-reported incident. If victims of DV find it difficult to access services, or determine which services are best suited for their needs, then they may rely on simple heuristics, such as utilizing police services with which they already interact. Unlike in these previous studies, we consider service users who choose among different, non-exclusive services. Potential service users can, and do, choose more than one service. Because services are not explicitly priced, users do not internalize the cost of service provision and may allocate themselves in such a way that service costs outweigh the private benefits. This is a general problem with any publicly available service.[6] Our results suggest that the service users who forgo police services when provided with the intervention, are those who, on average, benefit the least from police services. If the cost of providing police services is high relative to non-police services, then the intervention is likely to improve allocative efficiency. This is particularly important for services related to DV because of their frequency and relevance for policing. In the UK, domestic violence and abuse account for approximately 11% of all crimes reported to police,[7] creating very substantial service demand on police forces in the country.

There are limitations to the interpretation of our results. Specifically, we do not generalise beyond the selection criteria for our subject pool, focused on households with previous police reported domestic incidents. We do not attempt to draw any conclusions about the use or effectiveness of public support services for households going through their first police reported domestic violence, or households with unreported domestic violence. Also, the households in our subject pool received treatment at most once. We cannot draw any conclusions about

[6]This problem may, for example, show up in the case of school selection or attendance, as in Hastings and Weinstein (2008) or Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012).

[7]This number is based on official statistics provided in ONS 2018a and ONS 2018b.

whether repeated access to the programme would have different results.

The paper is structured as follows. Section 2 provides the contextual background information for the experiment. Section 3 provides details of RCT design and implementation, followed by data sources and data collection. The main results of the paper are presented in Section 4. This is followed by a discussion and interpretation of results in Section 5. Section 6 summarizes the findings and puts them in a wider context.

2 Background

Non-police services available to victims of DV

In the UK, DV support services are available through a number of publicly funded and voluntary service providers. In the police force area we study (Leicestershire, UK)[8], and the time of the intervention, 24 different agencies provided various domestic violence support. In Appendix F we provide detailed information about the available services including a table summarizing all DV service providers, a list of the categories of services most accessed by the treatment group in this study, and the service information pamphlet that police provided to all victims following a domestic incident.

Barriers exist that make it costly for victims to access these services.[9] These barriers can arise from four non-exclusive sources: first, victims may lack information about the existence and availability of these services or the process to access them; second, barriers may arise from the complexity of choice over the often large set of services, similar to that explored in Hastings and Weinstein (2008) and Kling, et al. (2012); third, barriers may originate at the individual level from psychological and/or language barriers; fourth, service providers

[8]This police force area covers three local councils, roughly comparable to US counties, Leicester city, Leicestershire and Rutland.

[9]In Appendix A we provide a stylized conceptual framework to guide our thinking about the relationship between access barriers and the choice between various DV services.

often put formal barriers in place, with the purpose of ensuring the safety of the users and to restrict the use of scarce resources.

While we do not explicitly distinguish between different sources of barriers, they are widely recognised as an impediment to service uptake.[10] The intervention we study is specifically designed to help victims of DV overcome any of these barriers and reduce the cost of victims to access services by providing information on existing services, by signposting victims to the appropriate service, by helping them overcome psychological and language barriers, and by providing referrals to these services.

Police services available to victims of DV

We refer to police attending a domestic violence incident in response to an emergency call made by a victim or a third party as the *initial callout*. When police officers attend an initial callout, they have two tasks. The first task is to defuse the immediate, and potentially volatile, situation and ensure the safety of all individuals involved. Police have the power to arrest and temporarily detain a perpetrator for up to 24 hours solely for this purpose.[11] The second task is to collect evidence within the initial investigation to determine whether to initiate further investigations for the purpose of pursuing criminal sanctions against the perpetrator. Evidence can be direct, such as police observing and recording a physical assault, for example, through body-worn cameras. More often, however, evidence is indirect in the form of statements made by witnesses. A statement is a recorded recollection of events by a witness that can be used as evidence in court.

[10] *Her Majesty's Inspectorate of Constabulary* (HMIC 2014) reports anecdotal evidence, based on victim interviews, that victims of DV felt that they did not know where to turn for help after an initial callout. These barriers to services are not unique to the UK context. In the US and Canada, Jaffe et al. (2002) find that "women reported feeling let down and confused by the [community and social services support] process." They find that many women removed their application for services out of frustration with the number of barriers. In interviews with DV victims in Chicago, Fugate et al. (2005) find that perceived barriers to access, particularly lack of information, are an important explanation for whether or not victims contact social and counselling services, but not important for explaining why they contact police services.

[11] This arrest may be made for 24 hours independent of the victim's willingness to make a statement. After 24 hours, the Crown Prosecution Service must press charges, or the perpetrator must be released.

We use *police services* to refer to the further investigative work undertaken to pursue criminal sanctions against a perpetrator. In contrast to the two tasks outlined above, which are performed at every initial callout, further investigative police services are only performed if there is reason to believe that there will be sufficient evidence for the *Crown Prosecution Service* (CPS) to pursue charges. The CPS makes a decision whether to charge the perpetrator for the purpose of pursuing criminal sanctions on the strength of the available evidence.[12]

Figure 1 about here

The decision to provide a statement is a mechanism through which the victim can influence the progression of the case towards criminal sanctions against the perpetrator. In the majority of DV cases, the victim is the primary witness, and the victim's statement is the major piece of available evidence. A victim can provide a statement at the initial callout (in our data, 50.1% of victims who provide a statement do so at the initial callout), or a victim can contact the police and provide a statement any time after the initial callout. Once a statement is provided, the victim may decide to withdraw the statement at any time (in our data, 17.0% of all statements are withdrawn). If this happens, the statement cannot be used as evidence in the case against the perpetrator.[13]

The correlation between victim statement provision and perpetrator arrest and charge is strong (see Figure 1). In the 743 cases for which no statement was provided, the perpetrator was subsequently arrested in 10.0% of cases and charged in 3.0% of cases. In the 272 cases for which a statement was provided, the perpetrator was arrested in 68.2% and charged in

[12]Evidence of this is found in that data (Figure 1). In cases where charges are laid, 62.9% result in sentencing by the courts (including prison time (24.7%), fines (43.6%), restraining orders (39.7%), and mandatory rehabilitation programmes (17.6%)). There is no significant difference for cases in which a statement is made (63.7% versus 59.0%). This is consistent with the role of the CPS in filtering cases that proceed to the courts based on the strength of the evidence.

[13]Advice for victims provided by the charity *Rights of Women* states, in reference to victim statement provision in domestic violence cases, "Without a witness statement from you it is unlikely that the police will continue." (Rights of Women, 2013).

37.6%. Of course, this does not tell us anything about the causal effect of statements on arrests and charges; victims may select into making a statement based on their subjective expectation of probability of an arrest. However, this correlation provides evidence on the importance of statement provision in pursuing punitive action against a perpetrator.

3 Experimental design and data

We set up a randomized controlled trial in the Leicestershire Police Force area (*Leicestershire* hereafter), UK, jointly with Leicestershire Police and the three local authorities in Leicestershire.[14] Leicestershire (see Figure 2 for map) covers a population of approximately one million people, and Leicestershire Police is one of 43 police forces in England. One-third of the population in Leicestershire is concentrated in the city of Leicester, with the remaining population distributed across approximately 300 towns and villages. The experiment ran between November 2014 and April 2015.

Figure 2 about here

3.1 Allocation of cases into the subject pool

We worked with the Leicestershire Police IT services team to design an automated computer application for selecting the subject pool and for assigning treatment.

After responding to a domestic incident, officers register the case, recorded as a domestic incident report, in the Leicestershire Police database. Our automated application performed a daily scheduled search through all newly recorded incidents and recovered domestic incident cases that met several conditions: 1) the report was filed as a *domestic incident*; 2) in the previous 365 days, the victim had shown up in at least three DV reports (including the

[14] Leicester City Council, Leicestershire County Council, and Rutland County Council.

current one) and fewer than seven DV reports;[15] 3) the victim was not in the subject pool previously (as either treatment or control); and 4) the victim was not assessed by responding officers as *high risk*.[16] All cases that met these criteria were assigned to the subject pool. The application automatically allocated subject pool cases to treatment or control groups, each with a 50% probability. During the trial period, more than fifty reported domestic incidents were recorded daily with seven, on average, qualifying for the subject pool. For the purpose of examining statements and conducting the survey, the person labelled *victim* in each case report was assigned as the subject.

The final sample consists of 1,017 cases, with each case referring to a unique victim. Of these, two cases were dropped due to restrictions on access to police data.[17] There were a small number of cases for which we do not have information on all control variables. For the purpose of the regression analysis, these missing values will be given a value of 0 and a variable-specific dummy will be used to indicate missing values.[18] The final dataset for our analysis consists of 1,015 unique cases. Of these, 510 cases are in the treatment group and 505 are in the control group.

3.2 Control

All cases in the subject pool received standard police procedure as described in Section 2. Upon attending the initial callout, responding officers left a pamphlet with victims that lists, describes and provides contact information for some of the available DV services in Leicestershire (see Appendix F). Victims were able to contact the services on this pamphlet

[15] The initial interest of this intervention was to assist victims of repeat domestic violence. The minimum of three offences was based on predicted capacity constraints of the trial. If there were more than seven DV incidents in the households, the case is classified as high-risk and was referred to a Multi-Agency Risk Assessment Conference that provides a separate intervention as standard procedure.

[16] Cases categorized as *High* were excluded as these are transferred to a multi agency risk assessment conference (MARAC) and treated separately.

[17] This would happen in the case in which individuals in the case are under investigation for a serious offence such as sexual assault involving a minor.

[18] All reported results are robust to the exclusion of missing variables from the analysis.

at any point. Victims were also invited to provide a statement to police at any point during or after the initial callout.

3.3 Treatment

A treatment group case was assigned to a caseworker the morning following the case being recorded in the police database. Cases were allocated non-randomly among the caseworkers according to workload and availability. Three dedicated caseworkers were employed for the trial. The caseworkers were female and between the ages of 25 and 35. Caseworkers all had previous training and experience as domestic abuse support workers. Specifically, all had previous experience in working with DV support services in Leicestershire and had specialized knowledge of the various local services available and how to access them. They also received training specific to the service provided through the intervention in this study.[19] Caseworkers were provided with desk space and IT support in a large Leicestershire Police station.

The caseworker attempted to contact subjects via telephone within 24 hours[20] of the initial police report. Once contact was made, the caseworker described to the subject the publicly provided support services that are available locally. If the subject expressed a wish to access a specific support service, the caseworker assisted in initiating access. This included organizing initial contact with the relevant support service, helping complete any paperwork, and providing a referral when necessary. All contacted subjects were offered a face-to-face meeting with the caseworker to go through the options available. If the victim expressed an interest to leave the perpetrator, the caseworker also assisted in preparing an escape plan. The intervention ended when either the victim declined to participate in the intervention or

[19] One of this study's authors was present during these training sessions.

[20] While caseworkers were on duty and attempted to make contact on Saturdays, victims of incidents occurring between Saturday evening and Monday morning were all contacted on Monday, thus extending the period of first contact to 36–48 hours in these cases.

a relevant support service had taken up the case.

Although the specific content of each interaction varied by case, important features of the intervention were common to all cases. First, a caseworker attempted initial contact with victims within a short time period (24 hours) after the police report of the incident was filed. Second, caseworkers had access to all police information about both victim and perpetrators, including historical police records. Third, subjects were informed of available non-police services, and, if they wished to move forward, caseworkers provided assistance in accessing these services.

We define a victim as having *engaged* with the intervention if they were successfully contacted by a caseworker and they accepted some form of assistance, ranging from the provision of advice during a one-time phone conversation to face-to-face follow-up meetings. While an effort was made to deliver the intervention to all 501 victims assigned to the treatment group, 240 (48%) of treatment group victims did not engage.[21] Of these, 143 victims were contacted by a caseworker by telephone but refused both phone-based assistance and a face-to-face meeting. For the remaining 97 victims, caseworkers were unable to make contact given the available contact information.[22]

Among victims whom the caseworkers were able to contact, the engagement rate was 65%.[23] Considering that caseworkers "cold-called" the victims, this is a notable take-up rate. Of the 261 victims who did engage, 128, or 49%, had at least one face-to-face meeting with the caseworker. Just under 35% of all home visits took place within 24 hours of the initial callout (the same day that caseworkers made first contact), with another 20% taking place within three days. In all, 33% of home visits took place after three days but within a

[21] A maximum of 5 attempts, at different times of the day across 5 days, was made to contact victims by phone.

[22] For the victims' safety, the caller ID was not displayed, which may have led some victims to not answer the call.

[23] In Appendix C we provide correlates between victim, perpetrator and household characteristics, and treatment uptake.

week, and the remaining 13% took place more than one week after the initial callout.[24]

3.4 Internal validity

Several design features of the trial safeguard the internal validity of this study. Most importantly, all assignments to the treatment and control groups were automated and randomized. Unlike previous RCTs of similar second responder interventions (Davis and Taylor, 1997; Davis, Weisburd, and Hamilton, 2007), caseworkers or police officers could not override assignment to treatment.[25]

The timing of treatment assignment occurs after the initial callout when the responding officer records the case in the police database. This ensures that the actions taken by police at the initial callout were not influenced by knowledge of treatment assignment. Further, this provides a falsification test, which we exploit in Section 4.1, as statements made during the initial callout cannot not be influenced by treatment status.

Caseworkers only received information on cases in the treatment group. While caseworkers could have theoretically searched police reports for other reported DV cases on their own initiative, we are confident this did not happen. Every access to a report in the police information system is recorded and monitored, and unauthorized access to cases not in the treatment group by the caseworkers might have resulted in disciplinary action.

3.5 Data

This study is built around a unique and innovative data set that we constructed from three sources, briefly discussed below.

[24] In Appendix D we provide and test an alternative rationalization of our main results based on the timing between the initial callout and the visit by the engagement worker creating a cooling off period, which decreases statement provision. We show that the data do not support this rationalization.

[25] Police officers did not have access to information on the treatment status of victims of DV. Furthermore, based on informal discussions with members of Leicestershire Police, most officers responding to DV calls were not aware of the intervention during the trial.

Leicestershire Police Database

We matched cases in the subject pool with Leicestershire Police administrative records, from a number of internal databases (detailed in Appendix E), using a unique crime reference number. The administrative records from these databases provide information on the initial incident (date, time, location, attending police officers, risk assessment score, provision of a statement by victim, and actions taken by police) and a wealth of information on the victim and perpetrator, including demographic characteristics, household information, and previous and subsequent police records.

We used personal identifiers, including name, date of birth, and address, to link information for victims and perpetrators across different cases over time. We collected information for reported incidents, involving victims in the subject pool, over a two year period following the intervention. For each reported case we collected information reflecting the timing, actions taken by police officers and the risk assessment score provided by responding officers.

The information was collected by three research assistants who did not have information on the treatment status of individual cases.[26] A fourth research assistant checked the recorded information for consistency and accuracy from a random draw of approximately 30% of the cases.

Police National Database

Outcomes of the criminal justice process are not contained in the administrative records of Leicestershire Police. This information is only available from the Police National Database (PND), designed to share intelligence across all police forces and criminal justice agencies throughout the UK. The PND holds over 3.5 billion searchable records with information

[26]IT and data protection training was provided by Leicestershire Police to the research assistants and the authors over a three-day workshop prior to data collection. Because of the sensitive nature of the data accessed in these databases, research assistants and the authors went through police vetting and criminal background checks. All research assistants were undergraduate students at the University of Leicester with a background in law or criminology.

about individuals who have been arrested, charged, and convicted. The nationwide coverage allows us to track individuals beyond the Leicestershire Police Force area and access information on all convictions of individuals.

The unique crime reference number given to each case allows us to link information from Leicestershire Police records to information from the PND. These linkages were cross-checked by the recorded date of the incident. We collected information on whether a perpetrator was arrested by police during or following a DV incident, whether the perpetrator was charged by the CPS, and whether a perpetrator was sentenced in court for the incident (along with details of the sentencing). Prosecution and court information was accessed more than 24 months after the randomized intervention took place, to allow for criminal justice proceedings to be completed.

Because access to the PND is highly restricted, even within the police force, the data were collected by a specially trained and licensed police officer for whom every access to the PND was authorized for the research project. This officer was blind to the treatment status of individual cases.

Victim survey

Outcomes of interest relating to victim safety, well-being and non-police service use are not available from administrative sources. For this reason, we designed a victim survey to collect supplementary information.[27] However, there were important practical and ethical implications for the repeated collection of sensitive survey information from domestic violence victims. For this reason, our data collection was limited to a single application of the survey one month after the intervention.

The victim survey was conducted by the Leicestershire Police Information Services Unit using researchers specifically trained in surveying victims of crime. Interviewers conducted

[27] The full survey can be found on the project website, https://prj360.org/wp-content/uploads/2019/04/P360_VictimSurvey.pdf

the survey blind to the treatment status of the interviewee. Surveys were administered approximately one month following the initial callout and completed over the telephone using the safe number provided to police at the initial callout.[28] The survey was administered to a 25% random sample of the full subject pool.[29] From this sample, we received an 84% response rate, resulting in complete surveys for 105 treatment group subjects and 109 control group subjects.

3.6 Descriptive statistics and treatment/control group balance

We test the random assignment of cases by comparing mean characteristics between the treatment and control groups (Table 1). Based on the reported characteristics, treatment and control are well-balanced; most observables do not differ significantly between the two groups.[30] Some important characteristics reflecting incident severity and the state of household violence are worth highlighting. Specifically, the average number of cases over the last year (2.33 and 2.26) and the responding officer's victim risk assessment score (1.28 and 1.28) do not differ significantly between treatment and control. Furthermore, we do not observe a significant difference of intimate partner status of victim and perpetrator or the presence of children in the household. We therefore interpret Table 1 as evidence that allocation to the treatment or control group was random.[31]

[28]Researchers followed strict procedure to ensure the safety of victims of DV, and conducted the interview only if the interviewee ensured the researcher that the perpetrator was not in the premises and after the location of the victim had been recorded. In case the connection to the victim's mobile phone was interrupted, a rapid response police unit was sent to the premises to ensure safety of the interviewee.

[29]This sample was negotiated with the Leicestershire Police Information Services Unit based on their resource constraints.

[30]Two exceptions should be noted. First, at the time of the initial callout, perpetrators in the treatment group have 1.16 more registered instances of domestic violence than do perpetrators in the control group. Second, victims and perpetrators are 6 percentage points more likely to be living together in the treatment group than in the control group. At the 5% and 10% levels of significance, the number of significant differences is roughly what one would expect to occur by chance. The remaining differences are both statistically insignificant and small in magnitude.

[31]In Appendix C.2 we provide additional evidence that treatment status is randomly distributed across the 68 neighbourhoods (police beats) represented in these data.

Table 1 about here

The descriptive statistics for this sample are consistent with the picture about demographic characteristics of victims and perpetrators based on previous studies. In total, 87% of victims versus 14% of perpetrators are female. On average, victims are slightly older than perpetrators (34.5 years versus 33.2 years). The victim and perpetrator are intimate partners in 77% of cases, and cohabiting at the time of the initial callout in 55% of cases. In all, 58% of the sample households with children have an average of 1.95 children each.

4 Results

In this section, we present results for a number of outcomes reflecting various stages of the case life-cycle. We start with outcomes reflecting the effect of the intervention on victim use of police and non-police services, and short-run measures of self-reported well-being. We then look at the effect of the intervention on the frequency and severity of repeat police-reported domestic violence over a two-year period. Finally, we look at intermediate outcomes for arrest, charges and conviction of the perpetrator.

Estimates are interpreted as an intention to treat (ITT), denoted by γ_1 in the linear probability regression (1).

$$S_i = \gamma_0 + \gamma_1 treat_i + X_i'\Gamma + e_i \tag{1}$$

S_i reflects the outcome measure under consideration. $treat_i$ is an indicator equal to 1 if i was assigned to the treatment group and 0 if i was assigned to the control group. X_i denotes a vector of variables including victim and perpetrator sex, victim and perpetrator age, a *white* race indicator for victim and perpetrator, an indicator for cohabitation, an indicator for children being present in the household, the number of police-reported domestic incidents in the previous year, and dummy variables for the location of the initial callout across 68 neighbourhoods (police beats).[32] e_i captures all other influences on the respective outcome y_i that are unobserved by the researchers. e_i and the randomly assigned $treat_i$ are assumed to be uncorrelated.

4.1 Programme effect on statement provision and police service use

In Table 2, we report the estimated treatment effects for the provision of victim statements to police. The unconditional difference between treatment and control (Column 1) shows that there is a 6.2 percentage point decrease in statement provision between the treatment and control group ($p = 0.026$). The coefficient is very similar when control variables are added, and this effect indicates that the intervention lead to a 6.5 percentage point decrease in the provision of statements by victims to the police ($p = 0.014$). This corresponds to a 21.7% decrease relative to statement provision in the control group.

In addition to the ITT we also report a local average treatment effect (LATE), reflecting the treatment effect for victims who engage with the intervention.[33] The LATE estimate suggests that victims who engaged with the intervention are 12.6 percentage points less likely to provide a statement to the police ($p = 0.012$). This is a large effect, and corresponds to a 42.1% decrease relative to statement provision by the control group.[34]

Table 2 about here

Given the timing of the intervention, we should not observe an effect on statements that

[32] Some of these variables contain a small number of missing values. In these cases we set the missing equal to 0, and include a corresponding missing dummy equal to 1 for missing values and 0 otherwise. X_i includes the full set of these dummy variables.

[33] The LATE is calculated using a two-stage least squares estimator. In the first stage we regress a dummy variable for intervention *engagement* on an indicator for treatment group status ($treat_i$ from Equation (1)), where engagement is defined in Section 3.3. In the second stage provision of a statement is regressed on predicted values of treatment engagement. The first stage instrument is strong, with a F-statistic $F = 537$.

[34] Of course, we cannot determine how large this effect is relative to statement provision among the unobservable subset of the control group that would take up the intervention had they been offered.

are provided to the police prior to contact with the caseworker. We test this by estimating the ITT for making a statement during the initial police callout ($t = 0$, before treatment) and (conditional on *no statement* at $t = 0$) making a statement at least one day after the initial police callout ($t > 0$, after treatment). As expected, the treatment and control group provide statements at $t = 0$ at the same frequency (Column 4, Table 2). The estimated treatment effect is a statistically insignificant difference of -0.011 percentage points (p=0.621). The treatment group is less likely than the control group to make a statement at $t > 0$ (Column 5, Table 2). The estimated treatment effect is -6.2 percentage points, confirming that the difference in statement making estimated earlier arises solely from any difference arising after the initial police callout as expected (p = 0.011).

Figure 3 about here

We examine the timing of statements further in Figure 3 by looking at the difference in statement provision between the treatment and control groups for ten days following since the initial callout (*day 0*). In Figure 3(a), we plot the probability of a statement (conditional on no statement in previous days) against days since the initial incident. In Figure 3(b), we plot the treatment-control group differences in the probability of statement provided corresponding to each day. These figures draw attention to several points. First, both the treatment and the control group exhibit a similar pattern of the propensity of early statement making that dissipates rapidly over time. By $t = 4$, the propensity to make a statement on a given day is less than 1%. Second, consistent with Table 2, we do not observe a significant difference at $t = 0$, the day with most statements made. Third, a negative treatment-control statement gap persists from $t = 1$ to $t = 4$ days following the initial callout; we do not observe a distinguishable statement difference in days for which statement making is relatively infrequent ($t > 4$).

4.2 Programme effect on non-police service use, re-victimization risk, and well-being

Non-police services cover a number of different forms of assistance. For treatment group subjects who engaged with the intervention, we have detailed information on service use following the initial incident.[35] The most common services include refuge (9.2% uptake), registering with a general practitioner (12.3% uptake), counselling services (48.4% uptake) and personal safety planning (60.5% uptake).

As discussed in Section 2, non-police services are administered by a large number of independent agencies, making the collection of administrative data for our sample infeasible. To estimate the effect of treatment on the use of non-police services we use information from the one-month follow-up survey, in which subjects self-report service uptake (Panel A, Figure 4).

Treatment effect estimates for use of non-police services are positive and non-trivial in magnitude.[36] The treatment group is 17.9 percentage points (61.7%) more likely than the control to state they have visited their general practitioner as a result of the initial incident ($p = 0.042$). The treatment group is also 6.5 percentage points (163%) more likely to have attended accidents and emergency department following the incident ($p = 0.056$). Subjects in the treatment group are 12.8 percentage points (21%) more likely to state they used a non-police service other than health services and they are 2.4 percentage points more to report to be confident in accessing existing DV services, both estimates are nevertheless not significant at conventional levels. We summarize these results with an index of service uptake (following Anderson, 2008). Overall, the intervention had a strong and significant positive

[35] This information, taken from caseworker reports for the 261 subjects who engaged with the intervention, is summarized in Table F2 of the supplementary appendix.

[36] However, our ability to get precise estimates is somewhat limited by the survey's sample size. Given the sample of 214, for variables with a mean of 50%, we require a treatment effect of over 11 percentage points to be statistically significant at the 10% level.

effect on service uptake beyond what was provided directly through the intervention (p = 0.042).

Figure 4 about here

We also asked victims, whether their personal safety had improved since the initial incident. The survey results suggest that the programme had a positive effect on the perceived risk of the subject being exposed to future domestic violence (Panel B, Figure 4). Victims in the treatment group are 19.4 percentage points (46%) less likely to be in contact with the perpetrator of the initial incident (p = 0.018). Treatment group subjects report being 16.2 percentage points (44%) more willing to report a future incident (p = 0.105). There is a minimal and insignificant positive effect on victims to say that their personal safety has improved since the initial incident. Overall, the repeat victimization risk index suggests a significant decrease in risk for the treatment group relative to the control group (p = 0.034).

We also investigate changes relative to the initial incident in a variety of well-being measures (Panel C of Figure 4). We find consistent negative short-term effects of the intervention on a number of measures of well-being. The treatment group was 23.2 percentage points (46%) less likely to report an improved stress level since the incident (p = 0.007). We find negative effects on subject-reported mental health and quality of sleep (p = 0.204; p = 0.234). The overall index suggests that the intervention had a negative impact on the well-being of subjects in the weeks following the intervention (p = 0.102).

It should be noted that a short-run decrease in well-being, particularly as measured through stress, is not inconsistent with a decrease in victimization risk as measured here. For example, leaving an abusive partner, while likely reducing the risk of future abuse, may introduce new problems for the subject. An abusive partner, for example, may have a role in the household as a provider of income, and assist in productive household activities. Leaving such a partner is likely to introduce household finance problems, which, in the short-run, some subjects may find more stressful than living with an abusive partner. The negative effect on stress is consistent with findings in the psychological literature that report higher stress levels for victims that are in the process of leaving or have recently left a perpetrator.[37]

4.3 Programme effect on repeat police-reported domestic violence

In the previous section, we investigated the effect the intervention had on the perceived risk of future victimization using survey data. In this section, we expand on this using police data on repeat victimization and ask whether the intervention had an effect on future police-reported incidents.

We start by looking at the probability that at least one police-reported repeat incident is observed over this two year period. The estimated treatment effects are positive, but statistically insignificant (columns 1–2, Table 3). The treatment group is 2.3 percentage points more likely than the control group to have reported a repeat domestic incident; a 3.1% increase over the control group mean, but not significant at conventional levels ($p = 0.390$). Next, we look at the results for the number of reported domestic incidents (columns 2–3). The coefficients are small, positive but not statistically significant. Over a two year period, the treatment group reported 0.230 more domestic incidents ($p = 0.294$). Results are similar when we condition on at least observing one repeat incident over the two year period reducing the coefficient on the treatment effect slightly to 0.196 (columns 5–6, Table 3). This is a 5.4% increase over the control group mean of 3.582 domestic incidents.

Table 3 about here

The challenge in using future repeats to draw conclusions about the state of violence in the household is that treatment may impact reporting, as indicated in the victim survey. If this is the case, then we may expect the treatment group to report incidents that are

[37] Anderson and Saunders (2003) provide an overview of the literature.

less severe than they would absent the treatment.[38] We look at three measures reflecting the severity of the reported incidents.[39] These will allow us to examine whether subsequent police reports for DV differ in their quality by treatment status. It should be noted that if the treatment leads to an increase in reporting, then γ_1, from the Equation (1) regression of severity, cannot be interpreted as a treatment effect. Observed changes in incident severity may mechanically arise through the effect of treatment on the composition of the sample observed in a repeat incident.

The first measure is the risk assessment score provided by the officers who respond to the initial incident. This score is based on a standardised tool used across UK police forces to assess the risk of escalation in domestic violence situations.[40] A higher risk assessment score (out of a possible 20) means that the victim meets more of the criteria on which risk is assessed. We interpret a higher risk score as a more severe incident. The second measure that we look at is the probability that a victim is identified as high-risk by police following a repeat incident. A victim is identified as high risk if either they have a risk assessment score of fourteen or higher, or if they have had at least seven police-reported incidents over a 365-day period. The final measure of the severity that we consider is the arrest of a perpetrator by the responding officers, where we assume that incidents where the perpetrator is arrested are of a more severe nature.

Table 4 about here

The treatment group had a risk assessment score 0.576 points lower than the control group (Column 2 of Table 4). This estimate is marginally significant (p = 0.102), and represents a non-trivial magnitude of a 9.4% reduction compared to the control group mean.

[38]This assumes that the likelihood of reporting is increasing in incident severity.

[39]Severity measures are observed for the first five repeats in the first year following the intervention.

[40]This tool is formally known as the Domestic Abuse, Stalking and Honour-Based (DASH) violence assessment. It is based on a series of twenty yes/no questions that the responding officer asks victims. We provide an example of the DASH assessment tool in the supplementary Appendix G.

Results are similar for proportion of reported repeat incidents that are identified as high risk (the treatment group is 2.7 percentage points less likely to be reporting a high risk incident and arrests (arrest is 5.3 percentage points less likely for the treatment group), but these estimates are not statistically significant.

Estimates across all measures of severity are negative, indicating that reported incidents in the treated group are of lower severity, even though they are not significant at conventional levels. Given these results, we cannot rule out that the increased propensity to report future incidents lead to less severe cases being reported to police, in line with the results on the increased willingness to report from the victim survey. This would suggest that the positive, but small and statistically insignificant, effects on repeat incidents may be due to an increased propensity to report lower severity incidents.[41]

4.4 Programme effect on perpetrator arrest, charge, and sentencing

Finally, we consider outcomes that mediate one side of the relationship between statement provision and repeat incidents. Given the decrease in statement provision due to the intervention, one might be concerned that this also leads to a reduction in punitive actions taken against the perpetrator. We examine this possibility here for subsequent perpetrator arrest by police, charges by the Crown Prosecution Service, and sentencing by the courts. Table 5 reports the estimates.

Table 5 about here

For each outcome, we estimate a negative effect that is small in magnitude, and no estimate is statistically significant. Treatment is linked to a 1.0 percentage points reduction in arrest, a 0.6 percentage points reduction in the perpetrator being charged, and a 0.3

[41] However, it is possible that a reduction in incidents is concealed by a large increase in reporting of minor cases previously not reported to police.

percentage points reduction in sentencing of the perpetrator. These magnitudes correspond to a 3.8%, 4.5%, and 3.6% decrease relative to the respective outcomes. These results suggest that there was little effect of the reduction in statement provision on punitive outcomes against the perpetrator.

5 Discussion and interpretation of results

There are two overarching concerns that a programme such as the one in this intervention is trying to address. The first, and most important, is the long-term safety and well-being of victims of domestic violence. The second concern regards the most efficient use of public services available to victims of DV. Specifically, did the intervention lead to a better allocation of service use between police and non-police services?

Effect of the intervention on victim outcomes

Taken together, the results from the one-month survey indicate that the intervention had the expected effect on victims, in particular regarding engagement with specialist DV services and personal safety (Figure 4). Victims in the treatment group are more likely to have used health and non-health services one month following the initial incident and appear to be less exposed to repeat victimization, largely driven by reduced contact with the perpetrator. We also document that the intervention increased stress levels. This is consistent with the victims in the treatment group engaging with services and making changes in their personal circumstances, which ultimately may improve their safety but may also increase short-term stress levels.

Despite the positive effects on safety of victims documented using the survey results, we do not find an effect of the intervention on the longer-term quantity of police reported domestic incidents. One possible explanation for this is that the intervention did not actually lead to a change in household violence. This interpretation contrasts with recent work in the economics literature that emphasises the role of outside options in reducing household abuse (Aizer, 2010; Bobonis, González-Brenes, and Castro, 2013; Anderberg, Rainer, Wadsworth, and Wilson, 2016). By making support services easier to access, the victim's outside options away from the relationship are improved, and the threat of leaving the perpetrator is more salient (Farmer and Tiefenthaler 1996).

Exploring this explanation, one can speculate as to why improving support service access might not work. A possible explanation is that the services which are available do not address the specific needs of these repeat-victims. There is a small number of empirical studies that look at the effectiveness of individual support services and repeat violence. Stover, Meadows and Kaufman (2009) review the literature that looks at specific victim-support services, and conclude that there is little evidence that these services lead to a fall in rates of repeat victimization. For example, it is possible that the mix of available services simply does meet the needs of these victims. This contrasts nevertheless with our findings that the intervention lead to an increased uptake of specialist DV services documented in our survey results.

An alternative interpretation of the our results, is that the intervention reduced violence within the household, but also increased the victim's willingness to report a future incident, hence leading the quantity of repeat incidents to appear unaffected by the intervention. We explored this channel by looking at measures which reflect the severity of the repeat police callouts. The consistently lower measures of severity for treatment group victims, although not statistically different from zero, is consistent with this interpretation. These effects are also in line with the results from the victim survey, where we find that treated victims are more likely to report future incidents to police. The magnitude of the estimates on the severity of subsequent repeat incidents are worthy of further consideration: for treated victims, repeat incidents are at least 2.3 percentage points (3.1%) more likely to be reported, and the likelihood a reported incident is high-risk decreases by 2.7 percentage points (12.6%).

It would be reasonable to consider this as an improvement in welfare for repeat DV victims.

Our lack of power makes both of these interpretations highly speculative. However, we are confident that the intervention did not lead to a worsening of safety for victims of DV. This is in contrast to previous studies (e.g. Davis, Weisburd and Hamilton 2010) and likely attributable to the careful design of the intervention ensuring victim contact was made without perpetrator involvement.

Productivity in the use of police services

Above we argue that the intervention did not lead to a deterioration of the safety of victims of DV, and possibly led to an improvement. Here we discuss the results showing a significant decrease in statement provision, but no significant change in perpetrator arrest or charge. These results might be surprising, given the strong positive correlation between victim statement provision and perpetrator arrest (Figure 1). Once a statement is made, it requires investigative efforts on the part of police to determine whether to build a case for prosecution. In this way, the correlation between statements and arrests, charges or sentencing provides a measure of productivity. With this interpretation, the results are consistent with the intervention leading to a non-random change in statement provision; victims who forgo statement provision due to the treatment have, on average, a lower statement productivity than other victims.[42]

In Appendix B we introduce a formal framework, defining victim types according to whether or not they change their statement provision upon receiving the treatment. Under the assumptions that a) the probability of a perpetrator arrest or charge is weakly increasing in statement provision, and b) conditional on statement provision the intervention is uncorrelated with perpetrator arrest, a decrease in statement without a change in arrest means that police services are being used more efficiently. Specifically, for victims who forgo statement

[42]We say that a statement A is more *productive* than statement B if the probability of A leading to an arrest and other actions by police is higher than B.

provision due to treatment, either their statement is less effective in resulting in criminal justice outcomes than for those who make statements due to treatment, or the probability their statements lead to a criminal justice outcome is close to zero.

We explore this interpretation further by comparing changes in outcomes for those victims who provided a statement to police. Changes in these outcomes are consistent with the intervention having affected the composition of the statement providers, as we argue in Appendix B.

Table 6 about here

Statement retraction is a plausible channel through which the findings presented in sections 4.1 and 4.4 may arise. If a victim retracts his or her statement, it is inadmissible as evidence against the perpetrator. We find a significant decrease in the retraction of statements that are provided after the initial callout, (Statements at $t > 0$, Panel A, Table 6). This suggests that statements made after the initial callout are 10.3 percentage points less likely to be retracted in the treatment group than they are in the control group (p = 0.013). Considering retraction of these statements for the control group is 12.2%, this is an 84% reduction, leaving treatment group statement retractions at only 1.9%. Furthermore, we do not see a similar reduction for statements made at the initial callout (Statements at $t = 0$, Panel A, Table 6), for which group differences are smaller in magnitude and are not statistically significant.

The correlation between statement and perpetrator arrest is 10.5 percentage points higher for the treatment group relative to the control group (*Any statement*, Panel B, Table 6). Consistent with previous findings, this is due to a 15.6 percentage points increase (p = 0.063) in the correlation for statements made after the initial callout (Statement at $t > 0$, Panel A, Table 6). We find no significant difference between the treatment group and control group in this correlation for statements made at the initial callout.

We also look at differences of treatment and control in the correlation between statement provision and perpetrator charges and sentencing (panels C and D, Table 6). The estimated differences between treatment and control are similar in size compared to the estimates for arrest, but not statistically different from zero.

This result indicates an increase in the correlation between statements and arrest following the intervention, which we interpret as an increase in the productivity of police services. Note that this arises purely from the composition of statement-makers in treatment and control.

6 Conclusions

The experimental evidence on improving access to domestic violence support services presented here leads to three key results. First, improving support service access for repeat victims led to a 22% reduction in statements to police. This suggests that, on the margin, victims in our subject pool treat police and non-police services as substitutes (see Appendix A). This is generally important; when service users view two different services as imperfect substitutes, barriers to access in one service (non-police services) may have a negative externality on the other service (police services).

Second, despite this decrease in statement provision, we do not see a corresponding decrease in punitive actions against perpetrators. We argue that this is consistent with the intervention having led to a more efficient use of police services by victims. For example, treatment group statements are 84% less likely to be withdrawn than control group statements. Therefore, making non-police services easier for victims to access will alleviate some of the pressure on scarce police services. A back-of-the-envelope calculation (assuming a conservative 10 investigative hours per statement)[43] suggests that this intervention freed up

[43]Because of the large variation in the time spent on further investigation of DV cases, it is difficult to quantify the average number of hours spent by the DV investigative team. Leicestershire police provided us

550 hours of police time to be allocated elsewhere.[44]

Third, unlike previous studies, we do not find evidence of an increase in household violence following the intervention. This is significant; the importance of the efficiency conclusion is moot if it comes at the expense of victim well-being. In fact, this study offers evidence that improving access to support services may improve outcomes for victims overall as evidenced by the lower risk of victimization from the victim survey.

The findings have general implications for the provision of public services, when individuals decide between different alternative services for which ease of access differs. Several relevant examples involve public health services. For example, the choice of seeking help for an acute health problem using general practitioner services versus emergency services and differences in ease of access based on the provision on weekdays compared to the weekend.

This study also highlights the difficulty in designing policy to address persistent domestic violence. Despite a significant improvement in the accessibility of domestic violence support services, we find little change in future victimization. These results serve as reminder of the exceptional complexity of the underlying root causes of DV and the limitations of interventions in breaking the persistent cycle of repeat victimization.

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with a benchmark based on their professional experience of an average of 20 hours investigative time per further investigation.

[44] An important limitation of this study is our imperfect view of non-police services. We cannot calculate service-specific effects of the treatment. Therefore, it is not possible to talk about the general distributional efficiency across all public services from a cost-benefit perspective.

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Table 1: Descriptive statistics

	Treatment	Control	Difference	N
A. Victim characteristics				
Female	0.888	0.857	0.031	1015
			(0.021)	
Age	33.929	34.984	-1.055	1015
			(0.768)	
White	0.844	0.835	0.008	991
			(0.023)	
Domestic cases (365 days)	2.330	2.259	0.071	1015
			(0.096)	
Registered domestic cases	11.720	10.721	0.999	1015
			(0.684)	
Risk assessment score	1.275	1.280	-0.005	955
			(0.035)	
B. Perpetrator characteristics				
Female	0.139	0.138	0.001	1004
			(0.022)	
Age	33.028	33.392	-0.364	1004
			(0.744)	
White	0.803	0.819	-0.016	925
			(0.026)	
Domestic cases (365 days)	2.226	2.248	-0.022	1004
			(0.124)	
Registered domestic cases	11.891	10.727	1.163	1004
			(0.650)	
C. Household characteristics				
Same victim and perpetrator†	0.422	0.471	-0.049	1004
			(0.031)	
Intimate partner	0.761	0.798	-0.036	983
			(0.026)	
Cohabitation	0.532	0.593	-0.060	982
			(0.032)	
Children in the household	0.586	0.570	0.016	1009
			(0.031)	
Number of children‡	1.923	1.983	-0.060	583
			(0.082)	

Notes: This table reports variable means for cases in the *treatment* and *control* groups. Column *difference* reports the difference in group means; the corresponding standard error on difference is reported in parenthesis.

†Binary variable equal to 1 if the same perpetrator is observed for the same victim, 0 otherwise.

‡Number of children conditional on having at least one child.

Table 2: Treatment effect for victim providing a statement to police

	Treatment effects			Falsification test	
	(1)	(2)	(3)	(4) t=0	(5) t>0
Treatment	-0.062	-0.065		-0.011	-0.062
	(0.028)	(0.027)		(0.021)	(0.024)
Engagement			-0.126		
			(0.050)		
Victim female		-0.004	-0.002	-0.006	0.004
		(0.045)	(0.043)	(0.036)	(0.039)
Perp female		-0.055	-0.065	-0.036	-0.026
		(0.044)	(0.043)	(0.035)	(0.038)
Victim white		0.087	0.083	0.059	0.040
		(0.049)	(0.048)	(0.039)	(0.046)
Perp white		-0.081	-0.087	-0.054	-0.046
		(0.047)	(0.046)	(0.038)	(0.045)
Cohabitation		0.121	0.124	-0.021	0.149
		(0.028)	(0.027)	(0.023)	(0.025)
Child in household		0.004	0.008	0.023	-0.016
		(0.028)	(0.028)	(0.023)	(0.025)
Previous DV		-0.004	-0.005	-0.002	-0.002
		(0.009)	(0.009)	(0.007)	(0.008)
Risk assessment		0.277	0.283	0.180	0.175
		(0.026)	(0.025)	(0.021)	(0.026)
Constant	0.299	0.466	0.455	-0.244	0.603
	(0.020)	(0.418)	(0.408)	(0.335)	(0.348)
N	1015	1015	1015	1015	878
R^2	0.005	0.227	0.197	0.167	0.210
Controls ($p\,value$)†		0.000	0.000	0.000	0.000

Notes: This table reports linear probability estimates for a binary outcome, equal to 1 if the person identified as "victim" provided police with a statement, and 0 otherwise. Columns (1) and (2) report OLS estimates, unconditional and conditioning on the reported control variables. Column (3) reports two-stage least square estimates, where assignment to the treatment group as an instrument for program engagement (coefficient is 0.512, excluded F = 495, excluded R^2=0.348). In Column (4), the outcome is equal to 1 if a statement is provided within 24 hours of the initial police callout, and 0 otherwise. In Column (5), the sample excludes cases in which a statement is provided within 24 hours of the initial police callout. Estimates in columns (2)–(5) also include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing variables. Robust standard errors are reported in parentheses.

†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 3: Repeat police-reported D.V., two-years following initial incident

	Repeats ≥ 1		Total repeats		Total repeats, conditional on ≥ 1	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.030	0.023	0.270	0.230	0.209	0.196
	(0.027)	(0.027)	(0.213)	(0.220)	(0.247)	(0.254)
Victim female		0.085		0.304		0.064
		(0.050)		(0.358)		(0.451)
Perp female		-0.054		-0.257		0.043
		(0.047)		(0.337)		(0.403)
Victim white		0.055		-0.002		-0.218
		(0.047)		(0.447)		(0.481)
Perp white		0.029		0.611		0.691
		(0.046)		(0.412)		(0.443)
Cohabitation		-0.052		-0.705		-0.455
		(0.028)		(0.242)		(0.281)
Child in household		0.036		-0.160		-0.442
		(0.030)		(0.263)		(0.319)
Previous DV		0.014		0.257		0.236
		(0.009)		(0.079)		(0.087)
Risk assessment		-0.028		0.005		0.109
		(0.028)		(0.213)		(0.246)
Constant	0.749	0.962	2.681	2.305	3.582	0.860
	(0.019)	(0.101)	(0.141)	(0.776)	(0.177)	(1.206)
N	1015	1015	1015	1015	775	775
R^2	0.001	0.114	0.002	0.098	0.001	0.128
Controls ($p\,value$)†		0.010		0.003		0.0384

Notes: This table reports estimates for the regression repeat police-reported domestic violence outcomes on treatment status. A repeat is defined as an incident recorded by the police involving the person identified as the "victim" in the initial incident. All outcomes reflect a period of two-years from the time of the initial call-out. The outcome in columns (1) and (2) is a binary variable equal to 1 if at least one repeat is observed, and 0 otherwise. The outcome in columns (3) and (4) is the total number of repeat police callouts for domestic violence. The outcome in columns (5) and (6) is the total number of repeat police callouts for domestic violence, conditional on at least one. Estimates in columns (2), (4), and (6) include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing control variables. Robust standard errors are reported in parentheses.

†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 4: Incident severity at repeat police callouts

	Risk level		High risk		Perpetrator arrested	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.410	-0.576	-0.014	-0.027	-0.031	-0.053
	(0.358)	(0.351)	(0.035)	(0.037)	(0.039)	(0.042)
Victim female		0.188		-0.045		-0.003
		(0.552)		(0.078)		(0.074)
Perp female		-1.781		-0.106		-0.086
		(0.560)		(0.064)		(0.066)
Victim white		-0.735		-0.236		-0.091
		(0.631)		(0.071)		(0.073)
Perp white		0.565		0.163		0.053
		(0.561)		(0.060)		(0.071)
Cohabitation		0.456		0.007		0.028
		(0.376)		(0.039)		(0.045)
Child in household		0.948		-0.026		0.071
		(0.391)		(0.042)		(0.046)
Previous DV		-0.128		0.015		0.013
		(0.109)		(0.012)		(0.013)
Risk assessment		1.897		0.108		0.078
		(0.394)		(0.043)		(0.040)
Constant	6.039	5.613	0.215	0.017	0.457	-0.076
	(0.255)	(1.930)	(0.025)	(0.211)	(0.028)	(0.154)
N	522	522	535	535	639	639
R^2	0.003	0.291	0.000	0.176	0.001	0.143
Controls ($p\,value$)†		0.000		0.001		0.0389

Notes: This table reports estimates for the regression of outcomes reflecting the severity of repeat domestic incidents. The dependant variable in columns (1) and (2) is the average DASH risk assessment score left by the responding officers across all repeats. The dependant variable in columns (3) and (4) is a binary variable equal to 1 if the victim is identified as high-risk during any repeat, and 0 otherwise. The dependant variable in columns (5) and (6) is a binary variable equal to 1 if the perpetrator was arrested by the police during at least one repeat callout, and 0 otherwise. Estimates in columns (2), (4), and (6) include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing control variables. Robust standard errors are reported in parentheses.
†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 5: Treatment effect for perpetrator arrest, arrest with charges and conviction

	Arrested		Charged		Sentenced	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.016	-0.010	-0.009	-0.006	-0.009	-0.003
	(0.027)	(0.027)	(0.021)	(0.022)	(0.017)	(0.018)
Victim female		0.047		-0.010		-0.038
		(0.041)		(0.035)		(0.029)
Perp female		-0.098		-0.038		-0.064
		(0.039)		(0.031)		(0.022)
Victim white		0.039		0.036		0.011
		(0.051)		(0.040)		(0.033)
Perp white		-0.093		-0.032		-0.012
		(0.052)		(0.039)		(0.033)
Cohabitation		0.094		0.077		0.058
		(0.028)		(0.021)		(0.017)
Child in household		-0.017		0.014		0.015
		(0.029)		(0.022)		(0.017)
Previous DV		-0.004		-0.004		-0.005
		(0.009)		(0.008)		(0.006)
Risk assessment		0.241		0.153		0.087
		(0.029)		(0.027)		(0.022)
Constant	0.263	-0.291	0.133	-0.191	0.083	-0.083
	(0.020)	(0.095)	(0.015)	(0.081)	(0.012)	(0.067)
N	1014	1014	1015	1015	1015	1015
R^2	0.000	0.176	0.000	0.137	0.000	0.107
Controls ($p\,value$)†		0.000		0.000		0.000

Notes: This table reports linear probability estimates for three binary outcomes, all referring to the initial callout case. Outcome *Arrest* is equal to 1 if the person identified as "perpetrator" is arrested by police, and 0 otherwise. Outcome *Charged* is equal to 1 if the person identified as "perpetrator" is charged by the Crown Prosecution Service, and 0 otherwise. Outcome *Sentenced* is equal to 1 if the person identified as "perpetrator" is convicted (fine, probation, or prison sentence) by the judiciary, and 0 otherwise. Columns report estimates of the intention to treat, unconditional and conditioning on the reported control variables. Estimates in columns (2), (4), and (6) include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing control variables. Robust standard errors are reported in parentheses.

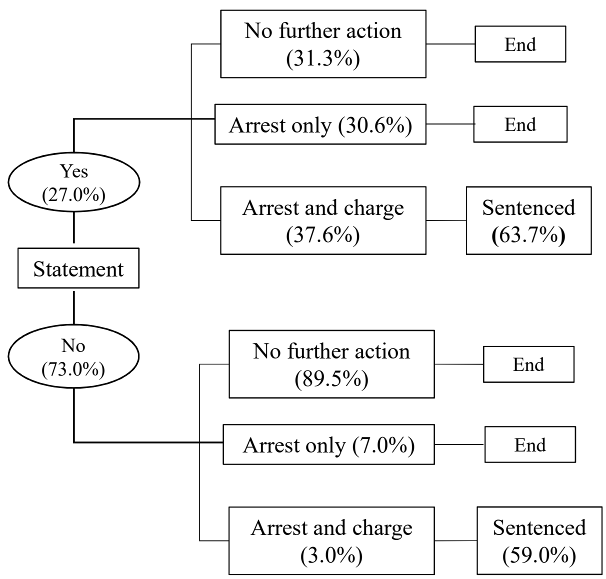
†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 6: Outcomes conditioning on statement provided by victim

	Treatment	Control	Difference
A. Statement retracted by victim			
Any statement	0.140	0.192	-0.052
			(0.045)
Statement at $t = 0$	0.235	0.275	-0.040
			(0.075)
Statement at $t > 0$	0.019	0.122	-0.103
			(0.041)
B. Perpetrator arrested by the police			
Any statement	0.744	0.636	0.108
			(0.056)
Statement at $t = 0$	0.765	0.725	0.040
			(0.075)
Statement at $t > 0$	0.717	0.561	0.156
			(0.083)
C. Perpetrator charged by the CPS			
Any statement	0.397	0.371	0.026
			(0.060)
Statement at $t = 0$	0.382	0.406	-0.023
			(0.084)
Statement at $t > 0$	0.415	0.341	0.074
			(0.086)
D. Perpetrator sentenced in court			
Any statement	0.240	0.245	-0.005
			(0.052)
Statement at $t = 0$	0.221	0.290	-0.069
			(0.075)
Statement at $t > 0$	0.264	0.207	0.057
			(0.076)

Notes: This table depicts the difference between treatment and control group for perpetrator arrests, charges laid against the perpetrator and victim retraction of statements, conditioning on the provision of a statement by victim. All outcomes refer to the initial callout case. Columns labelled *treatment* and *control* report the mean for each conditional outcome for the treatment and control groups; column difference reports the difference between these two values. Rows labelled *Statements at* $t = 0$ condition on statement provided within the first 24 hours following the initial police visit, rows labeled *Statements at* $t > 0$ condition on statement provided after 24 hours period. $N = 272$, with 137 for statement at $t = 0$ and 135 for statement at $t > 0$. Robust standard errors for differences reported in parenthesis.

Figure 1: Tree representing the life of a case

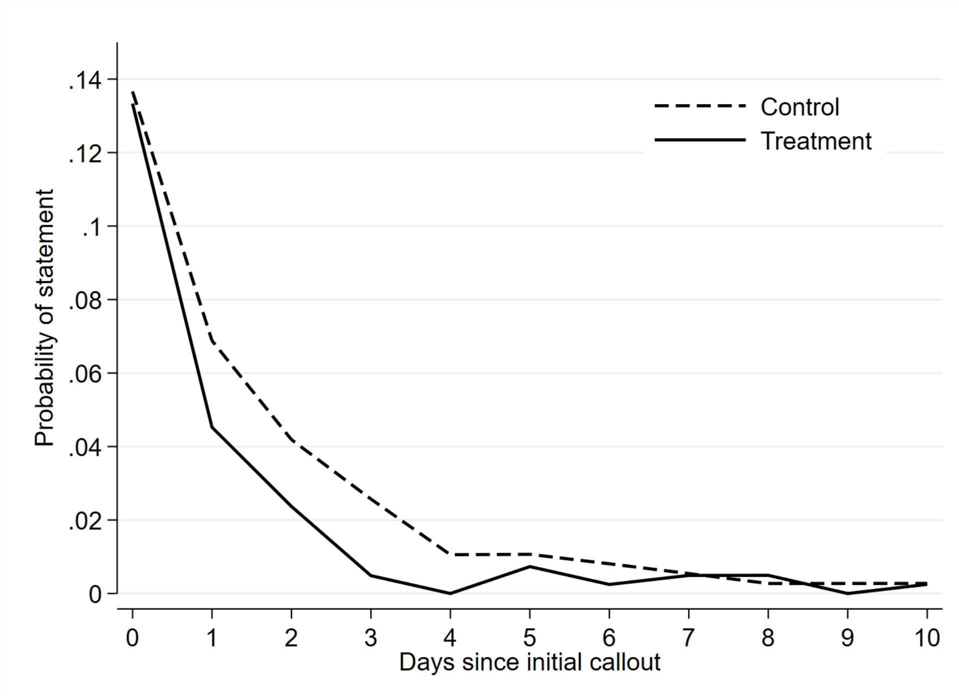


Notes: Percentages correspond to the probability of the event conditional on position in the tree, based on subject pool data. *End* nodes indicate that no further action is taken with respect to the case.

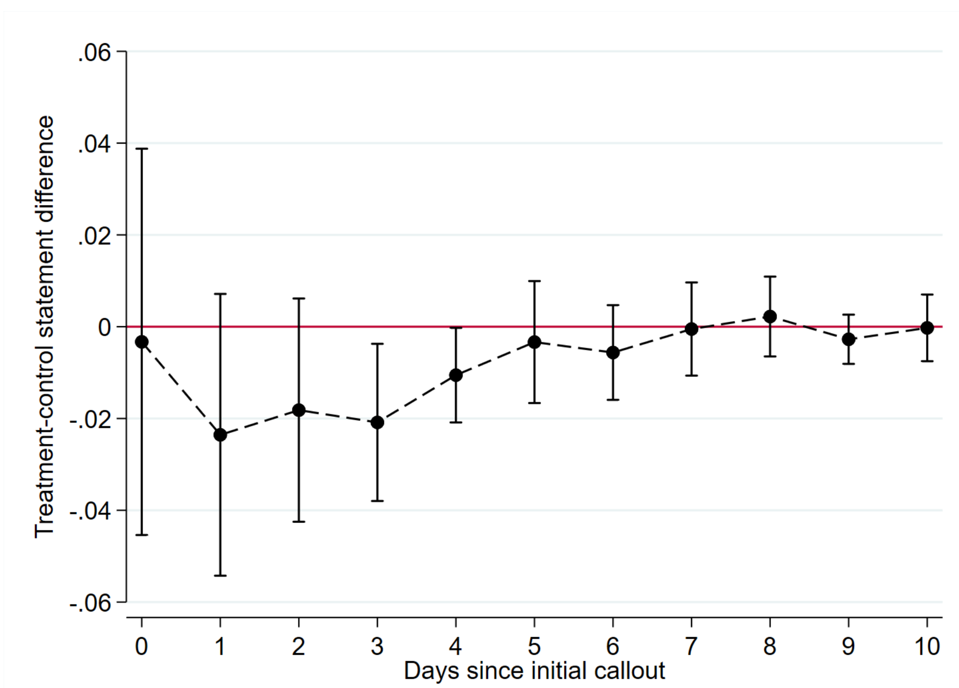
Figure 2: Leicestershire Police Force area

Notes: Map sections indicate counties for England. Area in red is the Leicester Police Force area.

Figure 3: Probability of victim statement by days since initial callout and treatment



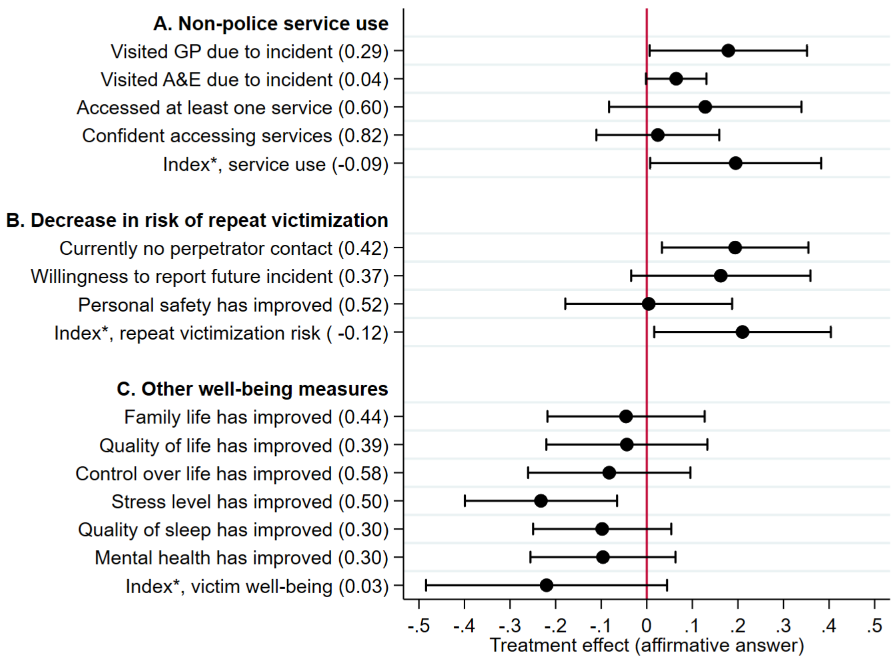
(a) Probability of statement, conditional on no previous



(b) Treatment-control group difference in the probability of statement provision, by days since initial callout

Notes: These figures show (a) the probability a statement is provided by days since the initial callout, conditional on not having not already provided a statement, and (b) the corresponding treatment-control group difference by day—bars show 95% confidence intervals on difference.

Figure 4: Non-police services and victim well-being, one-month survey



Notes: This figure reports results from selected questions on the one-month victim follow up survey. The complete survey questionnaire is available from the authors. Outcomes for each question are transformed into binary variables equal to 1 if the answer is affirmative, and 0 otherwise. Markers show the intention to treat effect (ITT); bars reflect the corresponding 95% confidence interval. Mean outcomes for the control group are reported in parenthesis. $N = 214$, with 105 in treatment and 109 in control. ITT estimates condition on characteristics X_i, described in Section 4 of the main text. *Services* are defined as any non-police services, excluding health services (GP or A&E), available specifically for domestic violence.
*Index variables are calculated following Anderson (2008).