Improving access to support services for victims of domestic violence: demand for services and victim outcomes*

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Abstract

We conducted a randomised controlled trial of an intervention designed to assist victims of domestic violence in accessing non-police support services. The intervention led to a 22% decrease in the provision of statements by victims to police, indicating a reduction in the use of subsequent police services. Despite the reduction in the use of police services, we do not find a significant change in perpetrator arrests and convictions or in reported future violence. Survey responses provide evidence of an increase in non-police service use, a reduction in future victimisation risk, but also a potential decrease in short-run well-being.

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

JEL Codes: I18, J12, H75

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1 Introduction

Domestic violence (DV)[1] is a problem of first-order importance across the world, with recent estimates suggesting that almost one-third of women are subject to violence from their intimate partner at one point in their lives (World Health Organization, 2021). 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; Office for National Statistics [ONS], 2019). These numbers do not include the many more cases never reported to the police. Of those cases reported to police, less than six per cent 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 contributes to the overall number of police reports. This situation comes amid the availability of several public and private non-profit organisations that help victims of DV address long-term abuse by providing services and opportunities to change personal circumstances.[2] A 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 randomised evidence on whether improving victim access to existing services will lead to better DV outcomes. We consider several margins along which

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

[2] These services 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, and other forms of support to leave a perpetrator.

[3] Victims' lack of information was highlighted in a report on the policing of DV by the UK police watchdog Her Majesty's Inspectorate of Constabulary and Fire & Rescue Services(HMIC, 2014) as well as Fugate et al. (2005), who show that information barriers are a significant deterrent for victims of DV in the United States.

we may expect to see better outcomes, including repeat violence, victim well-being, and changes in the use of police and non-police services. We worked with a large UK police force to implement a randomised controlled trial (RCT) of an intervention specifically designed to improve access to non-police support services. The trial focused 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 randomisation 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 several sources. The dataset includes information from local police administrative records on reported DV outcomes over a two year period, information from the UK Police National Database on perpetrator convictions and sentencing, and information from a victim follow-up survey capturing victim-centred outcomes such as the future risk of violence and other measures of well-being. These extraordinarily rich data 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 significant findings emerge from this study. First, treatment group victims were significantly less likely to provide police with a statement. This is important, as victim statements are a critical, and often the only, piece of evidence in building a case against a perpetrator.[4] 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 statement provision on the day police visit the house-

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

hold in response to the reported violence (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 statement provision, 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 correlation between the provision of a statement and criminal sanctions against a perpetrator is positive and strong. This suggests that for victims who make a statement in control, but not in treatment, the effect of their statement on criminal sanctions is low relative to other victims. One plausible explanation for this is that these victims are more likely to retract their statement, making their statement inadmissible as police evidence. 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 utilisation 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 victimisation, 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 reported treatment group cases are less severe. For example, we find that reported scores from a standardized tool for assessing violence escalation risk by responding officers are 9.5% lower for the treatment group cases than for the control group cases. 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 the willingness of victims to report less severe 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 the police. We also find that the treatment group has an overall lower risk of repeat victimisation. 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 directly observe violence in the household, we must attempt to infer changes based on reporting behaviour and our survey response. Our results are consistent with the intervention leading to a rise 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 literature studying the role of public policy to improve DV outcomes. In particular, several studies use 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 (Casey, et al, 2007; Davis and Taylor, 1997; Davis, Weisburd and Hamilton, 2010; Hovell, Seid and Liles, 2006; Stover, Berkman, Desai and Marans, 2010; Stover, Poole and Marans, 2009). 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 several significant 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 specialists 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 led 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 covering a two-year period. These records provide a standardised measure of the severity of each incident. This standardisation allows us to answer the question of whether the intervention reduced repeat incidents or changed reporting patterns by assessing the severity of the subsequent incidents attended by police.

This paper both contributes to and is informed by a second strand of literature studying barriers to public service uptake and interventions to overcome those barriers. The first set of papers focuses on information interventions in a variety of experimental settings. For example, studies have examined how simplifying information on public school performance leads parents to select higher-performing schools for their children (Hastings and Weinstein, 2008). Research has also addressed how the provision of information on the cost and benefits of education changes students' intention to stay in non-compulsory education (McGuigan, McNally, and Wyness, 2016) as well as increasing enrolment in post-secondary schooling for unemployment insurance recipients (Barr and Turner, 2018). In addition, researchers have examined how personalised prescription drug plan information makes Medicare users more likely to switch to lower-cost plans (Kling, Mullainathan, Shafir, Vermeulen, and Wrobel, 2012).

Another set of papers focuses on interventions that assist persons in accessing services. For example, Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) conclude that assistance in filling out complex college aid applications leads to an increase in college enrolment. Research has also shown that removing assistance in completing disability applications from closures of field offices led to a persistent decline in the number of disability recipients (Deshpande and Li, 2019). In practice, information and assistance often significantly interact. Finkelstein and Notowigdigdo (2019) study the effects of providing information and assistance aimed at increasing the take-up of SNAP benefits in Pennsylvania. They find that information alone increased enrolment in SNAP by 5 percentage points, but when combined with assistance, enrolment increased an additional 7 percentage points.

These studies demonstrate that relatively simple information and assistance interventions can help overcome bureaucratic barriers or costs to obtaining and processing information. These interventions reduce distortions in choice compared to what is selected without administrative and financial 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 utilising police services with which they already interact. Unlike previous studies, we consider service users who choose among different, non-exclusive services. Potential service users can and do choose more than one service. Services are not explicitly priced; therefore, users do not internalise 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.[5] 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 to policing. In the UK, DV and abuse account for approximately 11% of all crimes reported to police,[6] creating substantial service demands 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, namely focusing on households with previous police-reported domestic incidents. This study does not attempt to draw any conclusions about the use or effectiveness of public support services for households going through their first police-reported DV incident, or households with unreported DV. Moreover, the households in our subject pool received treatment at most once. We cannot conclude 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 the RCT design and implementation, followed by data sources and collection. The main results of the paper are presented in Section 4. A discussion and interpretation of results is offered in Section 5, and an analysis

[5] 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).

[6] This number is based on official statistics provided by ONS (2018a, 2018b).

of the intervention cost appears in Section 6. Section 7 summarises the findings and places 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)[7], 24 different agencies provided various DV support activities at the time of the intervention. In Appendix A, we provide detailed information about the available services, including a table summarising 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 provide to all victims following a domestic incident.

Several barriers make it costly for victims to access these services.[8] 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 or language barriers. Fourth, to ensure the safety of users or restrict the use of scarce resources, service providers often establish burdensome formal procedures, such as the requirement for a gatekeeper referral to access some houses of refuge.

While we do not explicitly distinguish between sources of barriers, they are widely recog-

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

[8] In Appendix B.1 we provide a stylised conceptual framework to guide our thinking about the relationship between access barriers and the choice between various DV services.

nised as an impediment to service uptake.[9] The intervention we study is specifically designed to help victims of DV overcome these barriers by providing information on existing services, signposting victims to the appropriate service, helping them overcome psychological and language barriers and providing referrals to these services.

Police services available to victims of DV

We refer to police attending a DV 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 is to defuse the immediate and potentially volatile situation and ensure the safety of all individuals involved.[10] The second task is to collect evidence at the initial callout to determine whether to initiate further investigations for pursuing criminal sanctions against the perpetrator. Evidence can be direct, such as police observing and recording a physical assault through body-worn cameras. More often, however, evidence is indirect in the form of statements made by witnesses, including any victims. A statement is a recorded recollection of events by a witness that can be used as evidence in court.

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 formal charges against a perpetrator. The CPS decides whether

[9] *Her Majesty's Inspectorate of Constabulary* (HMIC, 2014) reports anecdotal evidence based on subject interviews that victims of DV felt that they did not know where to turn for help after an initial police callout. These barriers to services are not unique to the UK context. For example, in the United States and Canada, Jaffe et al. (2002) show that 'women reported feeling let down and confused by the [community and social services support] process'. The authors 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 a lack of information, are a significant explanation for whether victims contact social and counselling services. However, these barriers are not important for explaining why victims contact police services.

[10] Police have the power to arrest and temporarily detain a perpetrator for up to 24 hours solely for this purpose. This arrest may be made independent of the victim's preferences. After 24 hours, either formal charges must be laid or the perpetrator must be released.

to charge the perpetrator to pursue criminal sanctions on the strength of the available evidence.[11]

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. In the absence of other witnesses, a victim statement is used both for charging a perpetrator and as evidence in court proceedings, giving it a key role in prosecuting perpetrators of DV. 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 retract the statement at any time until the CPS decides to pursue charges.[12] In our data, 17.0% of all statements are retracted. If this retraction occurs, the statement cannot be used as evidence in the case against the perpetrator, and often charges against the perpetrator will be dropped.[13] Many reasons motivate the retraction of statements, including a fear of repercussions by the perpetrator or other family members, lack of information on the criminal process, fear about immigration status, and remorse expressed by the perpetrator (CPS, 2021; McGuire, Evans and Kane, 2021; Robinson and Cook, 2006). Aizer and Dal Bó (2009) provide evidence from no-drop policies in the United States that statement retraction in DV is consistent with a model of

[11]Evidence of this screening process is found in the 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 finding is consistent with the role of the CPS in filtering cases that proceed to the courts based on the strength of the evidence.

[12]Once the CPS has decided to pursue charges, victims can add to, but may not be able to retract, their statement (CPS, 2021).

[13]The charity *Rights of Women* advises victims regarding the provision of a victim statement in DV cases that 'Without a witness statement from you, it is unlikely that the police will continue'. (Rights of Women, 2013).

time-inconsistent preferences, in which a victim's value from prosecuting the perpetrator is strongest shortly after the initial callout but declines over time. A belief in time inconsistent preferences also appears to underlie the policy studied by Ford (1983), in which US prosecutors impose a three-day cooling-off period to allow the victim to 'assess her options'.

The correlation between the provision of a victim statement and perpetrator charges and arrests is strong (see Figure 1). For our data, 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%. In the 272 cases where a statement was provided, the perpetrator was arrested in 68.2% and charged in 37.6%. These data, however, do not reveal anything about the causal effect of statements on arrests and charges. Victims may choose to make a statement based on their subjective expectation of the probability of an arrest. The correlation does provide evidence of the significance of statement provision in pursuing punitive action against a perpetrator.

3 Experimental design and data

We conducted an RCT in the English county of Leicestershire jointly with the Leicestershire Police Force and the three governing authorities in Leicestershire County.[14] Leicestershire (see Figure 2 for map) covers a population of approximately one million people, and the Leicestershire police force 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.[15].

Figure 2 about here

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

[15]The experiment and the data collection received approval from an Internal Review Board at the University of Leicester. We provide details on the application in Appendix section C.

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 assigning treatment.

After responding to a domestic incident, officers record a domestic incident report in the Leicestershire Police database. Our automated application performed a daily scheduled search for all newly recorded incidents. The recovered domestic incident cases must have met several conditions for inclusion in the subject pool: (1) the report was filed as a *domestic incident*; (2) in the previous 365 days, the victim had shown up in at least three and fewer than seven DV reports (including the current one);[16] (3) the victim was not previously in the subject pool (in either the treatment or control group); and (4) responding officers did not recommend the victim for a pre-existing intervention known as a *Multi Agency Risk Assessment Conference* (MARAC).[17] 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 50 reported domestic incidents were recorded daily with seven, on average, qualifying for the subject pool. To examine statements and conduct 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.[18] A few cases did not have information on all control variables. For the regression analysis, these missing

[16] The initial interest of this intervention was to assist victims of repeated DV. The minimum of three offences was based on the predicted capacity constraints of the trial. If more than seven DV incidents occurred in the households, the case was potentially referred to a separate pre-existing intervention as a standard procedure.

[17] MARACs are used UK-wide. During a MARAC, information on the highest-risk domestic abuse cases is shared between representatives of police services, health care, child protection, housing practitioners, probation and other DV specialists from the statutory and voluntary sectors. These stakeholders discuss options for a coordinated action plan to increase victim safety.

[18] This redaction would happen in a situation where individuals in the case are under investigation for a serious offence such as sexual assault involving a minor.

values are given a value of 0, and a variable-specific dummy will be used to indicate the missing values.[19] 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 A). Victims could contact the services on the pamphlet at any time. Victims were also invited to provide a statement to police any time during or after the initial callout.

3.3 Treatment

A treatment group case was assigned to a caseworker the morning following the recording of the case 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 working with DV support services in Leicestershire and had specialised knowledge of the various local services available and how to access them. The caseworkers also received training specific to the service provided through the intervention in this study.[20] 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[21] of the

[19] Reported results are robust to the exclusion of missing variables from the analysis.

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

[21] While caseworkers were on duty and attempted to make contact on Saturdays, subjects of incidents

initial police report. Once contact was made, the caseworker described the public support services locally available to the subject. If the subject desired to access a specific support service, the caseworker assisted in initiating access. This assistance included organising 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 subject expressed an interest to leave the perpetrator, the caseworker also assisted in preparing an escape plan. The intervention ended when either the subject declined to participate in the intervention or 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 subjects within a short period (24 hours) after the police report of the incident was filed. Second, caseworkers had access to all police information about victims and perpetrators, including historical police records. Third, subjects were informed of available non-police services, and if they wished to move forward, caseworkers assisted with accessing these services.

We define a subject 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 510 subjects assigned to the treatment group, 249 (49%) of treatment group subjects did not engage.[22] Of these attempts, 143 victims were contacted by a caseworker by telephone but refused both phone-based assistance and a face-to-face meeting. For the remaining 106 subjects, caseworkers were

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.

[22]A maximum of five attempts made at different times of day across five days, were made to contact victims by phone.

unable to make contact given the available contact information.[23]

Among subjects whom the caseworkers were able to contact, the engagement rate was 65% (we explore characteristics of those who engage in the following Section 3.7 and in Appendix F.2). Considering that caseworkers 'cold-called' the subjects, this is a notable take-up rate and similar to the engagement rate for other assistance interventions such as those studied in Finkelstein and Notowidigdo (2019) and Bettinger et al. (2012)[24]. Of the 261 subjects 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% occurring within three days. In all, 33% of home visits occurred after three days but within a week, and the remaining 13% took place more than one week after the initial callout.[25]

3.4 Internal validity

Several design features of the trial safeguard the internal validity of the study. Most importantly, all assignments to the treatment and control groups were automated and randomised. Unlike previous RCTs of similar second-responder interventions (Davis and Taylor, 1997; Davis, Weisburd, and Hamilton, 2010), caseworkers or police officers could not override

[23]For the subjects' safety, the caller ID was not displayed, which may have led to some subjects not answering the call.

[24]Similar to our intervention, the intervention in Finkelstein and Notowidigdo (2019) assisted eligible individuals with the application process by telephone, achieving an engagement rate, conditional on calling, of 60%. Bettinger et al. (2012), who provide application assistance in person, achieve an engagement rate of almost 70%. This contrasts with much lower response rates in interventions providing information via a letter only. Bhargava and Manoli (2012) find an overall 25% response rate in an EITC benefits experiment, while Barr and Turner (2016) report a response rate between 2–3% to letters sent informing individuals who recently experience job loss, on opportunities from postsecondary programmes. While the type of intervention and context certainly are an important factor for the response rates, that assistance was offered directly from a real person in the former interventions probably also plays an important role.

[25]In Appendix B.3, we provide and test an alternative rationalisation 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 the provision of statements. We show that the data do not support this rationalisation.

assignment to treatment.[26]

The timing of the 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 the treatment assignment. Furthermore, this procedure provides a falsification test, which we exploit in Section 4.1, as statements made during the initial callout cannot be influenced by the 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 unauthorised 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.

Leicestershire Police Database

We matched cases in the subject pool with Leicestershire Police administrative records, from a number of internal databases (detailed in Appendix D), using a unique crime reference number. The administrative records from these databases provide information on the initial incident (date, time, location, attending police officers, provision of a statement by the 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.

[26] 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 unaware of the intervention during the trial.

For each reported case, responding officers assessed the risk of violence escalating using a tool, standardised across all UK police forces, known as domestic abuse, stalking and honour-based violence (DASH).[27] We collected the risk assessment score of the responding officer, taking values of 1 for the lowest risk and 3 for the highest risk. We also collected the raw DASH score reflecting the total number of risk criteria (out of a possible 20), which the responding officer recorded as affirmative. A higher DASH score means that the victim meets more of the criteria on which escalation risk is assessed. We interpret a higher DASH score as indicative of a more severe incident.

We collected information on reported incidents involving victims in the subject pool for two years following the intervention. Personal identifiers, including name, date of birth and address, were used to link information for victims and perpetrators across different cases over time.

The information was collected by three research assistants who did not have information on the treatment status of individual cases.[28] 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

[27] The DASH assessment tool is based on a series of 20 yes/no questions that the responding officer asks victims of domestic abuse. The tool is used as guidance for referring cases to a MARAC meeting to manage the risk. We provide an example of the questions of the DASH assessment tool in supplementary Appendix I.

[28] 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.

throughout the UK. The PND holds over 3.5 billion searchable records with information 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 randomised 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 authorised 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.[29] However, there were important practical and ethical implications for the repeated collection of sensitive survey information from DV 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

[29] The full survey is provided in Appendix H.

using researchers specifically trained in surveying victims of crime. Interviewers conducted 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.[30] The survey was administered to a 25% random sample of the full subject pool.[31] From this sample, we received an 84% response rate, resulting in complete surveys for 105 treatment group subjects and 109 control group subjects.

In Appendix E.2, we provide a detailed analysis of selection into the survey sub-sample, and the potential for related biases. We conclude that bias due to sample selection is likely to be small.

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.[32] 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 risk assessment score (1.28 and 1.28) do not differ significantly between treatment and control. Furthermore, we do not observe a signif-

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

[31] This sample was negotiated with the Leicestershire Police Information Services Unit based on their resource constraints and an estimated 250 surveys.

[32] 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 DV 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 a 5% level 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.

icant difference in the intimate partner status of the victim and perpetrator or the presence of children in the household. Importantly, the pooled F-stat from a regression of treatment status on all characteristics used in the main analysis fails to reject that the treatment and control group are balanced ($p = 0.739$). We therefore interpret Table 1 as evidence that allocation to the treatment or control group was random.[33]

Table 1 about here

The descriptive statistics for this sample are consistent with the picture of demographic characteristics of victims and perpetrators in 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.

3.7 Characteristics by engagement status

We report differences in the pretreatment characteristics of subjects in the treatment group by their engagement with the intervention (Table 2). We divide the treatment group into three categories, subjects for whom a caseworker was unable to make contact (no contact), subjects whom the caseworker contacted, but who declined the service (contact with no engagement), and subjects who were contacted by a caseworker and the service was accepted (contact with engagement).

Table 2 about here

For many of the characteristics, we do not see a significant difference across the three groups. However, there are notable exceptions. The sex of the victim and the perpetrator

[33] In Appendix F.1, we provide additional evidence that the treatment status is randomly distributed across the 68 neighbourhoods (police beats) represented in the data.

appear to be important. Victims are female in 91.6% of cases with contact and engagement, but only 82.7% cases without contact (p = 0.005 for the difference). Similarly, 22.0% of cases with no contact have a female perpetrator compared to 9.0% of cases with contact and engagement (p = 0.000 for the difference). We also find that cases with no contact are 10.7 percentage points more likely to have a white victim (p = 0.002) and 12.4 percentage points more likely to have a white perpetrator (p = 0.002).

Perhaps most interesting, we find that the risk assessment score is significantly lower for no contact subjects than for subjects who are contacted. There is a 14.8% difference between the mean value of the risk assessment score in the no-contact group and the contact with engagement group (p = 0.002). Cases in the contact with no engagement group also had a higher risk assessment score than those in the no-contact group, equating to a difference of about 8.5% (p = 0.086). Put another way, 84.5% of no contact cases received the lowest risk score, as opposed to 75.8% and 72.8% of contact without and contact with engagement.

In Appendix F.2 we regress an indicator for the different margins of engagement status on all characteristics to assess the joint significance. The results are similar to what is reported in Table 2. In cases with female perpetrators, the victim is less likely to be contacted (p = 0.059), but we see no difference in engagement conditional on contact (p = 0.755). Age of the victim and perpetrator is not associated with contact rates, but engagement when contacted is increasing with age. Finally, conditional on other characteristics, a higher risk assessment score is associated with a greater likelihood of making contact (p = 0.011) and a higher engagement rate when contacted (p = 0.148).

4 Results

In this section, we discuss the estimated impact of the intervention on a number of outcomes reflecting various stages of the case life cycle. 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, the risk assessment score for the initial callout, and dummy variables for the location of the initial callout across 68 neighbourhoods (police beats).[34] 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 Intervention effect on statement provision

In Table 3, 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 led 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 Appendix F.4, we provide two-stage least square estimates to examine the treatment response of victims who engage in the intervention. Under reasonable assumptions, we find a 12.6 percentage point

[34] 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.

decrease (p = 0.012) in statement provision for victims who engaged with the intervention.

Table 3 about here

Given the timing of the intervention, we should not observe an effect on statements that 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 ($day = 0$, before treatment) and (conditional on *no statement* at $day = 0$) making a statement at least one day after the initial police callout ($day > 0$, after treatment). As expected, the treatment and control group provide statements at $day = 0$ at the same frequency (Column 4, Table 3). The estimated treatment effect is a statistically insignificant difference of -1.1 percentage points (p = 0.621). The treatment group is less likely than the control group to make a statement at $day > 0$ (Column 5, Table 3). 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 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 $day = 4$, the propensity to make a statement on a given day is less than 1%. Second, consistent with Table 3, we do not observe a significant difference at $day = 0$, the day with most statements made. Third, a negative treatment-control statement gap persists from $day = 1$ to $day = 4$ days following the initial callout; we do not observe a distinguishable statement difference in days for which statement making is relatively infrequent ($day > 4$).

4.2 Intervention effect on non-police service use, re-victimisation 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 group 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 visited the 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

[35] This information, taken from caseworker reports for the 261 subjects who engaged with the intervention, is summarised in Table A.1 of the supplementary appendix.

[36] However, our ability to get precise estimates is 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.

not significant at conventional levels. We summarise these results with an index of service uptake (following Anderson, 2008). Overall, the intervention had a strong and significant positive effect on service uptake beyond what was provided directly through the intervention ($p = 0.042$).

Figure 4 about here

The survey results suggest that the intervention had a positive effect on the perceived risk of the subject being exposed to future DV (Panel B, Figure 4). Subjects 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 victimisation 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 victimisation 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 Intervention effect on repeat police-reported domestic violence

In the previous section, we investigated the effect the intervention had on the perceived risk of future victimisation using survey data. In this section, we expand on this using police data on repeat victimisation 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 a two year period. The estimated treatment effects are positive, but small and statistically insignificant (columns 1–2, Table 4). 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 ($p = 0.390$). Next, we examine the results for the number of reported domestic incidents (columns 2–3). The coefficients are also small, positive but not statistically significant. Over two years, 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 4). This is a 5.4% increase over the control group mean of 3.582 DV incidents.[38]

Given the large standard errors, we cannot confidently rule out that the intervention led to an increase in subsequent police reported incidents of DV. However, we can compare our

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

[38] We provide a heterogeneity analysis by risk assessment score in Table F.2 and discuss the results in the appendix section F.5.

estimates to those of a similar intervention studied in Davis et al (2010),[39] in which a police officer team visited the household within 24 hours to offer victims assistance. They find that treatment increased repeat police callouts by 8 percentage points (33% of control mean) and the number or repeat callouts by 0.18 (39% of control mean). At 95%, the upper bounds on the estimates we report in Table 4 are 7.6 percentage points (10%) for probability of a repeat, and 0.66 (25%) for number of repeats. As a percent relative to mean values, we can rule out magnitudes as large as the point estimates reported in Davis et al (2010).

Table 4 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 less severe than would be reported absent the treatment.[40] We examine three measures reflecting the severity of the reported incidents.[41] These will allow us to examine whether subsequent police reported incidents of DV differ in their severity 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 rather in this case mechanically arise through the effect of treatment on the composition of the sample observed in a repeat incident.

As a first measure, we investigate the raw DASH score corresponding to the repeat incident, where a higher score (out of 20) is associated with a more severe incident. As a second measure, we investigate whether the victim passes the threshold for MARAC, the pre-existing multiagency intervention, either by a DASH score greater than 14 and/or by

[39] We make these comparisons cautiously, as there are several important differences in the design of Davis et al (2010) and our study. In particular, the selection of the subject pool in Davis et al (2010) is not conditional on previous reported incidents.

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

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

having at least seven police-reported incidents over 365 days. As a third measure of severity, we consider the arrest of a perpetrator by the responding officers, where we assume that incidents where the perpetrator is arrested are more severe.

Table 5 about here

For repeat police callouts, the treatment group had a DASH score 0.576 points lower than the control group (Column 2 of Table 5). 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. Results are similar for the proportion of repeat incidents that pass the MARAC intervention threshold (2.7 percentage points less likely for the treatment group) and arrests (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 average 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 led 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.[42]

4.4 Intervention 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

[42] However, a reduction in incidents may be concealed by a large increase in reporting of minor cases previously not reported to police.

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 6 reports the estimates.

Table 6 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 percentage points reduction in the 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

Two overarching concerns are targeted in this intervention. The first and most important is the long-term safety and well-being of victims of DV. The second concern regards the most efficient use of public services available to victims of DV, namely whether the intervention led 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 victimisation, 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 the safety of victims documented using the survey results, we do not find a strong 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).

Considering this explanation, one can speculate why improving support service access might not reduce violence. A possible explanation is that the available services do not address the specific needs of repeat victims. A few empirical studies examine the effectiveness of individual support services and repeat violence. For instance, Stover, Meadows and Kaufman (2009) review the literature that looks at specific victim-support services, concluding there is little evidence that the services lead to a fall in rates of repeat victimisation. For example, it is possible that the mix of available services simply does meet victims' needs. This suggestion contrasts with our findings that the intervention led to increased uptake of specialist DV services, as documented in our survey results.

An alternative interpretation of our results is that the intervention reduced violence within the household but also increased the victim's willingness to report a future incident. This change might lead to the number of repeat incidents appearing to be unaffected by the intervention. We explored this channel by examining measures that reflect the severity of the repeat police callouts. The consistently lower measures of severity for treatment group victims, although not statistically different from zero, are 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 is 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 of a reported incident is sufficiently serious to warrant a MARAC intervention 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 finding is in contrast to previous studies (e.g., Davis, Weisburd and Hamilton, 2010) and is likely attributable to the careful design of the intervention ensuring that 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 arrests or charges. These results might be surprising given the strong positive correlation between victim statement provision and perpetrator arrests (Figure 1). Once a statement is made, it requires investigative efforts on the part of the 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, lower statement productivity than other victims.[43]

In Appendix B.2, we introduce a formal framework defining victim types according to whether they change their statement provision upon receiving 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 statements without a change in arrest means that police services are being used more efficiently. Specifically, for victims who forgo statement provision due to treatment, either their statement is less effective in resulting in criminal justice outcomes than those who make statements due to treatment, or the probability that their statements lead to a criminal justice outcome is close to zero.

We explore this interpretation further by comparing changes in outcomes for 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.2.

Table 7 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 $day > 0$, Panel A, Table 7). 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

[43] 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.

$day = 0$, Panel A, Table 7), for which group differences are smaller in magnitude and not statistically significant.

The correlation between a statement and a perpetrator arrest is 10.5 percentage points higher for the treatment group relative to the control group (*Any statement*, Panel B, Table 7). 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 $day > 0$, Panel A, Table 7). We find no significant difference between the treatment group and control group in this correlation for statements made at the initial callout.

We also examine differences in the treatment and control in the correlation between the provision of statements and perpetrator charges and sentencing (panels C and D, Table 7). The estimated differences between treatment and control are similar in size compared to the estimates for arrests, but not statistically different from zero.

Figure 6 about here

This result indicates an increase in the correlation between statements and arrests 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. One way in which there may be a compositional difference is across the risk assessment score. In Figure 5, we show the distribution of statement providers across the risk assessment score for the treatment and control group. The control group has a larger proportion of statement providers in the lowest risk assessment score, treatment group statement providers are more likely to have higher risk assessment scores. Consistent with this, we show in Appendix F.5, that the reduction in statements in response to the intervention can entirely be attributed to subjects with the lowest risk assessment score.[44]

[44] In Appendix F.5, we reproduce the main results of this study allowing for a heterogeneous response according to the initial risk assessment score.

6 Intervention cost

In this section, we provide details on the cost of implementing the programme and target obtaining a sense of whether the intervention provides for good use of scarce public resources. To do this, we focus on the effect of the intervention on statement-making. We contrast the direct implementation cost of the intervention with the savings from the reduction in time spent on investigations by police officers following a statement. For the purpose of this analysis, we implicitly assume that the marginal effect of these forgone statements on DV outcomes (e.g. charges and convictions of perpetrators) is zero. This is consistent with our results, particularly around the retraction of statements, and allows us to engage in a simplified cost-benefit analysis reflecting the short-run costs and benefits of the intervention for police.[45] This provides us with a baseline on which to assess how expensive this programme is.

The implementation cost of Project 360 reflects the set-up cost and the cost of running the intervention during the RCT (November 2014 to April 2015). We exclude evaluation costs, such as the victim survey. The total cost of the intervention, including labour, management and expenses was £64,631 for the six-month period (we provide the full details of the exercise in Appendix G). For the 510 victims in the treatment groups, this works out at £126.72 per victim. This is comparable to the program cost of a perpetrator focused DV intervention of £100 per-participant (Strang et al., 2017), and dwarfed by the estimated £20,000 per victim to support high-risk victims through the pre-existing MARAC intervention (CAADA, 2010).[46]

[45]We refrain from a full cost-benefit analysis of the intervention due to important limitations on the available data. In particular, we do not consider the potentially large and difficult to quantify, intangible benefits to victims. As we lack information on how the use of the many different non-police services changed, and the corresponding cost of providing these services, our analysis does not consider the total change in public spending. Finally, we do not consider potentially long-lived benefits and costs to police, such as may arise from the intervention improving victims' subjective perception of the police.

[46]We exercise caution in our comparisons with these other programmes, which may have different objectives and outcomes to the intervention we analyse.

By reducing the time spent in an investigation following a statement to police, the intervention freed police resources. To get a sense of the value of this savings, we use official data on the cost of police officers from the *National Policing Guidelines on Charging for Police Services* (NPCC, 2019). The hourly cost of a full-time officer at the rank of Police Constable (the lowest rank) in 2019 was £58.99).

While we do not have information on the marginal change in police officer time due to the intervention, we can calculate the number of hours of police investigative time per statement not made that would have led to the programme breaking even with the cost of the intervention across all victims.[47] Based on an estimated effect of -0.065 on statements provided, we find that for an investigation time of 33 hours per statement, the cost of the intervention would be covered purely based on police officer time freed from investigating, ignoring any other positive effect the intervention may have. This is not too far off the median 20 hours of investigation time per DV case provided to us as an estimate by Leicestershire Police. This suggests that based only on police force cost-comparison, a victim focused intervention such as we study will recover roughly two-thirds of the total cost just from saving investigation time. Based on the benchmark figure of 20 hours per further investigation, the intervention saved 663 hours of police time to be allocated elsewhere over the six month period ($-0.065 \times 510 \times 20$hours).

7 Conclusions

Our experimental evidence on improving access to DV 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

[47] There are no official estimates on the number of hours per investigation available in the UK, but official estimates are available on the number of days per type of investigation to bring a case to closure. The median length of time to assign an outcome to domestic abuse-related offences for violence with injury, for example, is 17 days (Home Office, 2019).

pool treat police and non-police services as substitutes (see Appendix B). 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 criminal justice outcomes against perpetrators. We argue that this is consistent with the intervention having led to a more efficient use of police services by victims. One possible channel for improved efficiency is the retraction of a statement by the victim. Statement provision leads to an increase in police effort investigating and compiling a case, but retraction makes a statement inadmissible as legal evidence. Retraction is 84% lower in the treatment group than in the control group. In this way, making non-police services easier for victims to access will alleviate some of the pressure on scarce police services.

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 victimisation 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 DV. Despite a significant improvement in the accessibility of DV support services, we find little change in future victimisation. 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 victimisation.

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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)	
F-stat [p-value]			0.890	[0.739]

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. Column *SMD* reports the standard mean difference. F-stat corresponds to a test of the joint significance of a regression of treatment status on all control variables. †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: Characteristics by intervention engagement

	No contact (1)	Contact with no engagement (2)	Contact with engagement (3)	p-value (1) *vs.* (2) (4)	p-value (1) *vs.* (3) (5)	p-value (2) *vs.* (3) (6)
Victim female	0.827	0.916	0.916	[0.035]	[0.005]	[0.996]
Perpetrator female	0.220	0.121	0.090	[0.037]	[0.000]	[0.359]
Victim age	35.432	31.682	34.348	[0.015]	[0.366]	[0.050]
Perpetrator age	33.780	31.533	33.477	[0.119]	[0.794]	[0.130]
Victim white	0.910	0.865	0.803	[0.235]	[0.002]	[0.163]
Perpetrator white	0.881	0.812	0.757	[0.120]	[0.002]	[0.276]
Victim domestic cases (365 days)	2.163	2.561	2.249	[0.032]	[0.495]	[0.084]
Perpetrator domestic cases (365 days)	2.099	2.495	2.180	[0.084]	[0.652]	[0.161]
Intimate partners	0.777	0.755	0.759	[0.674]	[0.674]	[0.931]
Cohabitation	0.550	0.528	0.562	[0.723]	[0.809]	[0.563]
Children in household	0.508	0.607	0.623	[0.102]	[0.016]	[0.781]
Risk assessment score	1.173	1.273	1.347	[0.086]	[0.002]	[0.289]

Notes: This table reports mean values of each characteristic for cases in the treatment group, according to the victim's engagement with the intervention. p-values, reported in columns 4–6, correspond to a test that the true mean value in each corresponding columns are statistically different.

Table 3: Treatment effect for victim providing a statement to police

			Falsification test	
	(1)	(2)	(3) day=0	(4) day>0
Treatment	-0.062	-0.065	-0.011	-0.062
	(0.028)	(0.027)	(0.021)	(0.024)
Victim female		-0.004	-0.006	0.004
		(0.045)	(0.036)	(0.039)
Perpetrator female		-0.055	-0.036	-0.026
		(0.044)	(0.035)	(0.038)
Victim white		0.087	0.059	0.040
		(0.049)	(0.039)	(0.046)
Perpetrator white		-0.081	-0.054	-0.046
		(0.047)	(0.038)	(0.045)
Cohabitation		0.121	-0.021	0.149
		(0.028)	(0.023)	(0.025)
Children in household		0.004	0.023	-0.016
		(0.028)	(0.023)	(0.025)
Domestic cases (365 days)		-0.004	-0.002	-0.002
		(0.009)	(0.007)	(0.008)
Risk assessment score		0.277	0.180	0.175
		(0.026)	(0.021)	(0.026)
Control group mean	0.299	0.299	0.137	0.188
	(0.020)	(0.020)	(0.015)	(0.019)
N	1015	1015	1015	878
R^2	0.005	0.197	0.167	0.210
Control variables ($p\,value$)[†]		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 estimate, unconditional and conditioning on the reported control variables. In Column (3), the outcome is equal to 1 if a statement is provided within 24 hours of the initial police callout, and 0 otherwise. In Column (4), the sample excludes cases in which a statement is provided within 24 hours of the initial police callout. Estimates in columns (2)–(4) 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 4: 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)
Perpetrator 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)
Perpetrator 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)
Children in household		0.036		-0.160		-0.442
		(0.030)		(0.263)		(0.319)
Domestic cases (365 days)		0.014		0.257		0.236
		(0.009)		(0.079)		(0.087)
Risk assessment score		-0.028		0.005		0.109
		(0.028)		(0.213)		(0.246)
Control group mean	0.749	0.749	2.681	2.681	3.582	3.582
	(0.019)	(0.019)	(0.141)	(0.141)	(0.165)	(0.165)
N	1015	1015	1015	1015	775	775
R^2	0.001	0.114	0.002	0.098	0.001	0.128
Control variables ($p\,value$)†		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 5: Incident severity at repeat police callouts

	DASH score		MARAC threshold		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)
Perpetrator 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)
Perpetrator 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)
Children in household		0.948		-0.026		0.071
		(0.391)		(0.042)		(0.046)
Domestic cases (365 days)		-0.128		0.015		0.013
		(0.109)		(0.012)		(0.013)
Risk assessment score		1.897		0.108		0.078
		(0.394)		(0.043)		(0.040)
Control group mean	6.039	6.039	0.215	0.215	0.457	0.457
	(0.255)	(0.255)	(0.025)	(0.025)	(0.028)	(0.028)
N	522	522	535	535	639	639
R^2	0.003	0.291	0.000	0.176	0.001	0.143
Control variables ($p\,value$)†		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 number of affirmative DASH risk assessment criteria across all repeats. The dependant variable in columns (3) and (4) is a binary variable equal to 1 if the victim meets the threshold to be recommended for a multi-agency meeting (MARAC) intervention, 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 6: 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)
Perpetrator 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)
Perpetrator 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)
Children in household		-0.017		0.014		0.015
		(0.029)		(0.022)		(0.017)
Domestic cases (365 days)		-0.004		-0.004		-0.005
		(0.009)		(0.008)		(0.006)
Risk assessment score		0.241		0.153		0.087
		(0.029)		(0.027)		(0.022)
Control group mean	0.263	0.263	0.133	0.133	0.083	0.083
	(0.020)	(0.020)	(0.015)	(0.015)	(0.012)	(0.012)
N	1014	1014	1015	1015	1015	1015
R^2	0.000	0.176	0.000	0.137	0.000	0.107
Control variables ($p\,value$)†		0.000		0.000		0.000

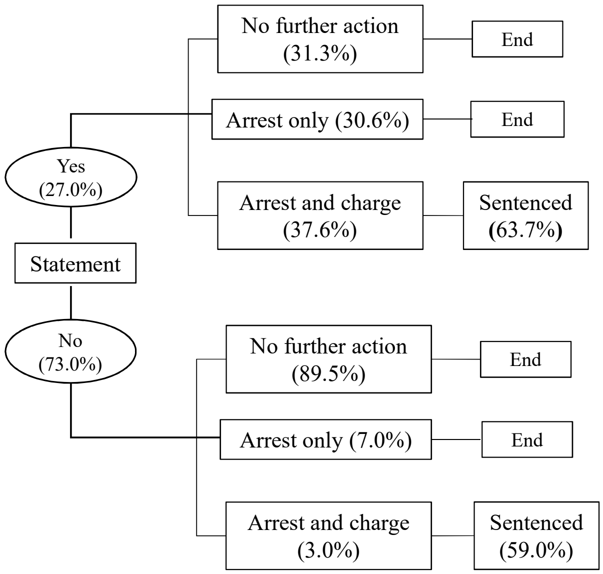
Notes: This table reports linear probability estimates for three binary outcomes, all referring to the initial callout case. Outcome *Arrest* is equal to 1 if the person identified as "perpetrator" is arrested by police, and 0 otherwise. Outcome *Charged* is equal to 1 if the person identified as "perpetrator" 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 7: 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 $day = 0$	0.235	0.275	-0.040
			(0.075)
Statement at $day > 0$	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 $day = 0$	0.765	0.725	0.040
			(0.075)
Statement at $day > 0$	0.717	0.561	0.156
			(0.083)
C. Perpetrator charged by the Crown Prosecution Service			
Any statement	0.397	0.371	0.026
			(0.060)
Statement at $day = 0$	0.382	0.406	-0.023
			(0.084)
Statement at $day > 0$	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 $day = 0$	0.221	0.290	-0.069
			(0.075)
Statement at $day > 0$	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* $day = 0$ condition on statement provided within the first 24 hours following the initial police visit, rows labelled *Statements at* $day > 0$ condition on statement provided after 24 hours period. $N = 272$, with 137 for statement at $day = 0$ and 135 for statement at $day > 0$. Robust standard errors for differences reported in parenthesis.

Figure 1: Tree representing the life of a case

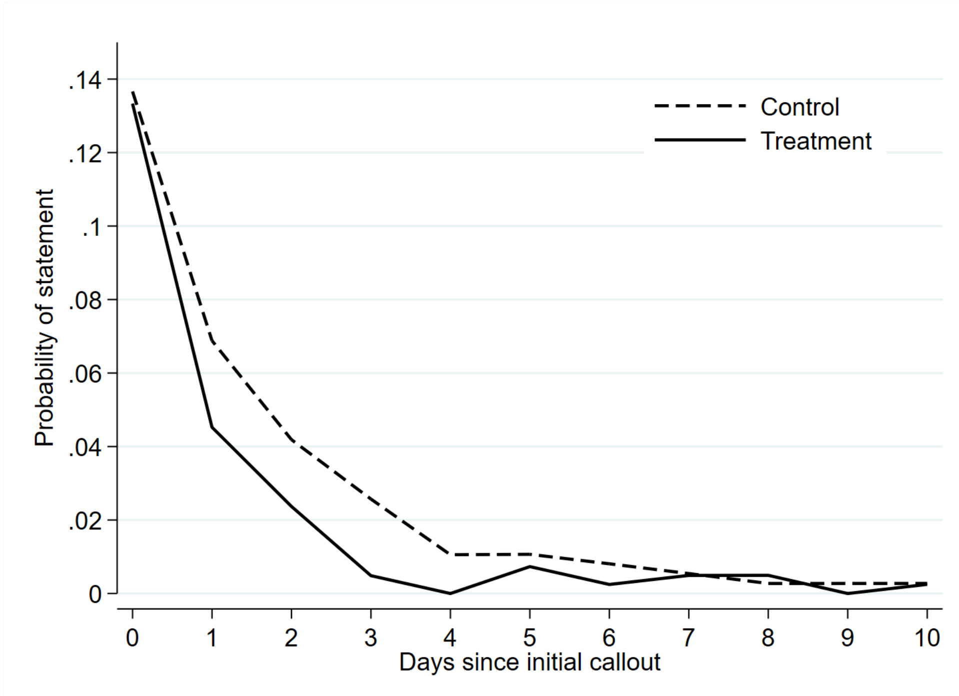


Notes: Percentages correspond to the probability of the event conditional on position in the tree, based on subject pool data. *End* nodes indicate that no further action is taken with respect to the case.

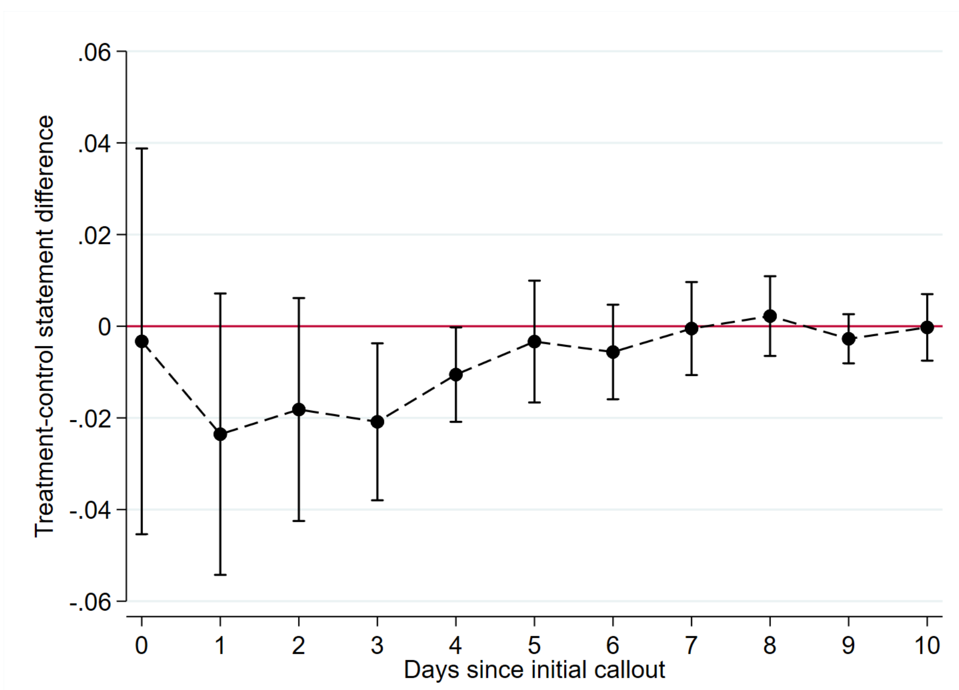
Figure 2: Leicestershire Police Force area

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



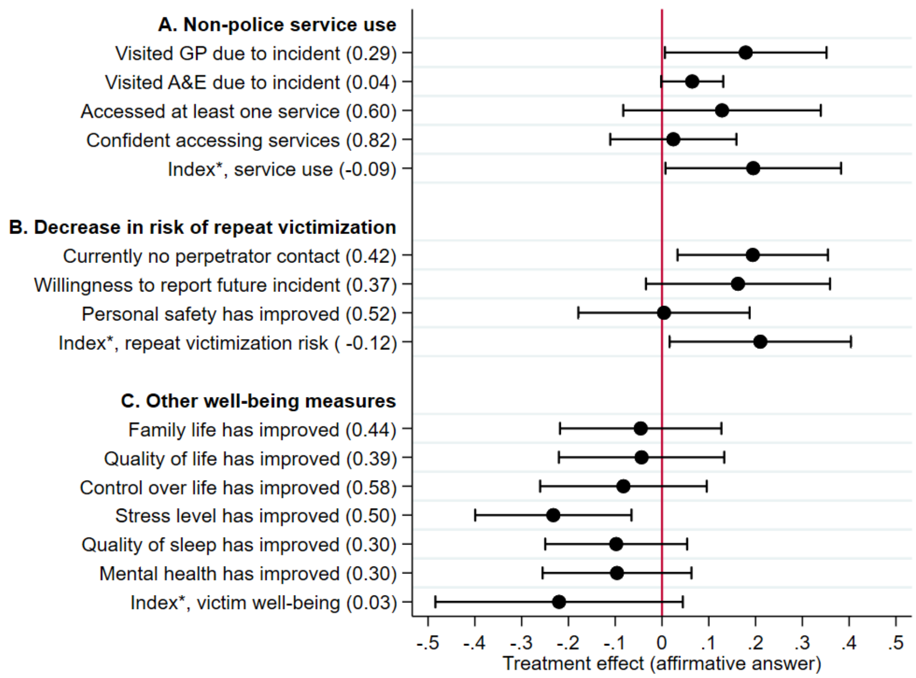
(a) Probability of statement, conditional on no previous



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

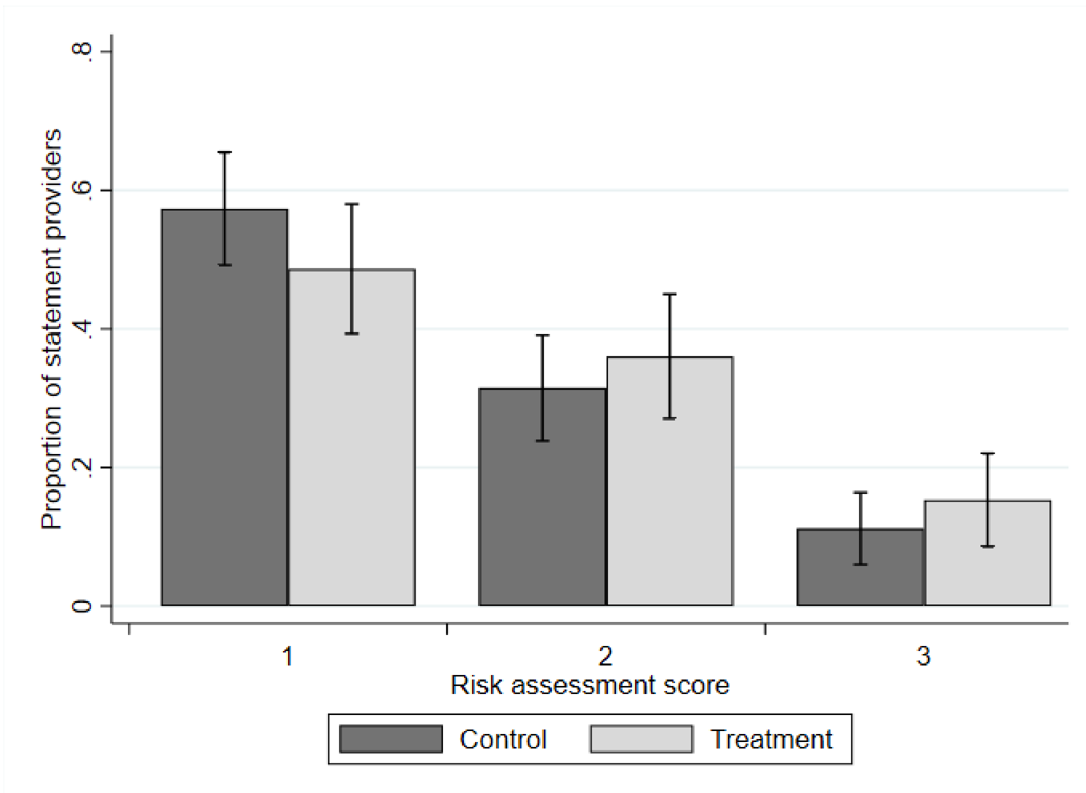
Notes: These figures show (a) the probability a statement is provided by days since the initial callout, conditional on not having not already provided a statement, and (b) the corresponding treatment-control group difference by day—bars show 95% confidence intervals on difference.

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).

Figure 5: Composition of statement providers by risk assessment score



Notes: In this figure bars show the percent of statement providers in each risk assessment score category, conditional on treatment status.

For Online Publication

Appendices to accompany "Improving access to support services for victims of domestic violence: demand for services and victim outcomes"

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May 2022

Contents

Appendix A Background information on support services

Table A.1: Common non-police services accessed by the engaged treatment group

Type of service	Details	% accessed†
Refuge housing		9.20
Register with GP		12.3
Grants	Supplemental support for basic household goods	16.2
Organize a solicitor		19.8
Counseling services	Freedom programme	48.4
Personal safety	Develop escape plan, install alarms, change locks	60.5

Notes: Information in this table comes from caseworker reports. †Reflects the percent of the 261 subjects in the treatment group who engaged with the intervention.

In this appendix section we provide information on the non-police support services that were available to victims of domestic violence at the time of the intervention. Table A.1 summarizes the most commonly accessed types of services for subjects in the trial's treatment group who engaged with the caseworker. Figure A.1 shows the information sheet that responding officers provided to victims of domestic violence when they attended an initial callout. Table A.2 lists all of the non-police support service providers that where available in Leicestershire at the time of the trial.

Figure A.1: Information pamphlet provided by responding officers at domestic incident

Emergency numbers

999
Police
Ambulance
Fire

24hour number

0808 2000 247
National Domestic Violence Helpline

Agencies offering specialist services in domestic violence in Leicester

Domestic Violence Integrated Response Project (DVIRP)
0116 255 0004
(Helpline)

DV specific agency or post
Agency

Client Group and Remit
Women and men aged 16 and over in the city and the county who are affected by domestic violence.

Men who self refer as perpetrators of domestic violence.

Specialist service for members of faith communities aged 16 and over, particularly members of black and minority ethnic communities.

A specific network of workers offering therapeutic support to children and young people who have experienced domestic violence.

Referral Process
Self referral

Service Offered
- Listening Ear
- Safety Planning and Options
- Support Advocacy
- Information
- Contact with religious scholars
- Face to face support
- Therapeutic support with young people (individual and group) and with their carer/parent

Women's Aid Leicestershire Ltd Community Outreach Service
0116 285 8079
(Voluntary)

DV specific agency or post
Agency

Client Group and Remit
A free, and confidential service for women with or without children who have experienced, are experiencing or are at risk of experiencing domestic violence irrespective of their age, cultural backgrounds, race, ability or sexuality. For those living independently or with the perpetrator/s.

Aged 16 and over.

Referral Process
Self referral, referral through any agency, including housing, and also through family and friends.

Service Offered
- Emotional Support either face-to-face or by telephone
- Housing related support
- Legal advice
- Practical (including finance) advice & information
- Advocacy
- Group work
- Social/cultural events
- Access to safe accommodation

Leicestershire Women's Aid Refuge Service
0116 244 0169
(Voluntary)

DV specific agency or post
Agency

Client Group and Remit
Women with or without children who need somewhere safe to stay due to fear of domestic violence.

Aged 16 and over (housing benefit needs to be secured, or alternative finance for rent).

Male children over 14years may not be considered appropriately placed within the refuge.

Referral Process
Self referral, referral through any agency, including housing, and also through family and friends.

Service Offered
- Housing
- Individual case - work support including help with finances, legal matters and re-housing

Suruksha
0116 274 0422
(Voluntary)

Client Group and Remit
Supported housing project for asian women who have fled their home due to domestic violence.

Referral Process
Self referral, agency referral

Service Offered
- Housing support
- Individual casework

Panahghar Shantighar and Shardhgar
0116 270 5320
(Voluntary)

DV specific agency or post
Agency

Client Group and Remit
Asian women with or without children who have experienced domestic violence and need somewhere safe to stay.

Referral Process
Self referral, referral through other refuge projects, police referral.

Service Offered
- Housing
- Individual case-work support including help with finances, legal matters and re-housing

Panahghar Shantighar Outreach Support Service
0116 270 5320
(Voluntary)

DV specific agency or post
Agency

Client Group and Remit
Asian Women with or without children who have experienced domestic violence who are living independently and would like support.

Referral Process
Self referral, referral through other refuge projects

Service Offered
- Individual face to face support

Family Welfare Association FLIP Project
0116 255 3738
(Voluntary)

DV specific agency or post
Posts

Client Group and Remit
Men and women who are parents or carers who have experienced domestic abuse (Men will not be able to enter the freedom programme). City residents.

Referral Process
Self-referral

Service Offered
- Agency referral where parent has given consent
- 12 week 'freedom' programme

Leicester City Council Housing Department

DV Unit, Border House
0116 221 1407

Client Group and Remit
Women and children that are survivors of DV. No male children over the age of 11. Extra security measures to protect survivors from abuse and neglect, 24 hour security measures in operation that are linked to Staffing quarters

Referral Process
Self-Referral and any other agencies working with this client group

Service Offered
- Emergency direct access accommodation and housing
- Related Support
- Holistic needs assessment and joint working with partner agencies to meet these needs

Pet retreat

07910 721 797
For people who have experienced domestic violence and need support in looking after their pets while they flee to a place of safety

Probation

0116 262 0400
(Statutory)

DV specific agency or post
Post

Client Group and Remit
Women who have court referred male partners on the perpetrator programme

Referral Process
Referral through probation

Service Offered
- Signposting
- Safety planning
- Risk assessment
- Support (4 session structure)

Police Domestic Violence Officers

0116 222 2222
(Statutory)

DV specific agency or post
10 Posts

Client Group and Remit
Women and men and who have recorded an incident of domestic violence to the police or would like advice

Referral Process
Self referral, police referral

Service Offered
- Support in the aftermath of an incident
- Liaison and guidance through the legal investigation
- Referral to other agencies

Additional local agencies that provide workers and volunteers with specific training on domestic violence

Rape Crisis

0116 255 8852
(Voluntary)

Client Group and Remit
Women who have experienced sexual assault

Referral Process
Self referral

Service Offered
- Counselling, support, information, training

Juniper Lodge

0116 273 3330
(Statutory Partnership)

Client Group and Remit
Adults who have experienced sexual assault.

Referral Process
Self referral or Police referral.

Service Offered
- One to one support sessions
- Forensic Medical Examination
- Video recorded interviewing facilities
- Information

Witness Cocoon

0116 222 9886
(Voluntary)

Client Group and Remit
Adults (aged 16 yrs and over) at risk or affected by crime or anti-social behaviour.

Referral Process
Self referral or referral from any agency

Service Offered
- Telephone support
- Allocation of a worker for face to face ongoing support
- Information and advice, including accompaniment to civil and criminal courts

Victim Support & Witness service

0116 253 0101
(Voluntary)

Client Group and Remit
Adults who identify themselves as the victim of a crime. Support to witnesses called to give evidence at criminal court.

Referral Process
Self referral, Police referral and other agencies.

Service Offered
- Face to face support, information and advice

Bridge House

0116 222 1845
(Voluntary)

Client Group and Remit
24-hour service.
Accept women with additional needs such as drugs and alcohol, mental health needs.

Referral Process
Self referral, agency referral

Service Offered
- Safe supported housing

New Futures

0116 255 9696
(Voluntary)

Safe and confidential service to anyone at risk of or currently involved in prostitution.

Youth work project

Referral Process
Self referral

Service Offered
- Outreach and practical on site facilities, including showers, health advice, a place to chill, access to counsellors.

* This list is not exclusive or exhaustive and is subject to change at any time. It focuses on agencies established to provide a domestic violence service exclusively and those where specialist posts for domestic violence service provision have been established.

Domestic Violence

Agencies offering specialist services in domestic violence in Leicester...

LDVF LEICESTER DOMESTIC VIOLENCE FORUM

Safer Leicester Partnership
Working together for a safer City

Table A.2: Leicestershire non-police service providers

Name of service provider	Administration	Type of services
Adam Project	Charitable	Men's domestic violence support and advice service.
Apna Ghar	Charitable	Refuge housing for Asian women with or without children.
Bethany House	Charitable	Refuge housing for women with children.
Boarder House	Municipal	Refuge housing.
Bridge House	Charitable	Refuge housing.
Broken Rainbow	Charitable	Domestic violence helpline for lesbian, gay, bisexual and transgender.
Free-Va	Charitable	Emotional and practical support for domestic violence victims.
Foundation Housing Association	Charitable	Refuge housing, emotional and practical support.
Hope House	Religious	Short-medium term refuge housing.
Jasmine House	Municipal	Counseling and emotional support services.
Juniper Lodge	Charitable	Sexual assault counseling and practical services.
Kirton Lodge	Municipal	Refuge housing.
Lawrence House	Charitable	Refuge housing, ages 16–25.
Living Without Abuse	Charitable	General support and referrals service.
Loughborough Road Hostel	Municipal	Refuge housing for women with children.
Panahghar Shantighar and Shardghar	Charitable	Refuge housing for Asian women with or without children.
Pet Retreat	Charitable	Pet fostering for people fleeing domestic violence.
Refuge	Charitable	Domestic violence helpline.
Respect	Charitable	Domestic violence helpline, focusing on male victims and perpetrators.
Safe Project	Charitable	Domestic violence helpline and referrals.
The Dawn Centre	Municipal	Short-term accommodations for homelessness.
The Jenkins Centre	Municipal	Counseling services for perpetrators.
Women's Aid	Charitable	Domestic violence helpline (national) and referrals.
Women's Aid Leicestershire	Charitable	Domestic violence helpline (local) and referrals.

Notes: *Type of services* refers to the primary service(s) provided. This information was taken from the service provider website or other literature. It may not reflect all provided services. The entries reflect service provision in the Leicestershire Police Force area for the period November 2014 to July 2015.

Appendix B Conceptual framework

B.1 Service use and barriers to services

In this section, we present a stylized conceptual framework to guide our thinking about the relationship between access barriers and the choice between various services for victims of DV.

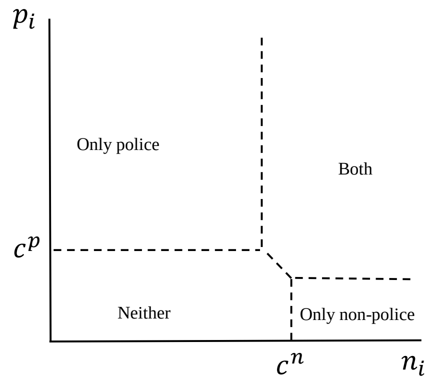
Consider a model in which individuals, denoted by i, choose between police and non-police services. Each service results in individual-specific benefits denoted by $p_i \geq 0$ from the police services and $n_i \geq 0$ from the non-police services. If both services are accessed, individuals also receive an incremental benefit of b, which may be positive or negative (i.e., services may be complements or substitutes), but which is common to all users. Barriers are reflected by a composite cost to the individual of accessing each service, c^p and c^n, common to all users. Costs and benefits are additively separable, and utility with no service use is normalized to 0. The utility for an individual i, denoted U_i, can be written as:

$$U_i = (p_i - c^p) \times \mathbb{1}[\text{police}_i] + (n_i - c^n) \times \mathbb{1}[\text{non-police}_i] + b \times \mathbb{1}[\text{both}_i] \tag{B.1}$$

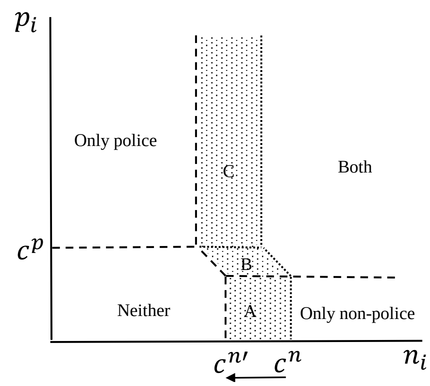
where $\mathbb{1}[\cdot]$ is an indicator function equal to 1 if the service in the argument is accessed and 0 otherwise. Individuals choose the service or services that provide them with the greatest utility. In Figure B.1, we depict service utilization at different values of p_i and n_i in the case when b is positive (B.1a, B.1b) and when b is negative (B.1c, B.1d). Figures B.1a and B.1c show the possible outcomes absent the intervention. Observed use within the population will depend on the distribution of individuals across the possible values p_i and n_i.

Consider the effect of an intervention that works by decreasing the cost of accessing non-police services, with no change in the cost of access to police services. This is depicted in B.1b and B.1d by a movement from c^n to $c^{n\prime}$. In both cases, $b > 0$ and $b < 0$, there will

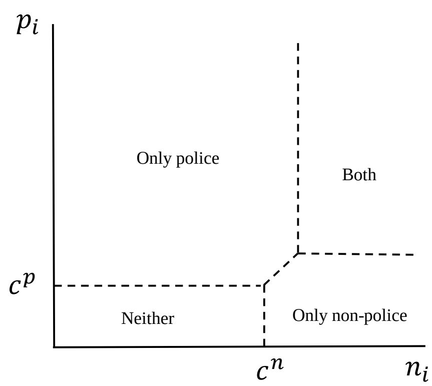
Figure B.1: Access frictions and service use



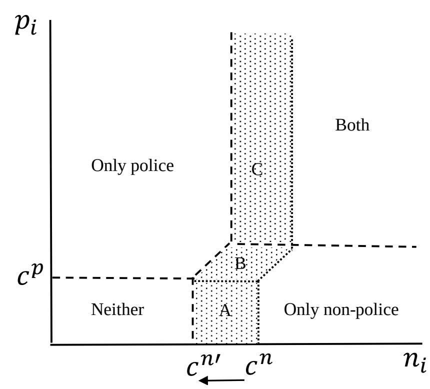
(a) Without intervention, $b > 0$



(b) With intervention, $b > 0$



(c) Without intervention, $b < 0$



(d) With intervention, $b < 0$

Notes: These figures are based on equation (B.1).

be an unambiguous increase in the use of non-police services, shown by areas A, B, and C. However, the impact on the use of police services depends on the sign of b. If b is positive, then the use of police services will increase; this is due to users with preferences in area B of Figure B.1b. If b is negative, then the use of police services will decrease relative to before the intervention; this is due to users with preferences in area B of Figure B.1d. Note that, the observed variation in non-police services is attributable to individuals who have a value of p_i that is low, relative to other service users. This highlights the benefit of focusing on police services. In examining the demand for police services, we learn about the sign of b, reflecting whether the two types of services are complements or substitutes.

B.2 Statement making and productivity of police services

In our framework, victims can be classified into four types according to their statement making response to treatment (corresponding to the familiar label of *compilers* and *defiers*), labelled $d \in \{-1, 0^+, 0^-, 1\}$. A $d = -1$ type provides a statement in the control but not in the treatment group. A $d = 1$ type provides a statement in the treatment but not in the control group. A $d = 0^+$ type always provides a statement, and $d = 0^-$ type never provides a statement. We assume that a) the probability of a perpetrator arrest (charge or sentencing) is weakly increasing in statement provision, and b) conditional on statement provision, the intervention is uncorrelated with perpetrator arrest (charge or sentencing).[1] The relationship between the intervention and a perpetrator arrest (ignoring control variables) can be written as

$$P_{id}(treat_i) = \alpha_0^d + \alpha_1^d S_d(treat_i) + \mu_{id} \tag{B.2}$$

where i denotes the case and d denotes the victim type. P_{id}, is a binary indicator equal to 1 if the relevant punitive action (arrest, charge, sentencing) is taken against the perpetrator, and 0 otherwise. S_d is a binary variable equal to 1 of the victim provides a statement to police, and 0 otherwise, and is a function of treatment status and type. μ_{id} reflects unobserved heterogeneity in the outcome. From assumption b) above, we know that $E(\mu_{id}|treat_i, S_d) =$

[1] Assumption a) follows from the argument in Section 4.1 that statements provide evidence in building a case against a perpetrator. It rules out, for example, that a caseworker coaches the victim in a way that improves the statement. Assumption b) follows from arrests being made on the basis of the evidence needed for the CPS to press charges. This requires that the intervention influences arrest only through a victim's statement provision. Caseworkers are required not to interfere in the statement making process because the facts of a case might be distorted in the process.

0, treatment affects P_{id} only through statement provision.[2] The coefficient α_1^d reflects the type-specific effect of statements on punitive actions.[3] From assumption a) above, we know that $\alpha_1^d \geq 0$. This implies that P_{id} is a weakly monotonic, increasing function of victim statement provision.

Where w^d is the proportion of type d victims in the sample, such that $w^{-1} + w^{0^-} + w^{0^+} + w^1 = 1$, the ITT corresponding to equation (B.2) can be written as:

$$E(P(1)) - E(P(0)) = (\alpha_1^1 - \alpha_1^{-1})w^1 + \alpha_1^{-1}(w^1 - w^{-1}) \tag{B.3}$$

Notice that $w^1 - w^{-1}$ is the change in the proportion of cases for which a statement is provided due to the intervention. In other words, $w^1 - w^{-1} = \gamma_1$ from equation (1) in the main text when the outcome is statement provision. $\alpha_1^1 - \alpha_1^{-1}$ is the difference in the treatment effect of a statement on y_{id} between $d = 1$ and $d = -1$ types.

The estimates reported in Table 2 suggest that $w^1 - w^{-1} < 0$. Given that $\alpha_1^d \geq 0$, if $E(P(1)) - E(P(0)) = 0$, it follows that either $\alpha_1^1 - \alpha_1^{-1} > 0$, or $\alpha_1^d = 0$ for $d = \{-1, 1\}$. That is, either statements have no effect on punitive actions for the $d = \{-1, 1\}$ types, or statements have a greater effect for the $d = 1$ types than for the $d = -1$ types.

B.3 A cooling off period as an alternative hypothesis

In the main text of this article, we propose that the intervention led victims of DV to substitute away from using police services and toward using non-police services. However, a model of time inconsistent preferences (TIP) might alternatively also rationalize the results reported in Table 2. Here we briefly explain and test this alternative rationalization. We conclude that the data do not support this alternative theory.

[2]This rules out, for example, caseworkers directly influencing the decision of police to make an arrest.

[3]It is tempting to use $treat_i$ as an instrument for statement provision in the above equation. However, the possibility of both $d = 1$ type or $d = -1$ types means that we cannot assume monotonicity.

During their initial phone contact with the caseworker, some victims choose to schedule a face-to-face visit for further assistance (127 treatment group victims altogether). This meeting often takes place several days after the phone call (see Table B.1). If victims put-off making a statement until the face-to-face meeting, the passage of time between the phone call and the meeting may create a "cooling off" period, decreasing the willingness of victims to provide a statement. This is consistent with the qualitative findings in Ford (1983) who looks at the effect of judicially imposed cooling off periods in domestic violence cases. This suggests that the decrease in statements may be driven by time TIP, similar to Aizer and Dal Bo (2009).

We propose two tests of TIP using our data. First, if TIP is driving the change in statements, we expect to see a negative correlation between the length of time between the cooling off period (time between the phone call and the meeting) and statement provision. In Table B.1, we report the frequency of statements conditional on the length of time between the initial incident and the meeting with the caseworker.[4] We fail to reject the null hypothesis that the proportion of statements observed in columns (1) to (6) are statistically equivalent (*F-test* = 0.430, p=0.830), suggesting statement probability does not vary with meeting times. We also fail to reject that the proportion of statements for 1-day meetings and 4–7 day meetings, the lengths of time with the most observations, are equivalent (*F-test* = 1.130, p=0.290). If anything, we see an increase in the magnitude of statement making at 4–7 days relative to 1-day.

We can also check, among victims who make statements, if scheduling later face-to-face meetings means their statement is made later. If this is true, we expect to see a positive correlation between time to statements and time to meeting. In Figure B.2 we plot—for victims who both had a face-to-face visit and made a statement[5]—the correlation between

[4] All estimates are conditional on being in the treatment group and having a face-to-face meeting.
[5] This results in a sample of 35 observations, so results should be interpreted with caution.

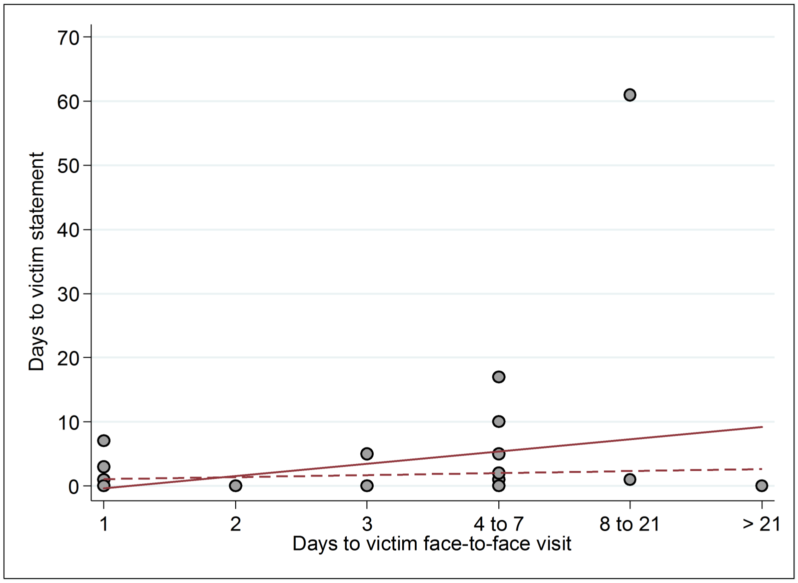
time to statement and time to face-to-face meeting. This shows weak evidence of a positive correlation between the timing of meetings and the timing of statements. A linear regression (solid red line) suggests that time to statement is increasing with time to meeting. However, when a single outlying observation is removed, there is no clear relationship between meeting and statement timing (dashed red line).

Table B.1: Correlation between statement provision and time until face-to-face meeting

	Days from intial incident[†]						
	1	2	3	4 to 7	8 to 21	>21	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Statement provided	0.244	0.308	0.250	0.349	0.167	0.250	0.276
	(0.071)	(0.125)	(0.131)	(0.069)	(0.131)	(0.226)	(0.040)
N	44	13	12	42	12	4	127
F-stat	0.430						
(Columns 1–6 equal)	[0.830]						
F-stat	1.130						
(Columns 1 and 4 equal)	[0.290]						

Notes: This table reports the proportion of cases for which a statement is provided conditional on the number of days between initial callout and a face-to-face meeting with a caseworker. Data reflect treatment group subjects who scheduled a face-to-face meeting with a caseworker. Standard errors reported in parentheses, p-values for F-tests reported in brackets.
[†] Number of days between the initial incident and the face-to-face meeting with the caseworker.

Figure B.2: Time to statement and time to face-to-face



Notes: This figure shows a plot of days (from the initial callout) to the face-to-face visit against days until a statement is provided. Points represent individual observations; some points capture multiple observation with the same value. Only cases in which both a face-to-face visit and a statement are reported. Solid line shows linear fit of all points, dashed line shows linear fit removing one observation at point (8 to 21, 61).

Appendix C Internal Review Board approval

The research protocol of the evaluation of the intervention was reviewed and approved by the Research Ethics Committee of the University of Leicester under application number mk332-5e3e and has been registered with the AEA RCT Registry number AEARCTR-0000537. The protocol has also been reviewed and approved by an internal review board of the Leicestershire Police Force. As the intervention was run by the Leicestershire Police Force, no informed consent was required from individuals in the subject pool regarding their participation. The IRBs also agreed that the collection of anonymized data from police administrative records (Leicestershire Police Database and the Police National Database) would therefore not require informed consent. Collection of the Victim Survey data was completed by the Leicestershire Police Information Services Unit for the evaluation of the trial. Using a police embedded survey team ensured that no sensitive information was shared with the external university researchers and that all interactions with victims were through a team member trained in dealing with victims of domestic abuse. Survey team members followed a dedicated safety protocol during the interview, a condition set by the IRBs. The protocol required that:

- Victims were only contacted by phone using the safe number provided to police during the initial callout.
- At the beginning of the phone call, the interviewer established the location of the victim, ensured that the victim could talk safely and that the perpetrator was not present.
- In case the phone call was interrupted or in case a victim indicated an imminent threat, the call handler requested police officers to attend the location as an emergency to ensure the safety of the victim.

As the victim survey was not part of the regular data collection of the police force, informed consent was required from all participants in the survey by the IRBs. The IRBs agreed that written consent was not appropriate, because of the potential risk of victims in case any written communication was intercepted by the perpetrator. Instead, it was agreed to inform participants at the beginning of the phone call and ask for their consent for the data to be used in the research project. To this effect, the interviewer read the following text prior to asking the survey questions:

> *With your permission, your responses and information about your case will be stored and shared with the University of Leicester for research purposes. Your name, personal contact details and any other identifying information will not be shared and will be treated in the strictest confidence.*
>
> *The goal of the research is to understand how police response to domestic disturbances can be improved.*

Participation in this survey is voluntary. You are allowed to refuse to answer any questions, or stop the survey at any time.

If you have any specific questions I would be happy to provide you with contact information.

Are you happy for me to continue with the survey?

The conditions of the IRB also restricted the types of questions to be included in the survey, as some topics were perceived to potentially cause unnecessary distress to the victim. For example, the research team was asked to exclude questions that would require victims to recall specific details about events of household violence, or to provide details of any violence that may have taken place since the initial incident. We also excluded any direct questions about the safety or well-being of children in the household upon request by the IRBs.

Appendix D Administrative data

D.1 Collection of administrative data

The primary administrative data was collected from two police databases. The first is known as the Crime Information System (CIS), which stores information about all local crimes handled by Leicestershire police.[6] The collection of data was undertaken by the evaluation team and research assistants hired for this task. The second is the Police National Database (PND), which holds information about cases and criminal convictions by the courts, for all cases in the UK. As access to the PND is highly restricted, even within the police force, information was collected by a specially trained and licensed police officer for whom every access to the PND was authorized for the research project.

All data collection took place on-site at a large Leicestershire Police station. Only anonymized and vetted data was permitted to be removed from Leicestershire police. The unique crime reference number was replaced by a researcher-generated ID, with the key linking crime reference numbers and ID stored with Leicestershire police. This ensured that the researchers could link future information collected to the vetted data, but vetted data could not be linked back to specific cases without the key. After the data collection was completed, the dataset was vetted by a senior officer to make sure no identifying information was present. Data was then transferred to the researchers via a secure data transfer.

Collection of administrative data from the CIS and PND systems took place between between November 2014 and July 2017 in three stages. The first stage took place during the running of the randomized-controlled trial (November 2014–April 2015). In this stage, we gathered information on the socio-demographic characteristics of victims, perpetrators and the children in the household, the date and details relating to the initial domestic incident,

[6] Data storage and access was replaced by the NicheRMS365 police records management system at the end of April 2015.

and the history of police incidents for victims and perpetrators. For victims who received treatment, details about their engagement in the programme where also recorded from the hard-copies of each caseworker's engagement records.

The second stage of data collection involved returning to the data at 12 month and 24 months after the last incident (in June 2016 and June 2017) to collect information on whether the victim was involved in further police incidents after the initial report was filed, as well as on the action taken by the police and the DASH risk assessment for the first five recorded incidents.

In the third stage, we collected data from the Police National Database (PND). We collected information on whether a perpetrator was arrested by police during or following a DV incident, whether a perpetrator was charged by the CPS, and whether a perpetrator was sentenced in court (and the details of sentencing). We accessed information on prosecution and court outcome for perpetrators for up to 24 months after the initial incident to allow for criminal justice proceedings to be completed. We linked the information from the different databases by crime reference number, and cross-checked the link through the date of the incident.

Additionally, information was also taken from the detailed reports completed by the programme's caseworkers. These reports were filled out by hand and stored as hard copies. The information on these sheets includes details about the level of engagement and services accessed by subjects. Of course, this information is only available for subjects in the treatment group who engaged with the intervention.

Appendix E Survey data

In this appendix section, we outline details of the collection of our survey data and provide analysis of the survey balance, and representativeness of the survey sample in comparison to the full sample pool of cases. The full set of survey questions and instructions are included in Appendix H.

E.1 Collection of the survey data

The one-month victim survey was administered by the Leicestershire Police *Service Improvement Department* (SID). The department includes a survey division with extensive experience in collecting data from victims of domestic abuse. The data was collected via telephone survey from victims in both the treatment and the control group. SID team surveyors were blind to treatment status.

At the beginning of each month, the SID team was provided with crime reference numbers corresponding to cases added to the subject pool in the previous month. From these cases, the SID team randomly sampled 25% of cases to be surveyed. Completed surveys were returned to our research team manager for the police data collection, who used the crime reference number to merge survey responses with the administrative data.

The survey was implemented with the safety of victims being of the utmost priority when establishing contact and completing the survey over the phone. Only victims who supplied police officers with a safe telephone number were included in the pool from which the survey sample was drawn. Upon contact, the interviewer asked for the name of the person answering the phone. If a person other than the victim answered the telephone, the interviewer said that they were calling to conduct a customer survey and would try again later, without identifying themselves as police staff. If the victim answered the phone, interviewers asked if there was any possibility that the call could be overheard by the person who caused the harm;

in such a case, they would arrange for the survey to be completed at another time. Before starting the survey, interviewers would first establish the precise location of the interviewee. In case the call was interrupted for any reason, a police response car would be sent to this location to establish whether the interviewee was safe. There were no such interrupted calls in the surveying done for this project.

The conditions for this project set out by the institutional review boards, and Leicestershire Police in a Data Processing Agreement, state that only survey data for which informed consent was provided could be linked to administrative data for the purposes of this project. In practice, this means we are restricted to the survey information for respondents who answered "Yes" to Q8 on the survey (Appendix H). As a result, we are unable to evaluate the characteristics of victims who where surveyed, but either where unable to be contacted or, did not consent to participating in the survey.

E.2 Survey balance and representativeness

Survey participation is voluntary, and conditional on a survey researcher being able to establish contact. Here we address concerns about non-random selection into the survey and the interpretation of our estimates. As a reminder, in this analysis we are only able to observe survey outcomes, including inclusion in the survey sample, for victims who provided consent to being included in the survey. We will refer to these observations as the *surveyed group*.

The first concern with this type of study is that treatment may affect survey participation, giving rise to non-random selection of the type addressed in Lee (1995). For example, it is reasonable to be concerned that treatment leads to victims feeling more engaged with the police and therefore more willing to participate in the survey. If this is the case, we expect to see the treatment group over-represented as a proportion of the total completed surveys. The surveyed sample consists of 214 observations (21.3% of the total sample), 105 in the treatment group (20.6% of total treatment) and 109 in the control group (21.3% of total control). The difference in the proportion of individuals in the treatment and control groups who completed the survey is small and not statistically significant (p =0.698). We also compare stats about survey completion across the treatment and control groups. The number of days between the initial callout and the survey for the treatment group (38.3 days) and control group (38.6 days) are not significantly different (p =0.906). The difference in time spent completing the survey, 13.1 minutes for the treatment and 13.8 minutes for the control group, is also not statistically significant (p =0.566). The similarity between the two groups is consistent with random sampling from the pool of study cases and selection into the survey being uncorrelated with treatment status.

We also look at balance across pre-treatment characteristics for the surveyed treatment and control. In Table E.1 we report, for the surveyed cases only, mean values for victim, perpetrator and household characteristics by treatment status. We find that the survey sample is balanced across treatment and control for many different pre-treatment characteristics. The only variables that come up significantly different across the two groups (at 5%) are race of the perpetrator, and an indicator for the same perpetrator in the victim's first reported domestic incident. We further test the balancing properties by regressing treatment status on all control variables. Importantly, the F-stat for joint significance of the control variables does not allow us to reject the null hypothesis that the treatment and control group are balanced (p=0.408).

A second concern is that, although balanced across treatment and control, the surveyed cases may not be representative of all cases in the administrative data. If this is the case, the treatment effect that we estimate from the survey outcomes may over- or under-represent the ITT that we would get for the outcomes from the full dataset. To explore this, we first compare the mean pre-treatment characteristics of cases in the full dataset to the same characteristics for cases for which we have a completed survey (Table E.2). A number of characteristics differ across the two groups (Column 3). Specifically, victims in the surveyed cases are more likely to be female, have fewer total recorded cases of domestic violence (although the number in the previous year is the same), and are significantly more likely to be living with the perpetrator. We further investigate this by regressing an indicator dummy, equal to 1 if the observation has a completed survey and 0 otherwise, on the pre-treatment characteristics. The coefficients of this regression are reported in column (4) of Table E.2. The number of previous domestic cases and cohabitation status remain significant predictors of a completed survey (the regression F-stat is 1.21, p=0.094).

Table E.1: Descriptive statistics and balance, surveyed sample

	Control	Treatment	Difference	N
A. Victim characteristics				
Female	0.89	0.943	0.053	214
	(0.314)	(0.233)	(0.038)	
Age	34.22	35.56	1.340	214
	(12.800)	(12.370)	(1.721)	
White	0.89	0.829	-0.061	209
	(0.314)	(0.379)	(0.048)	
Domestic cases (365 days)	2.514	2.267	-0.247	214
	(1.507)	(1.325)	(0.194)	
Registered domestic cases	8.917	11.190	2.273	214
	(7.975)	(11.580)	(1.364)	
Risk assessment score	1.202	1.267	0.065	201
	(0.590)	(0.683)	(0.087)	
B. Perpetrator characteristics				
Female	0.128	0.086	-0.042	214
	(0.336)	(0.281)	(0.042)	
Age	31.680	33.050	1.370	214
	(10.720)	(11.380)	(1.512)	
White	0.853	0.705	-0.148	198
	(0.356)	(0.458)	(0.056)	
Domestic cases (365 days)	2.780	2.219	-0.561	214
	(2.428)	(1.901)	(0.298)	
Registered domestic cases	10.170	11.280	1.110	214
	(9.375)	(9.935)	(1.321)	
C. Household characteristics				
Same victim and perpetrator†	0.587	0.438	-0.149	214
	(0.495)	(0.499)	(0.068)	
Intimate partner	0.780	0.752	-0.028	210
	(0.416)	(0.434)	(0.058)	
Cohabitation	0.706	0.676	-0.030	209
	(0.458)	(0.470)	(0.063)	
Children in household	0.716	0.600	-0.116	214
	(0.453)	(0.492)	(0.065)	
Number of children‡	1.910	1.921	0.011	141
	(0.885)	(1.067)	(0.168)	
F-stat⋆[*p-value*]		1.04 [0.408]		

Notes: This table reports variable means for cases in the sample included in the victim survey by treatment status; corresponding standard deviations are reported in parenthesis. Column *difference* reports the difference in group means; the corresponding standard error on difference is reported in parenthesis. Column *N* reports number of observations with non-missing values.

†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.

⋆ *F-stat* corresponds to the joint significance of a regression of all characteristics, plus police-beat dummy variables, on treatment status (surveyed group only).

Table E.2: Characteristics of cases with completed survey

	Mean values all cases (1)	Mean values surveyed cases (2)	Difference in means (3)	Regression of survey dummy (4)
A. Victim characteristics				
Female	0.861	0.916	0.054	0.068
			(0.022)	(0.036)
Age	34.34	34.88	0.538	0.002
			(0.960)	(0.001)
White	0.829	0.880	0.052	0.060
			(0.026)	(0.043)
Domestic cases (365 days)	2.269	2.393	0.124	-0.001
			(0.111)	(0.010)
Registered domestic cases	11.540	10.030	-1.508	0.000
			(0.785)	(0.001)
Risk assessment score	1.268	1.313	0.046	0.002
			(0.045)	(0.027)
B. Perpetrator characteristics				
Female	0.147	0.107	-0.039	-0.026
			(0.025)	(0.040)
Age	33.440	32.350	-1.091	-0.001
			(0.866)	(0.001)
White	0.802	0.843	0.041	-0.000
			(0.030)	(0.045)
Domestic cases (365 days)	2.165	2.505	0.340	0.0178
			(0.164)	(0.009)
Registered domestic cases	11.470	10.710	-0.759	-0.002
			(0.757)	(0.001)
C. Household characteristics				
Same victim and perpetrator†	0.428	0.514	0.086	0.058
			(0.038)	(0.029)
Intimate partner	0.779	0.781	0.002	-0.070
			(0.032)	(0.037)
Cohabitation	0.523	0.708	0.185	0.140
			(0.036)	(0.027)
Children in household	0.556	0.659	0.103	0.069
			(0.037)	(0.047)
Number of children‡	1.966	1.915	-0.051	-0.007
			(0.094)	(0.019)

Notes: This table reports variable means for cases in the sample included in the victim survey versus the rest of the sample; corresponding standard deviations are reported in parenthesis. Column (1) and (2) report mean values of characteristics for all cases and the surveyed cases only. The corresponding difference in these means and the standard error (in parenthesis) is reported in column (3). Column (4) reports the coefficients resulting from a regression of a dummy indicating survey completed on characteristics. Regression also includes police-beat dummy variables, and binary indicators corresponding to missing variables. Robust standard errors are reported in parentheses.
†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.

We check the sensitivity of our results reported in Section 4.2 of the main paper by rerunning the survey results using alternative estimators. These estimates are reported in Table E.3. The first three columns in Table E.3 report OLS and weighted OLS estimates: Column (1) reports the unconditional treatment-control difference in mean values for each of the survey outcomes; Column (2) reports the preferred estimates (corresponding to Figure 4 in the main paper); Column (3) reports estimates weighted to match means of the full sample across previous cases, sex of the victim, and cohabitation[7]. Across all estimates, the survey results are notably stable. There is little difference between the magnitude of weighted and unweighted estimates.

For the estimates reported in columns (1)–(3) to be representative of the full subject pool, we require that selection into the survey is random with respect to the effect of treatment assignment on outcomes. This is a relatively strong assumption. For example, it will be violated if the same factors that led the caseworker to not be able to contact the victim also led to the surveyor not being able to contact the victim.[8] In this case we would systematically exclude subjects from the survey who do not engage with the intervention.

In Column (4) of Table E.3, we report two-stage least-squares estimates, where intervention engagement is the right-hand-side variable of interest and assignment to the treatment group is the instrumental variable. In Appendix F.4 we discuss in detail two-stage least-square estimation in our setting. To interpret these estimates as local average treatment effects, for subjects who engage with both the intervention and the survey, we require

[7]This is done by dividing the surveyed cases, and all cases, into strata according to victim sex, recorded cases (four different groups), and cohabitation. We calculate the proportion of cases which fall into each strata for each of the surveyed cases and all cases. The survey weight, corresponding to which strata an individual observation falls, is the ratio of the proportion calculated for the full sample divided by the proportion for the surveyed sample.

[8]Comparing engagement across treated subjects in the survey group versus the full sample is consistent with this form of selection. The survey group has an engagement rate of 69.5%, compared to 51.2% for the full sample. Interestingly, when we look at type of engagement (phone only versus in-person visit) conditional on engagement there is no statistical difference between the surveyed group and the full sample. In the surveyed group 65.8% of engaged victims have a face-to-face visit, compared to 65.52% for the full sample.

the standard instrumental variable conditions be satisfied: 1) treatment independence, 2) treatment exogeneity, 3) treatment assignment increases intervention engagement, 4) monotonicity of treatment response. In Appendix F.4, we discuss these conditions, and how likely they are to be met on our setting, in detail. In addition to these conditions, we further require that treatment status does not affect selection into the survey; we argue above that the evidence is consistent with this condition.

Assuming these standard conditions are satisfied, the estimates reported in Column (4) are representative of LATE estimates for the full (administrative) sample if, conditional on subjects being the type who engages with the intervention when assigned to treatment (a *complier*), selection into the survey is random. This is a considerably weaker requirement then what is needed for representativeness of the ITT estimates. Furthermore, if these estimates are an unbiased representation of the full sample LATE, we can use them to get an idea about the unbiased ITT estimate, where the ITT = LATE×(engagement rate)[9]. Focusing on outcomes for which estimation precision is relatively high, we see that LATE estimates imply an ITT consistent with the OLS estimates. For example, improved stress levels would have an ITT of $-0.323 \times 0.51 = -0.165$, which is not statistically different from our OLS estimates. We interpret this as evidence that the selection bias in our OLS estimates is not so large as to significantly change the interpretation of our results.

In the interest of transparency, we conduct one final exercise to evaluate the potential selection bias in our survey results. If we do not assume that survey selection is random with respect to the treatment effect, the OLS estimates based on the survey sample only partially identify the ITT for the full sample. To get a sense of the range of the possible *true* ITT point estimates, we conduct a worst-case scenario exercise in the spirit of Manski (2007). The subjects for whom the surveyors attempted to contact are based on a random draw of 25% of the full population. Based on this we assume that this full 25% is representative

[9] Where engagement rate is the full sample value of 51%.

of the administrative sample (and therefore an ITT estimated based on the 25% will be an unbiased estimate of the ITT of the full sample.) Of these 84% participated in the survey, 16% did not.

Table E.3: Survey results, alternative estimates

	Estimates no covariates (1)	Unweighted estimates (2)	Weighted estimates (3)	2SLS (engagement) (4)
A. Non-police service use				
Visited GP due to incident	0.121	0.179	0.184	0.241
	(0.065)	(0.087)	(0.088)	(0.097)
Visited A&E due to incident	0.050	0.065	0.056	0.089
	(0.033)	(0.034)	(0.035)	(0.036)
Accessed at least one service	0.087	0.128	0.155	0.169
	(0.074)	(0.106)	(0.113)	(0.106)
Index*, service use	0.125	0.195	0.212	0.252
	(0.069)	(0.095)	(0.099)	(0.096)
B. Decrease in risk of repeat victimization				
Currently no perpetrator contact	0.199	0.194	0.201	0.267
	(0.068)	(0.081)	(0.083)	(0.093)
Willingness to report future incident	0.153	0.162	0.199	0.223
	(0.070)	(0.100)	(0.103)	(0.109)
Personal safety has improved	0.068	0.004	-0.014	0.006
	(0.068)	(0.093)	(0.097)	(0.103)
Index*, repeat victimization risk	0.246	0.210	0.230	0.289
	(0.078)	(0.098)	(0.102)	(0.112)
C. Other well-being measures				
Family life has improved	0.036	-0.046	-0.031	-0.062
	(0.069)	(0.087)	(0.092)	(0.096)
Quality of life has improved	0.101	-0.044	0.008	-0.060
	(0.068)	(0.089)	(0.093)	(0.098)
Control over life has improved	-0.054	-0.082	-0.094	-0.115
	(0.068)	(0.090)	(0.098)	(0.100)
Stress level has improved	-0.171	-0.232	-0.219	-0.323
	(0.067)	(0.085)	(0.091)	(0.095)
Quality of sleep has improved	-0.036	-0.098	-0.080	-0.136
	(0.062)	(0.077)	(0.081)	(0.085)
Mental health has improved	0.008	-0.096	-0.104	-0.134
	(0.062)	(0.081)	(0.086)	(0.091)
Index*, victim well-being	-0.052	-0.220	-0.199	-0.302
	(0.104)	(0.134)	(0.151)	(0.148)

Notes: Cells in this table report the estimated coefficient corresponding to the regression of a treatment dummy on the survey outcome labelled in each row. Outcomes from survey questions have been transformed to be binary variables in which a value of 1 indicated "improved". Column (1) reports difference in outcome between treatment and control, not conditioning on any other variables. Column (2) reports estimates for unweighted regression, including controls, corresponding to Figure 4 in the main paper. Column (3) reports estimates for weighted data, where weights have been calibrated such that the survey distribution across victim sex, number of previous cases, and cohabitation status, reflect the full sample. Column (4) reports two-stage-least square estimates in which coefficients correspond to victim engagement and treatment is used as an instrument (see Appendix F.4 for a detailed explanation). The first stage excluded F-stat for regressions in Column (4) is 137.6. Regression controls include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing variables. Robust standard errors are reported in parentheses. *Index variables are calculated following Anderson (2008), as described in Section 4.2 of the main text.

For each of the survey outcomes, denoted by S, the ITT can be specific as:

$$E(S_1-S_0) = E(S_1-S_0|survey = 1)P(survey = 1)+E(y_1-y_0|survey = 0)(1-P(survey = 1)) \tag{E.1}$$

where S_1 and S_0 denote outcomes for the treatment and control groups, $survey$ is an indicator equal to 1 for subjects who completed a survey and 0 otherwise. We know that $P(survey = 1) = 0.84$ and $E(S_1 - S_0|survey = 1)$ corresponds to the estimates reported in Column 2, Table E.3. We bound the above equation with the two extreme assumptions on the value of survey responses for subjects who do not complete a survey, $E(S_1 - S_0|survey = 0)$. The lower bound assumes that control subjects will always provide an affirmative response to survey questions, while treatment treatment subjects will always provide a negative response to survey questions, such that $E(S_1 - S_0|survey = 0) = -1$. The upper bound assumes that control subjects will always provide a negative response to survey questions, while treatment treatment subjects will always provide a positive response to survey questions, such that $E(S_1 - S_0|survey = 0) = 1$. The extreme bounds on our point estimates are therefore determined by

$$E(S_1 - S_0) = E(S_1 - S_0|survey = 1)P(survey = 1) \pm (1 - P(survey = 1)). \tag{E.2}$$

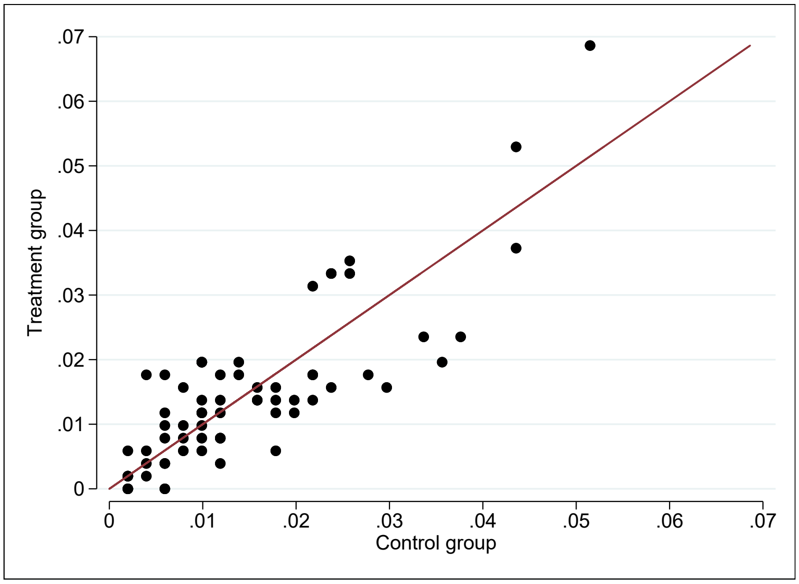
We report these bounds in Table E.4. While we cannot rule out these extremes based on the information we have, intuitively they seem highly unlikely. For this reason we also report point estimates based on the more plausible assumption that there was no treatment effect for the $survey = 0$ group. For example, this will be the case if the $survey = 0$ subjects also do not engage with, and benefit from, the intervention when assigned to treatment. The values under this assumption are lower in magnitude than the estimated OLS results reported in Table E.3, but not dramatically so.

Table E.4: Survey results, extreme bounds on ITT point estimates

	Lower bound $ITT(survey = 0)$ $= -1$	$ITT(survey = 0)$ $= 0$	Upper bound $ITT(survey = 0)$ $= 1$
	(1)	(2)	(3)
A. Non-police service use			
Visited GP due to incident	-0.010	0.150	0.310
Visited A&E due to incident	-0.105	0.055	0.215
Accessed at least one service	-0.052	0.108	0.268
B. Decrease in risk of repeat victimization			
Currently no perpetrator contact	0.003	0.163	0.323
Willingness to report future incident	-0.024	0.136	0.296
Personal safety has improved	-0.157	0.003	0.163
C. Other well-being measures			
Family life has improved	-0.199	-0.039	0.121
Quality of life has improved	-0.197	-0.037	0.123
Control over life has improved	-0.229	-0.069	0.091
Stress level has improved	-0.355	-0.195	-0.035
Quality of sleep has improved	-0.242	-0.082	0.078
Mental health has improved	-0.241	-0.081	0.079

Notes: Cells in this table report bounds on the ITT point estimates under three extreme assumptions about the treatment effect for subjects who do not respond to the survey, in the spirit of Manski's worst case scenario bounds (Manski, 2007). Baseline point estimates reported in Column 2, Table E.3.

Figure F.1: Balance of treatment and control groups by police-beat area



Notes: Each marker in this figure represents one of 68 beats in the Leicestershire police area. Markers plot the proportion of cases for the treatment group (*y-axis*) versus the portion of cases in the control group (*x-axis*) for each beat. The solid red line shows a perfectly equal distribution across beat areas. We fail to reject the null hypothesis that the distribution of cases across beats is identical for the two groups; $\chi^2(68) = 55.8$ ($p = 0.855$).

Appendix F Additional analysis

F.1 Treatment-control group balance across geography

The Leicestershire police force is made up of 92 beats, which define the geographic areas to which officer teams are assigned to patrol. 68 of these beats are represented in the data used in this study. In this section, we investigate the distribution of cases in the treatment and control group across these beat areas. In Figure F.1 we scatter the proportion of treatment group cases in each police beat area by the proportion of control group cases in each police beat area. From a visual inspection we do not find any large or systematic differences in the distribution of cases by treatment status. Consistent with this, in a formal test we are unable to reject the null hypothesis that the two groups have the same distribution across police beats ($\chi^2(68) = 55.8,\ p \geq 0.855$).

F.2 Intervention engagement and victim, perpetrator and household characteristics

In Table 2 of the main paper, we report averages of treatment group characteristics according to engagement with the intervention. In this appendix section, we look at the joint significance of these characteristics for predicting engagement. We also look at what characteristics tell us about why engagement fails—i.e. the caseworker fails to establish contact versus victims do not engage when contacted. For the treatment group subjects, we regress on characteristics, the three binary outcomes taking the following values: a) equal to one for subjects who are contacted by the caseworker and engage with the intervention (contacted and engaged), and zero otherwise; b) equal to one for subjects who are contacted by the caseworker (contacted), and zero otherwise; c) for the subset of subjects who are contacted, equal to one for subjects who engage and zero otherwise (engagement conditional on contact). Coefficients for each regression are reported in Figure F.2. For comparability across characteristics, we transform regressor variables into standard deviations; coefficients reflect the percentage point change in the outcome for a standard deviation change in the characteristic.

Several characteristics stand out as noteworthy. Sex of the perpetrator is significantly associated with engagement. Engagement rates are lower in cases where the perpetrator is female. However, this appears to be due to a significant decrease in the likelihood of a caseworker making contact (p=0.059); conditional on making contact, sex of the perpetrator does not have a significant association with engagement (p=0.755). It is also interesting that the sex of the victim does not appear to be as significant a determinant for engagement. Contact by the caseworker is independent of age, but a one standard deviation increase in either victim or perpetrator age (approximately 12 years) is associated with more than a 5 percentage point increase in the engagement rate when contact is made (p=0.074 and p=0.080 for victim and perpetrator). Victims with more previous cases are less likely to engage when contacted; a standard deviation increase in previous cases (approximately 1.5 cases) is associated with a 5.0 percentage point decrease in engagement (p=0.075). Finally, a higher risk assessment score of the responding officers is significantly associated with an increase in engagement. This is both through a greater likelihood of making contact (p=0.011), and to a lesser extent, through greater engagement once contact is made (p=0.148).

F.3 Timing of repeat domestic incidents

It is possible that the intervention led to a temporary change in the pattern of reported repeat domestic incidents. To examine this, we look at the timing of repeat incidents across treatment and control.

We employ two methods to test for treatment-control differences in the timing of repeat incidents. First, in Figure F.3 we examine the timing of a repeat domestic incident across the treatment and control group using Kaplan-Meier estimates of the survivor function for the treatment and control groups. In this framework a fail is identified by the first repeat police-incident. The survivor functions suggest that the treatment group has repeats sooner than the control group, and over the two year period is more likely to have a repeat incident. However, a log-rank test fails to reject the equality of the two curves for the treatment group and the control group ($\chi^2_{(1)} = 1.61$).

As a second method, we look for differences in the timing between subsequent reported domestic instances, for the first five reports over the two-year period since the initial police callout. We report the mean number of days between reported incidents in Figure F.4, for all repeats (left panel) and for victims that experience at least five repeats (right panel). Differences between the treatment and control group in timing of repeats are small and statistically insignificant. Furthermore, there does not appear to be a systematic difference in the direction of these differences.

Based on this analysis, we are unable to detect any differences in the timing of police-reported domestic incidents between the treatment and the control group.

F.4 Local average treatment effects

In addition to the intention to treat estimates reported in the main paper, we also estimate a local average treatment effect (LATE) reflecting the treatment effect for victims who engage with the intervention. We define treatment engagement in Section 3.3 of the main paper. We calculate the LATE estimates using a two-stage least squares estimator as specified below:

$$engage_i = \lambda_0 + \lambda_1 treat_i + X_i' \Lambda + \upsilon_i \tag{F.1}$$

$$S_i = \gamma_0 + \gamma_1 \widehat{engage}_i + X_i' \Gamma + \hat{e}_i \tag{F.2}$$

In the first stage (Equation (F.1)), we regress an indicator variable for intervention engagement, $engage_i$, on an indicator for treatment group status, $treat_i$. In the second stage, we regress the outcome of interest on the first-stage predicted value of intervention engagement.

Our interpretation of the estimated $\hat{\gamma}_1$ as a local average treatment effect is subject to four assumptions.

1. Independence of the instrument: The instrument is uncorrelated with unobserved characteristics.

$$E(e_i | treat_i) = 0 \tag{F.3}$$

2. Exclusion restriction: Conditional on intervention uptake, treatment status has no

effect on outcomes.

$$E(S_i|engage_i = 1, \, treat_i = 0) = E(S_i|engage_i = 1, \, treat_i = 1) \tag{F.4}$$

3. First stage: Treatment has a non-zero effect on uptake of the intervention.

$$E(engage_i|treat_i = 1, X_i) - E(engage_i|treat_i = 0, X_i) \neq 0 \tag{F.5}$$

4. Monotonicity: Subjects are never less likely to take up the intervention when assigned to the treatment group than they would be if assigned to the control group.

$$E(engage_i|treat_i = 1, X_i) - E(engage_i|treat_i = 0, X_i) \geq 0 \tag{F.6}$$

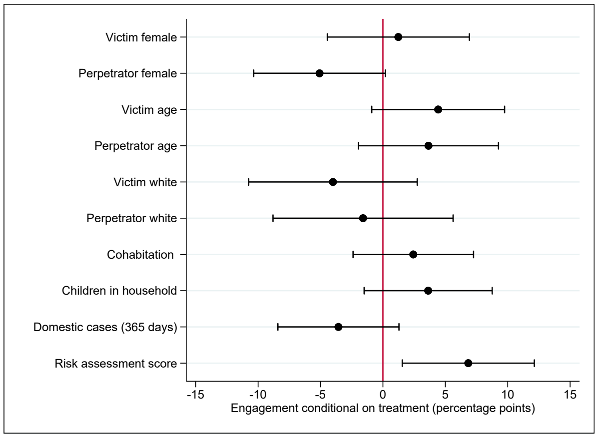
The first assumption is satisfied from the randomization of cases into treatment and control. The second assumption requires that it is only through the intervention that treatment status affects the outcomes. As discussed in Section 3.4 of the main paper, the design features of this RCT ensure that it is highly likely that this assumption is satisfied. The third assumption states that treatment status has a non-zero effect on engagement with the intervention. This assumption is testable from the first-stage regression. The first stage instrument is strong, being in the treatment group increases the likelihood of engagement with the intervention by 51.7%, with an excluded variable $F = 537$ and an excluded variable $R^2 = 0.348$ (Column 1, Table F.1). The final assumption, monotonicity, requires that the treatment does not lead victims to be less likely to engage with the intervention than they would have been had they been assigned to the control group. The design of this RCT is such that victims in the control group do not receive the opportunity to engage with the intervention, therefore the monotonicity assumption is satisfied by design.

In Table F.1, we report two-stage least squares estimates for outcomes corresponding to the estimates reported in Tables 3–5 of the main paper. For statements made to police, the

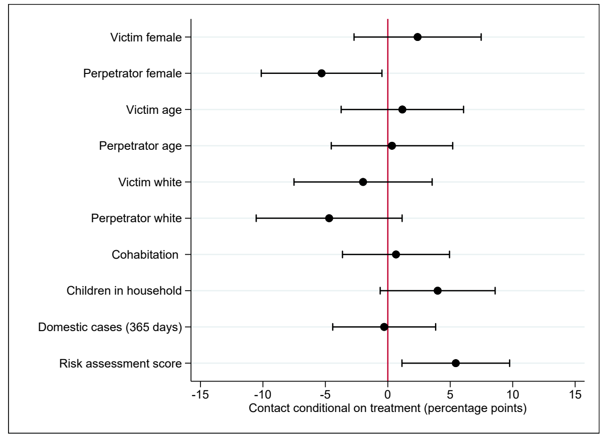
LATE estimate suggests that victims who engaged with the intervention are 12.6 percentage points less likely to provide a statement to the police (p = 0.012). This is a large effect, and corresponds to a 42.1% decrease relative to statement provision by the control group.[10] Estimates for other outcomes are larger in magnitude than the ITT estimates reported in the main paper, but have the same sign. Overall, the qualitative story is very similar to that from the main paper. For example, for those who engage with the intervention, treatment leads to a 4.5 percentage points increase (6.0% relative to the control group mean, p=0.369) in the probability of a repeat police callout, but a decrease of 1.113 units (18.8% relative to the control group mean, p=0.076) in the average risk assessment. This is consistent with the intervention having a weak positive effect on the number of repeat incidents, but with the average severity of an incident decreasing.

[10] 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.

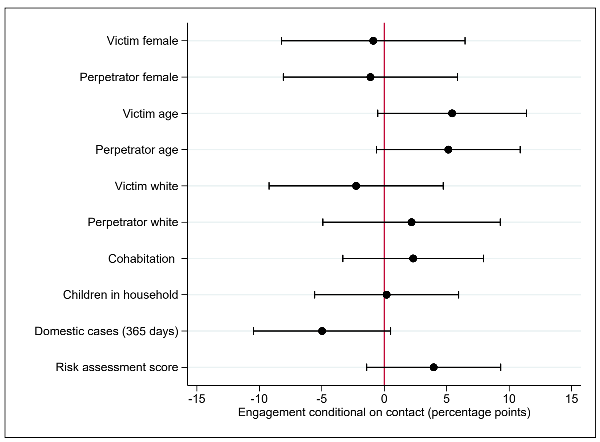
Figure F.2: Characteristics by intervention engagement



(a) Contacted and engaged



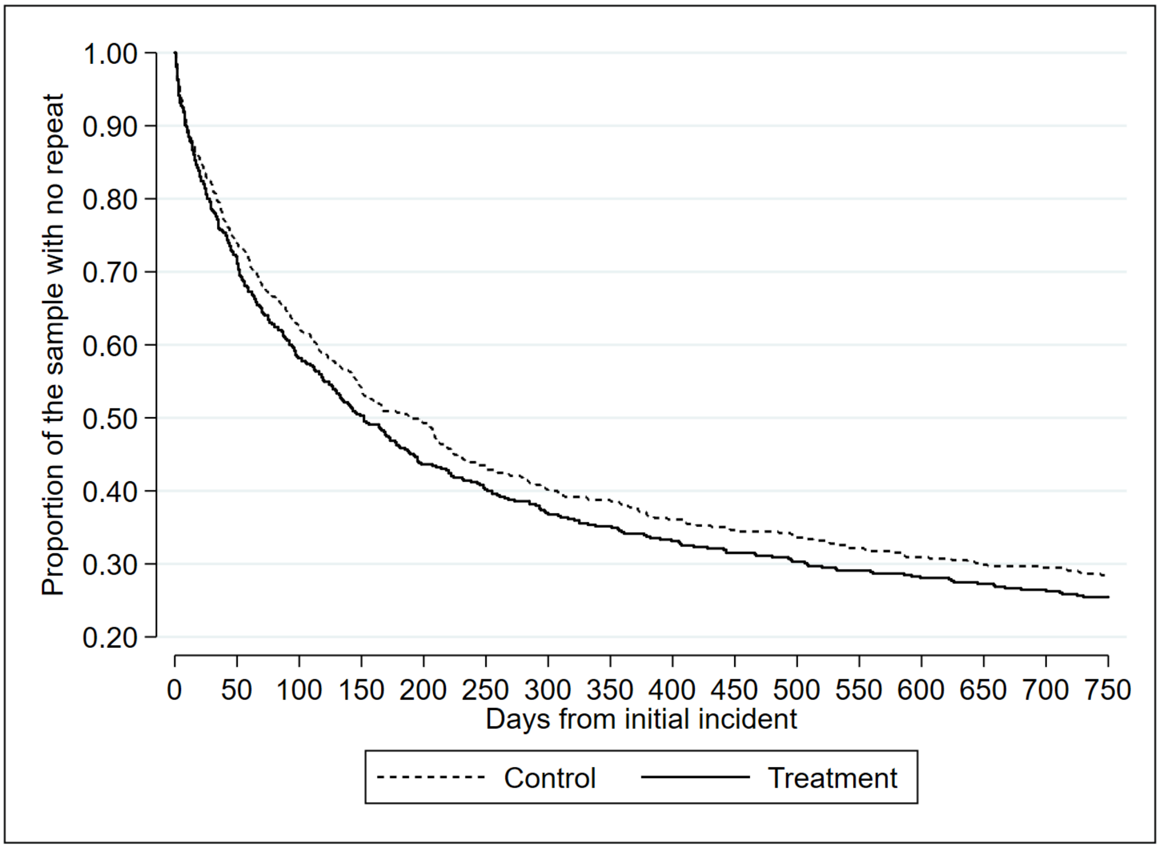
(b) Contacted



(c) Engaged conditional on contact

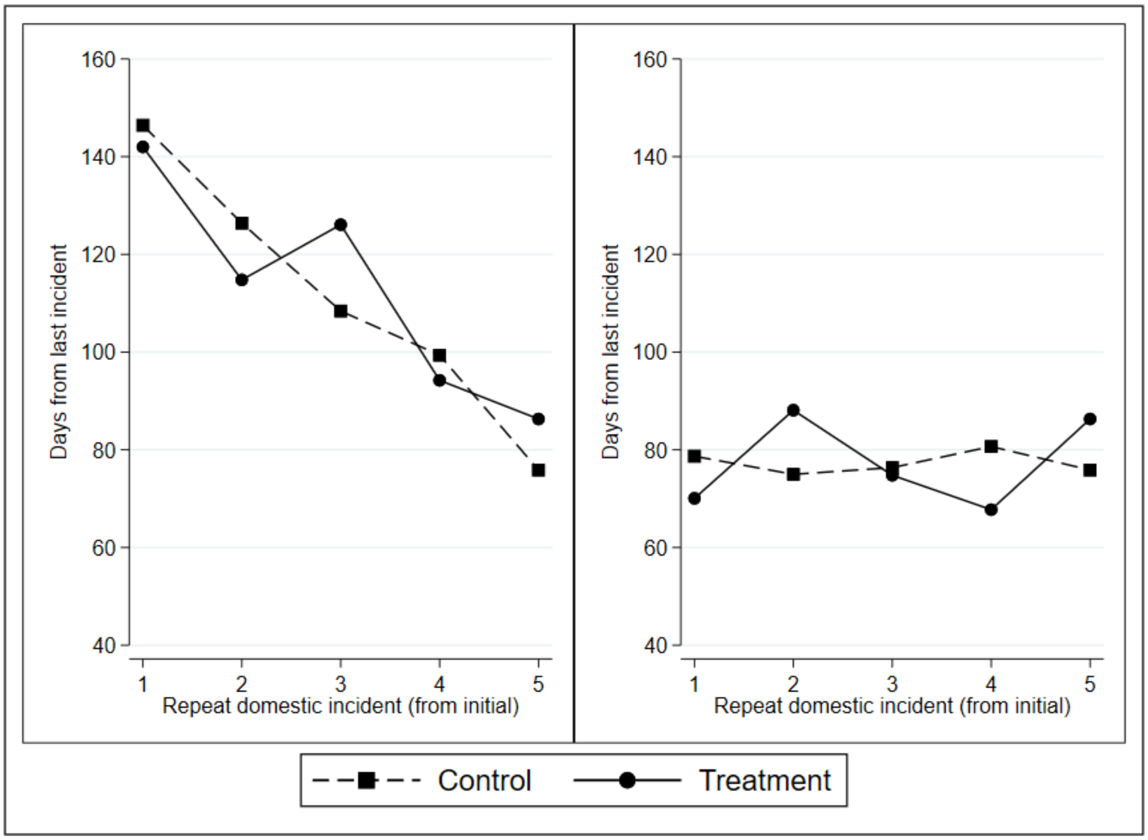
Notes: These figures report the coefficients corresponding to a regression of binary indicators for three outcomes for cases in the treatment group: a) Contacted by the caseworker and engaged with the intervention ($R^2 = 0.1693$, $N = 510$); b) Contacted by the caseworker ($R^2 = 0.1986$, $N = 510$); c) Engaged with the intervention conditional on contact ($R^2 = 0.2269$, $N = 368$). All regression include the full set of control variables (see main text) including dummies for missing variables and police-beat dummies. Reported control variables are in standard deviations; coefficients reflect the percentage point change in the outcome for a standard deviation change in the characteristic. Points reflect point estimates of coefficients, bars denote 95% confidence intervals.

Figure F.3: Kaplan-Meier estimates of the time to repeat from initial incident



Notes: This figure displays estimated Kaplan-Meier survival functions for the treatment group (solid line) and the control group (dashed line). A *fail* is identified by the first repeat police incident. A log-rank test fails to reject the null hypothesis that the survival function is the same for treatment and control groups ($\chi^2_{(1)} = 1.61$).

Figure F.4: Number of days to next repeat, first five repeats



Notes: This figure documents the average number of days between police-reported incidents by treatment status. The left figure shows the average number of days between each incident for all reported cases. Observations are 753, 552, 402, 289, and 210 for repeats 1–5 respectively. The right figure includes only cases for which we observe at least five repeats in the two-year period. 210 observations for all days.

Table F.1: Local average treatment effect estimates

	First stage	Statement provided	Repeats ≥ 1	Total repeats	Total repeats conditional on ≥ 1	DASH score	MARAC threshold	Perpetrator arrested
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Engagement		-0.126	0.045	0.446	0.379	-1.113	-0.053	-0.104
		(0.050)	(0.050)	(0.407)	(0.464)	(0.627)	(0.066)	(0.076)
Treatment	0.517							
	(0.023)							
Victim female	0.013	-0.002	0.085	0.298	0.077	0.099	-0.049	-0.008
	(0.040)	(0.041)	(0.048)	(0.341)	(0.422)	(0.508)	(0.072)	(0.069)
Perpetrator female	-0.080	-0.065	-0.050	-0.221	0.096	-1.932	-0.113	-0.099
	(0.041)	(0.038)	(0.046)	(0.324)	(0.380)	(0.529)	(0.060)	(0.063)
Victim white	-0.033	0.083	0.056	0.013	-0.216	-0.766	-0.237	-0.095
	(0.041)	(0.046)	(0.045)	(0.425)	(0.452)	(0.589)	(0.066)	(0.069)
Perpetrator white	-0.047	-0.087	0.031	0.632	0.708	0.560	0.163	0.052
	(0.043)	(0.048)	(0.044)	(0.395)	(0.418)	(0.523)	(0.056)	(0.066)
Cohabitation	0.022	0.124	-0.053	-0.714	-0.461	0.461	0.007	0.031
	(0.026)	(0.027)	(0.027)	(0.232)	(0.266)	(0.350)	(0.036)	(0.042)
Children in household	0.037	0.008	0.034	-0.177	-0.462	1.034	-0.023	0.075
	(0.025)	(0.028)	(0.029)	(0.252)	(0.302)	(0.368)	(0.039)	(0.043)
Domestic cases (365 days)	-0.010	-0.005	0.014	0.262	0.241	-0.139	0.015	0.012
	(0.008)	(0.008)	(0.009)	(0.075)	(0.082)	(0.102)	(0.011)	(0.012)
Risk assessment score	0.051	0.283	-0.030	-0.018	0.085	1.937	0.109	0.079
	(0.022)	(0.028)	(0.027)	(0.203)	(0.231)	(0.362)	(0.040)	(0.038)
Control group mean	0.000	0.299	0.749	2.681	3.582	6.039	0.123	0.457
N	1015	1015	1015	1015	775	522	535	639

Notes: Column (1) reports estimates for a linear (first stage) regression of engagement dummy on treatment group dummy. Columns (2)–(8) report two-stage least squares estimates corresponding to outcomes reported in Tables 2–4 of the main paper. Regression controls include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing variables. Robust standard errors are reported in parentheses.

F.5 Treatment effect heterogeneity

We rerun the regression of tables 3–6 in the main paper, allowing for the treatment effect to vary by the risk assessment score reported for the initial callout. To do this, we create a high-risk dummy variable equal to 0 when the risk assessment score is 1 (the lowest value), and equal to 1 otherwise. This dummy is interacted with treatment in Equation (1) of the main paper. For each outcome we estimate:

$$S_i = \lambda_0 + \lambda_1 treat_i + \lambda_2 highrisk_i + \lambda_3 treat_i \times highrisk_i + X_i'\Lambda + u_i, \tag{F.7}$$

where $highrisk_i$ is the dummy variable for i, u_i reflects the unobserved influences on the outcome, and X_i is as previously specified.[11] Estimates corresponding to λ_1, λ_2, and λ_3 are reported in Table F.2.

The strongest result from these regressions comes from the effect of treatment on the provision of a statement to police. For $highrisk = 0$ cases, the victim is 7.4 percentage points less likely to make a statement (p = 0.011), consistent with ITT reported in Table 3. This is a 32.0% decrease relative to the $highrisk = 0$ control group cases. The interaction of treatment with the high-risk indicator results in a positive coefficient of 8.3 (p = 0.249), suggesting a total effect of treatment for the $highrisk = 1$ cases of a 0.9 percentage point increase in statements to police. This total effect is not statistically significant and only a 2.3% increase over the control group mean of 53.0% for $highrisk = 1$ cases. We interpret this as evidence that the statement-making response to treatment is coming from the cases identified as lower risk.

[11] We exclude from these regressions the risk assessment score, which is highly correlated with the high-risk dummy. Including the score reduces the magnitude and significance of the $highrisk$ coefficient, but does not have a substantive effect on the $treat$ or $treat \times highrisk$ coefficients.

Table F.2: Heterogeneous treatment effect by responding officer risk assessment

		Repeat police-reported DV			Severity at repeat police callouts			Punitive actions against perpetrator		
	Statement	Repeats ≥ 1	Total repeats	Total repeats conditional on ≥ 1	DASH score	MARAC threshold	Perpetrator arrested	Perpetrator arrested	Perpetrator charged	Perpetrator sentenced
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	-0.074	0.012	0.125	0.109	-0.246	-0.039	-0.042	-0.020	-0.021	-0.013
	(0.028)	(0.030)	(0.252)	(0.294)	(0.392)	(0.039)	(0.047)	(0.028)	(0.022)	(0.018)
High-risk	0.285	-0.055	-0.163	-0.037	2.427	0.094	0.095	0.252	0.117	0.071
	(0.054)	(0.048)	(0.338)	(0.400)	(0.669)	(0.072)	(0.072)	(0.053)	(0.043)	(0.033)
Treatment × High-risk	0.083	0.046	0.404	0.370	-1.473	0.059	-0.043	0.076	0.091	0.054
	(0.074)	(0.069)	(0.534)	(0.631)	(1.001)	(0.104)	(0.101)	(0.076)	(0.065)	(0.053)
Control group mean (High-risk= 0)	0.231	0.756	2.73	3.607	5.454	0.115	0.428	0.200	0.105	0.067
	(0.021)	(0.022)	(0.167)	(0.195)	(0.268)	(0.016)	(0.032)	(0.020)	(0.016)	(0.012)
N	1015	1015	1015	775	522	535	639	1015	1015	1015
R^2	0.211	0.114	0.098	0.128	0.267	0.174	0.140	0.168	0.122	0.103

Notes: This table reports estimates for the regression specified in Tables (2)–(5) allowing for heterogeneous treatment effect by risk assessment. *High-risk* is a binary variable taking a value of 1 if the responding officer risk assessment is high and and a value of 0 otherwise. All regression include control variables as specified in Tables (2)–(5), with the exception of the risk assessment score. Robust standard errors are reported in parentheses.

The heterogeneity of results across the outcomes reflecting the quantity and severity of repeat domestic incidents, suggest that the treatment effects reported in tables 4 and 5 are stronger for the high-risk cases. For example, consider the average DASH score for repeat police call-outs. For low-risk cases, the treatment group has an average DASH score of 0.246 points, or 5.3%, lower than the control group ($p = 0.477$). For high-risk cases, the treatment group has an average DASH score of 1.473 points, which is 18.9% lower than the control group ($p = 0.139$). These results are consistent with the treatment having a heterogeneous effect across cases according to their reported risk level: lower statement provision among lower-risk cases and a higher reporting rate among the higher-risk cases. We also see differences by risk assessment in the estimated treatment effects for the perpetrator outcomes in the initial incident. Low-risk cases see a decrease in arrests, charges and sentencing for treatment relative to control, and the opposite sign for high-risk cases. Although all estimates are statistically insignificant, the magnitudes are on the order of 10%–20% relative to low risk control groups means.

We also repeat the above exercise, providing heterogeneous treatment effects for the survey data, as for the above estimates. The results of this exercise are reported in Table F.3, where the columns provide estimates for each survey outcome corresponding to treatment, the high-risk dummy and the interaction of treatment and high-risk. In the final column, we report the means for control group subjects who are not assessed as high-risk. While we focus our discussion below largely on the magnitude of the point estimates, these estimates are noisy; interpretation should be done cautiously.

As with the administrative data, we see some interesting differences in treatment responses by risk assessment. For example, we find that the index reflecting service use is slightly higher for the high-risk group ($p=0.652$). However, the increased visits to a general practitioner for medical attention are largely coming from the subjects who are not assessed as high-risk ($p=0.029$), while visits to accidents and emergency department are 10.5 percentage points higher for high-risk subjects than subjects who are not high-risk (p=0.215).

There is little difference in the index for repeat victimization by risk assessment. However, the positive treatment effects for reduced perpetrator contact and willingness to report future incidents can be entirely attributed to subjects assessed as lower risk (p= 0.012 and p=0.048). The high-risk subjects are more likely than others to respond to treatment by reporting their personal safety having improved (p= 0.132).

Perhaps the most interesting results come from the survey measures of well-being. Consistent with the results reported in Figure 4 of the main paper (appendix Table E3), subjects who are not assessed as high-risk report a worsening across all measures of well-being. Further, the magnitude of these negative results are more than double relative to the homogeneous results. For example, subjects who are not assessed as high-risk are 26.5 percentage points less likely to report improved stress levels when in treatment (p=0.007), compared to 17.1 percentage points less in the heterogeneous estimates. This estimates reflects a 53% decrease over the control group mean. Subjects assessed as high-risk were more likely to report improvements in well-being across several measures. For example, compared to the low-risk cases, the high-risk subjects are 21.1 percentage points more likely to report an improvement in quality of life in treatment than in control, corresponding to a 43% improvement (p=0.522). However, the treatment effect for high-risk subjects on reported stress improvement is still negative, a decrease of 17.2 percentage points (p=0.3328) relative to the high-risk control group.

Table F.3: Survey outcomes, heterogeneous treatment effects

	Treatment	High-risk	Treatment × High-risk	Control group mean High-risk = 0
	(1)	(2)	(3)	(4)
A. Non-police service use				
Visited GP due to incident	0.218	0.149	-0.142	0.259
	(0.099)	(0.146)	(0.179)	(0.048)
Visited A&E due to incident	0.047	-0.032	0.105	0.047
	(0.036)	(0.047)	(0.084)	(0.023)
Accessed at least one service	0.132	0.032	0.011	0.530
	(0.126)	(0.184)	(0.207)	(0.062)
Index*, service use	0.171	-0.089	0.089	0.056
	(0.103)	(0.180)	(0.197)	(0.061)
B. Decrease in risk of repeat victimization				
Currently no perpetrator contact	0.243	0.279	-0.214	0.341
	(0.095)	(0.148)	(0.185)	(0.052)
Willingness to report future incident	0.213	0.109	-0.194	0.341
	(0.107)	(0.202)	(0.239)	(0.053)
Personal safety has improved	-0.072	-0.186	0.300	0.552
	(0.098)	(0.163)	(0.198)	(0.054)
Index*, repeat victimization risk	0.226	0.173	-0.073	-0.171
	(0.111)	(0.198)	(0.226)	(0.066)
C. Other well-being measures				
Family life has improved	-0.025	0.070	-0.083	0.435
	(0.099)	(0.166)	(0.206)	(0.054)
Quality of life has improved	-0.101	-0.170	0.211	0.424
	(0.101)	(0.159)	(0.216)	(0.054)
Control over life has improved	-0.133	-0.085	0.195	0.600
	(0.098)	(0.169)	(0.098)	(0.053)
Stress level has improved	-0.265	-0.027	0.093	0.494
	(0.096)	(0.159)	(0.200)	(0.054)
Quality of sleep has improved	-0.145	-0.153	0.176	0.329
	(0.088)	(0.130)	(0.157)	(0.051)
Mental health has improved	-0.136	-0.148	0.161	0.294
	(0.085)	(0.137)	(0.179)	(0.050)
Index*, victim well-being	-0.299	-0.151	0.280	0.061
	(0.156)	(0.240)	(0.317)	(0.089)

Notes: Cells in this table report the estimated coefficient corresponding to the regression of a treatment dummy on the survey outcome labelled in each row, allowing for heterogeneous treatment effects by risk of escalation. Outcomes from survey questions have been transformed to be binary variables in which a value of 1 indicated "improved". Columns report: (1) coefficients corresponding to the treatment dummy, (2) coefficients corresponding to the high-risk dummy, (3) coefficients corresponding to the interaction of the treatment dummy and high risk dummy, (4) mean value of outcome for high-risk= 0 control group. Regression controls include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing variables. Robust standard errors are reported in parentheses. *Index variables are calculated following Anderson (2008), as described in Section 4.2 of the main text.

Appendix G Details of intervention cost analysis

Here we provide supplementary details on the cost analysis of Section 6 in the main paper.

The estimated incremental cost, over the six-month period between November 2014 and April 2015, of providing the intervention came to £64,631. This figure includes overhead costs not explicitly included during the experiment, as this was provided in-kind by Leicestershire Police. The primary incremental cost from the implementation of Project 360 arises from the labour involved. This comprises three full-time caseworkers, at a total cost of £35,217, plus £2,756.52 employer National Insurance contributions. We also cost for a part-time supervisor and programme coordinator, at a total cost of £7,333. We allow for £16,574.49 in overheads, provided in-kind by Leicestershire Police. This covers the cost of office space, communication support, computers, etc. in line with overheads paid for full time police officers. An estimated £2,550 was spent on car hire, fuel and parking. Finally, £200 was spent on security upgrades for victims (locks and alarms).

Over this period, the three caseworkers were assigned 510 cases, which works out at 4.9 cases per working day (based on 104 working days in the six month period), or 1.6 cases per worker per day. Using the total cost of the programme, this means that the intervention cost £126.73 per case. From all cases in the treatment group, contact was successfully made with 402 victims, 260 of whom engaged with the intervention. Based on this, we can work out the intervention cost of £248 per victim engagement. The cost of the intervention may be expected to come down over time as caseworkers and supervisors learn new and more efficient processes for delivery of the service.

We calculate the cost of police time based on official figures from the National Police Chief Council (NPCC, 2019) on the costing of police services. The full cost of a full-time officer at the rank of police constable (the lowest rank) in 2017 is £88.662; and £107,517 for a police sergeant (the next higher rank). These cost include employer National Insurance contributions, and the police-specific allowances and pension contributions, and direct overheads, for example police uniform, insurance etc. Based on 208 net working days and 7.25 productive hours per shift this equates to an hourly cost of a full-time officer at the rank of police constable in 2019 of £58.99 and £71.50 for police sergeant, respectively.

We calculate the savings to police time from the intervention through the reduction in statements (Table 3, main paper), which would have triggered further police investigations. Based on the figures above and the estimated reduction in statements ($0.065 \times 510 = 33$), we calculate the cost savings from the reduced demand on police officer time. Using a 20 hours per investigation provided as benchmark by Leicestershire Police Force, the project saved a total of $£58.99 \times 33 \times 20 = £38,980.71$ worth of police hours based on police constable, and £47,270.41 based on sergeant costing, a saving of £76.43 and £92.69 per victim, respectively. Alternatively, one can calculate the number of hours of police time per investigation required to break even with cost of the intervention. For this, we divide the intervention costs over the cost savings calculated based on our estimates in the reduction of statements. For police investigation costs based on the salary of Police Constable the number of hours to break even is 33 hours $(64,631/(58.79 \times 0.065 \times 510))$ or 27 hours for a sergeant salary $(64,631/(71.30 \times 0.065 \times 510))$.

Appendix H One month victim survey

Leicestershire Pilot Domestic Abuse Survey

Before contacting the victim, complete questions 0 to 3

Q1 Name
____
____
____

Q2 Reported
____
____
____

Q3 Crime Number:
____

Q4 Is there a safe telephone number
- ❑ Yes - ok to proceed with survey
- ❑ Yes - but a different person answered the phone
- ❑ No
- ❑ Yes - But third / final attempt made & no reply / Faulty Phone number or no phone number.

Q5 Is the phone number a....
- ❑ Land Line number
- ❑ Mobile number

Hello, could I speak to {Q0.a} please?

INTERVIEWER: If another person in the household answers the phone and wishes to know what we are calling about say: "I am calling to conduct a survey, it's not urgent or important and we're not trying to sell anything, so I'll try again later thank you."

My name is ____ from Leicestershire Police.

Q6 Is it safe to speak to you now?
- ❑ Yes
- ❑ No

Q7 For the purpose of ensuring your safety, can I ask is there any possibility that this call could be overheard by the person who caused you harm?
- ❑ Yes
- ❑ No

I would like to conduct a survey with you about your experience, when would be a better time to call you when you can't be disturbed or overheard?

Arrange a different time to call the person back. If however the respondent advises that it is fine to continue with the call inform them that we are not able to continue with the call as they have advised that there is a possibility of being disturbed by the person who caused the harm.

Text to introduce the survey:
I would like to conduct a survey with you following the report you made to the police on (INSERT DATE), and what affect this has had on you. The interview will take between 5-10 minutes. This call may be recorded for training and quality purposes.

With your permission, your responses and information about your case will be stored and shared with the University of Leicester for research purposes. Your name, personal contact details and other identifying information will not be shared and will be treated in the strictest confidence.

The goal of the research is to understand how police response to domestic incidents can be improved.

Participation in this survey is voluntary. You can refuse to answer any questions, or stop the survey at any time.

If respondent would like to talk to someone at Leicestershire Police to check that this survey is genuine or for any other reason connected with this survey the contact details are:

telephone - XXXXX
or email XXXXX

I'm calling about the domestic incident that was reported on {Q0.b}.

Q8 Are you happy for me to proceed and ask you some questions?
(PAUSE FOR RESPONSE)
- ❑ Yes
- ❑ No

Reason for not taking part (DO NOT ASK) ____
____
____

Q9 In case we get cut off can I check your current location - are you at home?
- ❑ Yes
- ❑ No

Please can I take the details of your current location i.e address inc. postcode ____

This survey will take between 5 -10 minutes, the questions are split into 3 sections and will relate to your experience. The questions are statements and the answers will be read out to you. Please choose the answer that best fits how you feel.

ARRANGE TO CALL BACK AT A LATER TIME/DATE, IF REQUIRED AND TERMINATE THE CALL - DO NOT REFUSE

I'd like to begin by asking a few questions around how you are feeling:

Q10 Since making this report, my safety has...
- ❑ Improved a lot
- ❑ Improved a little
- ❑ No Difference
- ❑ Declined a little
- ❑ Declined a lot
- ❑ Don't know

Q11 Since making this report, my control over my life has...
- ❑ Improved a lot
- ❑ Improved a little
- ❑ No Difference
- ❑ Declined a little
- ❑ Declined a lot
- ❑ Don't know
- ❑ *Partially Completed*

Q12 Since making this report, my stress levels have...
- ❑ Improved a lot
- ❑ Improved a little
- ❑ No Difference
- ❑ Declined a little
- ❑ Declined a lot
- ❑ Don't know
- ❑ *Partially Completed*

Q13 Since making this report, my quality of sleep has...
- ❑ Improved a lot
- ❑ Improved a little
- ❑ No Difference
- ❑ Declined a little
- ❑ Declined a lot
- ❑ Don't know
- ❑ *Partially Completed*

Q14 Since making this report, my mental health has....
- ❑ Improved a lot
- ❑ Improved a little

- ❑ No Difference
- ❑ Declined a little
- ❑ Declined a lot
- ❑ Don't know
- ❑ *Partially Completed*

Q15 Since making this report, my family life has....

- ❑ Improved a lot
- ❑ Improved a little
- ❑ No Difference
- ❑ Declined a little
- ❑ Declined a lot
- ❑ Don't know
- ❑ *Partially Completed*

Q16 Since making this report, the quality of my life has...

- ❑ Improved a lot
- ❑ Improved a little
- ❑ No Difference
- ❑ Declined a little
- ❑ Declined a lot
- ❑ Don't know
- ❑ *Partially Completed*

Now, I am going to ask you a few questions about the other person in relation to the incident that you reported:

Q17 I currently have ongoing contact with this person

- ❑ Agree
- ❑ Disagree
- ❑ *Partially Completed*

Q18 The reason for the ongoing contact is:

- ❑ Children
- ❑ Family and Social Networks
- ❑ Legal Proceedings
- ❑ Financial Arrangements
- ❑ Suspect seeks contact
- ❑ Other
- ❑ *Partially Completed*

Please specify: ____
____
____

Q19 I have attempted to leave this person permanently in the past.

- ❑ Agree
- ❑ Disagree
- ❑ Don't Know
- ❑ *Partially Completed*

I would now like to ask you a few questions around Help & Support

Q20 As a direct result of this report, I have...

	Yes	No	Prefer not to say
Visited my GP	❑	❑	❑
Visited A&E (Accident and Emergency Department)	❑	❑	❑

Q21 I feel confident in knowing how to access help and support

- ❑ Agree
- ❑ Disagree
- ❑ Don't know
- ❑ N/A
- ❑ *Partially Completed*

Q22 I am aware of independent organisations that may be able to offer support and assistance.

- ❑ Agree
- ❑ Disagree
- ❑ Don't know
- ❑ N/A
- ❑ *Partially Completed*

Q23 Which independent organisations in particular? (DO NOT READ OUT THE GROUPS)

- ❑ SAFE
- ❑ LWA
- ❑ WALL
- ❑ Refuge / Accommodation
- ❑ Outreach
- ❑ IDVA
- ❑ Helpline
- ❑ Family Support
- ❑ Group Programme
- ❑ One to one support
- ❑ Other
- ❑ *Partially Completed*

Please specify ____
____
____
____

Q24 Since making this report I have used one or more of these organisations for support?

- ❑ Agree
- ❑ Disagree
- ❑ Do not wish to answer
- ❑ N/A
- ❑ *Partially Completed*

Q25 I feel confident in taking steps to improve my personal safety.

- ❑ Agree
- ❑ Disagree
- ❑ Don't know
- ❑ N/A
- ❑ *Partially Completed*

Q26 Why do you say that?

____

Lastly I would like to ask you a few questions about your experience with the staff that responded to your report.

Q27 Are you satisfied, dissatisfied or neither with the way that staff have treated you throughout this report?

- ❑ Completely Satisfied
- ❑ Very Satisfied
- ❑ Fairly Satisfied
- ❑ Neither Satisfied or Dissatisfied
- ❑ Fairly Dissatisfied
- ❑ Very Dissatisfied
- ❑ Completely Dissatisfied
- ❑ Don't Know
- ❑ *Partially Completed*

Q28 Why do you say that?

____

Q29 Prior to this report, was your overall opinion of the police:

- ❑ Generally High
- ❑ Generally Low
- ❑ No Opinion
- ❑ *Partially Completed*

Q30 As a result of the way you were treated throughout this report, has your opinion of the police changed?

- ❑ Yes
- ❑ No
- ❑ Don't Know
- ❑ *Partially Completed*

Q31 And has your opinion changed to:

- ❑ A better opinion
- ❑ A worse opinion
- ❑ Don't Know
- ❑ *Partially Completed*

Q32 Why do you say that?

____

Q33 As a result of the way you were treated throughout this report, how likely are you to report future incidents:

- ❑ More likely than before
- ❑ Less likely than before
- ❑ As likely as before
- ❑ *Partially Completed*

Q34 Do you have any further comments that you would like to add about the police service that you received?

____
____
____
____
____

Q35 We would like to contact you again in three months time, to ask you some similar questions which will aid our research, are you happy for us to recontact you in the future?

- ❑ Yes
- ❑ No

Q36 Would you be interested in taking part in a face to face interview to help Leicestershire Police understand how we can improve the way in which we deal with victims of domestic incidents?

- ❑ Yes
- ❑ No

Q37 What is the best way of getting in contact with you to arrange this?

- ❑ Telephone
- ❑ Email
- ❑ Text Message
- ❑ Letter
- ❑ Other

Specify what number, add, email add etc to contact on: ____
____
____
____

For more information on how to access help and support you can call Domestic Violence Support on XXXXX for City, or XXXXX for County, and XXXXX for Rutland.

That brings us to the end of this survey. I would like to thank you for your time.

If *Partially Completed,* please state why. ____
____
____
____
____

<u>Close interview</u>

Thank the victim for their time and close. Remaining questions to be completed by the Researcher

Q38 LPU

- ❑ CB - Beaumont Leys
- ❑ CH - Hinckley Road
- ❑ CK - Keyham Lane
- ❑ CM - Mansfield House
- ❑ CN - Spinney Hill
- ❑ CW - Welford Road
- ❑ LC - Charnwood
- ❑ LO - Loughborough
- ❑ LM - Melton
- ❑ LR - Rutland
- ❑ LN - NW Leics
- ❑ LB - Blaby
- ❑ LH - Hinckley & Bosworth
- ❑ LA - Harborough
- ❑ LW - Oadby & Wigston
- ❑ Unknown

Q39 Researchers Collar Number

____
___

Q40 Investigating Officers Collar Number

____
____
____

Appendix I DASH risk assessment tool

CAADA-DASH Risk Identification Checklist (RIC)[i] for MARAC Agencies

Aim of the form:

- To help front line practitioners identify high risk cases of domestic abuse, stalking and 'honour'-based violence.
- To decide which cases should be referred to MARAC and what other support might be required. A completed form becomes an active record that can be referred to in future for case management.
- To offer a common tool to agencies that are part of the MARAC[1] process and provide a shared understanding of risk in relation to domestic abuse, stalking and 'honour'-based violence.
- To enable agencies to make defensible decisions based on the evidence from extensive research of cases, including domestic homicides and 'near misses', which underpins most recognized models of risk assessment.

How to use the form:

Before completing the form for the first time we recommend that you read the full practice guidance and Frequently Asked Questions and Answers[2]. These can be downloaded from http://www.caada.org.uk/marac/RIC_for_MARAC.html. Risk is dynamic and can change very quickly. It is good practice to review the checklist after a new incident.

Recommended Referral Criteria to MARAC

1. **Professional judgement:** if a professional has serious concerns about a victim's situation, they should refer the case to MARAC. There will be occasions where the particular context of a case gives rise to serious concerns even if the victim has been unable to disclose the information that might highlight their risk more clearly. ***This could reflect extreme levels of fear, cultural barriers to disclosure, immigration issues or language barriers particularly in cases of 'honour'-based violence.*** This judgement would be based on the professional's experience and/or the victim's perception of their risk even if they do not meet criteria 2 and/or 3 below.

'Visible High Risk': the number of 'ticks' on this checklist. If you have ticked 14 or more 'yes' boxes the case would normally meet the MARAC referral criteria.

2. **Potential Escalation:** the number of police callouts to the victim as a result of domestic violence in the past 12 months. This criterion can be used to identify cases where there is not a positive identification of a majority of the risk factors on the list, but where abuse appears to be escalating and where it is appropriate to assess the situation more fully by sharing information at MARAC. It is common practice to start with 3 or more police callouts in a 12 month period but this will need to be reviewed depending on your local volume and your level of police reporting.

Please pay particular attention to a practitioner's professional judgement in all cases. The results from a checklist are not a definitive assessment of risk. They should provide you with a structure to inform your judgement and act as prompts to further questioning, analysis and risk management whether via a MARAC or in another way.

The responsibility for identifying your local referral threshold rests with your local MARAC.

What this form is not:

This form will provide valuable information about the risks that children are living with but it is not a full risk assessment for children. The presence of children increases the wider risks of domestic violence and step children are particularly at

[1] For further information about MARAC please refer to the 10 Principles of an Effective MARAC: http://www.caada.org.uk/marac/10_Principles_Oct_2011_full.doc

[2] For enquiries about training in the use of the form, please email training@caada.org.uk or call 0117 317 8750.

risk. If risk towards children is highlighted you should consider what referral you need to make to obtain a full assessment of the children's situation.

CAADA-DASH Risk Identification Checklist for use by IDVAs and other non-police agencies[3] for identification of risks when domestic abuse, 'honour'-based violence and/or stalking are disclosed

Please explain that the purpose of asking these questions is for the safety and protection of the individual concerned. Tick the box if the factor is present ☑. Please use the comment box at the end of the form to expand on any answer. It is assumed that your main source of information is the victim. If this is not the case please indicate in the right hand column	Yes (tick)	No	Don't Know	State source of info if not the victim e.g. police officer
1. Has the current incident resulted in injury? (Please state what and whether this is the first injury.)	☐	☐	☐	
2. Are you very frightened? Comment:	☐	☐	☐	
3. What are you afraid of? Is it further injury or violence? (Please give an indication of what you think (name of abuser(s)...) might do and to whom, including children). Comment:	☐	☐	☐	
4. Do you feel isolated from family/friends i.e. does (name of abuser(s) ……….) try to stop you from seeing friends/family/doctor or others? Comment:	☐	☐	☐	
5. Are you feeling depressed or having suicidal thoughts?	☐	☐	☐	
6. Have you separated or tried to separate from (name of abuser(s)….) within the past year?	☐	☐	☐	
7. Is there conflict over child contact?	☐	☐	☐	
8. Does (……) constantly text, call, contact, follow, stalk or harass you? (Please expand to identify what and whether you believe that this is done deliberately to intimidate you? Consider the context and behavior of what is being done.)	☐	☐	☐	
9. Are you pregnant or have you recently had a baby (within the last 18 months)?	☐	☐	☐	
10. Is the abuse happening more often?	☐	☐	☐	
11. Is the abuse getting worse?	☐	☐	☐	
12. Does (……) try to control everything you do and/or are they excessively jealous? (In terms of relationships, who you see, being 'policed at home', telling you what to wear for example. Consider 'honour'-based violence and specify behavior.)	☐	☐	☐	

[3] Note: This checklist is consistent with the ACPO endorsed risk assessment model DASH 2009 for the police service.

Tick box if factor is present. Please use the comment box at the end of the form to expand on any answer.	Yes (tick)	No	Don't Know	State source of info if not the victim
13. Has (……..) ever used weapons or objects to hurt you?	☐	☐	☐	
14. Has (……..) ever threatened to kill you or someone else and you believed them? (If yes, tick who.) You ☐ Children ☐ Other (please specify) ☐	☐	☐	☐	
15. Has (………) ever attempted to strangle/choke/suffocate/drown you?	☐	☐	☐	
16. Does (……..) do or say things of a sexual nature that make you feel bad or that physically hurt you or someone else? (If someone else, specify who.)	☐	☐	☐	
17. Is there any other person who has threatened you or who you are afraid of? (If yes, please specify whom and why. Consider extended family if HBV.)	☐	☐	☐	
18. Do you know if (………..) has hurt anyone else? (Please specify whom including the children, siblings or elderly relatives. Consider HBV.) Children ☐ Another family member ☐ Someone from a previous relationship ☐ Other (please specify) ☐	☐	☐	☐	
19. Has (……….) ever mistreated an animal or the family pet?	☐	☐	☐	
20. Are there any financial issues? For example, are you dependent on (…..) for money/have they recently lost their job/other financial issues?	☐	☐	☐	
21. Has (……..) had problems in the past year with drugs (prescription or other), alcohol or mental health leading to problems in leading a normal life? (If yes, please specify which and give relevant details if known.) Drugs ☐ Alcohol ☐ Mental Health ☐	☐	☐	☐	
22. Has (……) ever threatened or attempted suicide?	☐	☐	☐	
23. Has (………) ever broken bail/an injunction and/or formal agreement for when they can see you and/or the children? (You may wish to consider this in relation to an ex-partner of the perpetrator if relevant.)	☐	☐	☐	

Bail conditions ☐ Non Molestation/Occupation Order ☐ Child Contact arrangements ☐ Forced Marriage Protection Order ☐ Other ☐				
24. Do you know if (……..) has ever been in trouble with the police or has a criminal history? (If yes, please specify.) DV ☐ Sexual violence ☐ Other violence ☐ Other ☐	☐	☐	☐	
Total 'yes' responses				

For consideration by professional: Is there any other relevant information (from victim or professional) which may increase risk levels? Consider victim's situation in relation to disability, substance misuse, mental health issues, cultural/language barriers, 'honour'- based systems, geographic isolation and minimisation. Are they willing to engage with your service? Describe:

Consider abuser's occupation/interests - could this give them unique access to weapons? Describe:

What are the victim's greatest priorities to address their safety?

Do you believe that there are reasonable grounds for referring this case to MARAC? Yes / No

If yes, have you made a referral? Yes/No

Signed: **Date:**

Do you believe that there are risks facing the children in the family? Yes / No

If yes, please confirm if you have made a referral to safeguard the children: Yes / No

Date referral made ………………………………………….

Signed: **Name:**	**Date:**

[1] This checklist reflects work undertaken by CAADA in partnership with Laura Richards, Consultant Violence Adviser to ACPO. We would like to thank Advance, Blackburn with Darwen Women's Aid and Berkshire East Family Safety Unit and all the partners of the Blackpool MARAC for their contribution in piloting the revised checklist without which we could not have amended the original CAADA risk identification checklist. We are very grateful to Elizabeth Hall of Cafcass and Neil Blacklock of Respect for their advice and encouragement and for the expert input we received from Jan Pickles, Dr Amanda Robinson and Jasvinder Sanghera.

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