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**Working Paper**

## Understanding Access Barriers to Public Services: Lessons from a Randomized Domestic Violence Intervention

IZA Discussion Papers, No. 12461

**Provided in Cooperation with:**

IZA – Institute of Labor Economics

*Suggested Citation:* Koppensteiner, Martin Foureaux; Matheson, Jesse; Plugor, Réka (2019) : Understanding Access Barriers to Public Services: Lessons from a Randomized Domestic Violence Intervention, IZA Discussion Papers, No. 12461, Institute of Labor Economics (IZA), Bonn

This Version is available at:

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# Understanding Access Barriers to Public Services: Lessons from a Randomized Domestic Violence Intervention

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ISSN: 2365-9793

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## ABSTRACT

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# Understanding Access Barriers to Public Services: Lessons from a Randomized Domestic Violence Intervention\*

We study the effect of reducing barriers to accessing non-police services on the demand for police services in cases of police-reported domestic violence. Variation comes from a large randomized controlled trial designed to assist victims in accessing non-police services and we link information from local and national police administrative records and a survey of victims to form a unique dataset for the evaluation. The intervention led to a 18% decrease in the demand for police services, as measured by the provision of a statement by victims. Despite a strong correlation between statements and criminal sanctions against perpetrators, we do not find a corresponding effect of the intervention on perpetrator arrest, charges, or sentencing. This suggests that treated victims who do not provide a statement do so because their potential statement was relatively less effective for pursuing criminal sanctions. Consistent with this result, we find treatment group statements are significantly less likely to be withdrawn than are control group statements.

**JEL Classification:** I18, J12, H75

**Keywords:** public services, domestic violence, RCT, allocative efficiency

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\* We are grateful for comments provided by Dan Anderberg, Orazio Attanasio, Sonia Bhalotra, Herb Emery, Gianni De Fraja, Mark Hoekstra, Pamela Jervis, Magne Mogstad, Krzysztof Karbownik, Melanie Khamis, Chris Wallace, Tanya Wilson, and seminar participants at the Bristol Workshop on Economic Policy Interventions and Behaviour, the Essen Health Conference, Warsaw Ce2 workshop, Royal Holloway UL, and the IZA Workshop on Family and Gender Economics. Eliot Button, Corinne Crosbourn, Pooja Pattni, and Chloe Rawson provided excellent research assistance. We thank Leicestershire Police, the Leicestershire Police and Crime Commissioner, and Leicester City, Leicestershire County, and Rutland County Councils for enabling the evaluation as a randomized controlled trial and for data access. We gratefully acknowledge funding from the UK Ministry of Justice PCC Fund. AEA RCT Registry No. AEARCTR-0000537.

# 1 Introduction

A common feature of publicly provided services is the lack of an explicit pricing mechanism to allocate scarce services to users. Instead, access barriers are used, formally or informally, to manage demand. For example, many publicly provided specialist health services are accessed through referrals from a general practitioner operating as gatekeeper. Services such as social housing often manage demand through waiting lists. Several studies have documented how barriers created by bureaucracy or information have a non-trivial impact on service use (Hastings and Weinstein, 2008; King, Mullainathan, Shafir, Vermeulen and Wrobel, 2012; Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012). Less well understood is the effect that barriers can have on the allocation of users across different, non-competing, public services. The existence of asymmetric barriers can lead to a user-service mismatch, through which users fail to select the service(s) best suited to their needs. In this case, an intervention to reduce unintended access barriers may improve the quality of the user-service match and the distributional efficiency of the utilization across the range of available services.

Services available to domestic violence (DV) victims following an incident of DV provide a stark example of access barriers across many different, but related, services. We categorize DV support services into two types: non-police services—including refuge housing, counseling, and practical support with general safety or planning for an escape from an abusive partner—and police services—specifically, investigation for the purpose of criminal sanctions against the perpetrator. Anecdotal evidence suggests that significant barriers hinder victims’ access to non-police services, including lack of knowledge of existing services and lack of clarity around what different services offer, how to access each service, and barriers due to gatekeepers.<sup>1</sup> In contrast, for the population that we focus on (DV victims who have

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<sup>1</sup>This was highlighted as an issue in a report on the policing of domestic violence in the UK (HMIC, 2014). Fugate et al. (2005) show that information barriers are a significant deterrent for victims of domestic violence in the United States.

already reported to police), accessing police services is relatively frictionless. In this paper, we investigate the effect of reducing barriers to accessing non-police services on the demand for police services among victims of police-reported DV.

To obtain credible estimates of the relationship between ease of access and demand, we study variation in ease of access to non-police services from a randomized controlled trial (RCT) of an intervention specifically designed to remove access barriers to non-police services.<sup>2</sup> The intervention provides victims of police-reported DV with a caseworker who offers information about and support in accessing non-police support services. Randomization takes place at the level of individual *victims*, across a large UK police force, over a six-month period. The final sample of over one thousand households constitutes one of the largest individual-level RCTs on DV to date. Our analysis is based on a unique dataset that we construct by linking information from local police administrative records, the UK Police National Database, and a victim follow-up survey. These extraordinarily rich data, collected over a two-year period, allow us to follow the lifecycle of every case in our sample, from the time a case is opened to the time a perpetrator is sentenced.

We use the provision of a statement to police by the victim to measure the demand for police services.<sup>3</sup> Statements provide a clear measure of demand because in most cases, they are an essential input for police to pursue criminal sanctions against a perpetrator. We find that the intervention led to a 5.4 percentage point decrease in the provision of statements to police (as measured by the intention to treat), or a 18.1% decrease relative to the control group. This large treatment effect demonstrates that for the average subject in our sample, reducing access barriers to non-police services leads individuals to use fewer police services, suggesting that non-police services and police services are substitutes. We demonstrate the

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<sup>2</sup>The program is known as *Project 360*, reflecting the role of the caseworker in taking a 360-degree look at victims' needs and the corresponding available services.

<sup>3</sup>In our context, providing a statement to police would be analogous to what is commonly called "pressing charges" in the North American context.

robustness of this estimate by exploiting a falsification test that occurs from the design of the intervention.

There is a strong correlation between the provision of a statement and criminal sanctions against a perpetrator. Therefore, from a policy point of view, the significant decrease in statement provision may be a concern if it is matched by a large decrease in criminal sanctions. The evidence we provide rejects this concern. We look at three outcomes reflecting criminal sanctions for the perpetrator: arrest by the police, charges by the Crown Prosecution Service, and sentencing by the courts. The corresponding effect of treatment on all of these outcomes is small and statistically insignificant. This suggests that for victims who forgo making a statement in treatment, the effect of their statement on criminal sanctions is low relative to other victims. One plausible mechanism underlying this result is statement retraction. Relative to the control group, treatment group statements are 10.1 percentage points, or 84%, less likely to be retracted. This result suggests that the intervention increased the efficiency of police service utilization by removing ineffective statements from the service load of police officers related to further investigations.

The survey information that we collect allows us to look at outcomes not found in administrative records. These data suggest that the intervention leads to an increase in the use of non-police support services. We find that although stress increases more for treatment group victims than for control group victims, the treatment group is more likely to report family life having improved and greater satisfaction with the service provided by police.

This study contributes to a literature studying policy "nudges" to overcome information barriers in service choice. Previous work finds that simplifying information on public school performance leads parents to select higher performing schools for their children (Hastings and Weinstein, 2008), providing information on how the cost and benefits of education changes students' intention to stay in non-compulsory education (McGuigan, McNally, and Wyness, 2016), personalized prescription drug plan information makes Medicare users more likely to

switch to lower cost plans (Kling, Mullainathan, Shafir, Vermeulen, and Wrobel, 2012), and assistance for filling out complex college aid applications leads to a significant increase in college enrolment (Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012). These studies demonstrate that even small bureaucratic barriers or costs to obtain or process information will lead to distortions in choice relative to what is chosen absent these barriers. Our study is similar in spirit, considering a relatively simple change to the way that victims of DV receive assistance following a police-reported incident. If victims of DV find it difficult to access services, or determine which services are best suited for their needs, then they may rely on simple heuristics, such as utilizing police services with which they are already interacting.

Unlike the previous studies, we consider service users who choose among different, non-exclusive services. Potential service users can, and do, choose more than one service. Because services are not explicitly priced, users do not internalize the cost of service provision and may allocate themselves in such a way that service costs outweigh the private benefits. This is a general problem with any publicly available service.<sup>4</sup> Our results suggest that the service users who forgo police services when provided with the intervention, are those who, on average, benefit the least from police services. If the cost of providing police services is high relative to non-police services, then the intervention is likely to improve allocative efficiency. This is particularly important for services related to DV because of their frequency and relevance for policing. In the UK, domestic violence and abuse account for approximately 11% of all crimes reported to police,<sup>5</sup> creating very substantial service demand on police forces in the country.

This study also contributes to a growing literature that looks at the economic causes and consequences of domestic violence. Some of these studies focus on modelling and estimating the mechanisms that lead to or exacerbate household violence (Aizer, 2010; Anderberg and

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<sup>4</sup>This problem may show up in the case of school selection or attendance, as in Hastings and Weinstein (2008) or Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012).

<sup>5</sup>This number is based in a total of ONS 2018a, ONS 2018b.



Rainer, 2013; Anderberg, Rainer, Wadsworth and Wilson, 2016; Bobonis, Gonzalez-Brenes, and Catro, 2013). A smaller strand of the economic literature focuses on identifying the social spillovers (Carrell and Hoekstra, 2010; Currie, Rossin-Slater and Mueller-Smith 2018). Our paper specifically contributes to the literature studying the role of public services for victims of domestic violence. In this literature, Aizer and Dal Bo (2009) investigate the role of no-drop policies as a commitment device, compelling prosecutors to continue with prosecution even in cases for which the victim expresses a desire to drop charges. They show that these policies result in an increase in reporting. Farmer and Tiefenthaler (1996) use a household bargaining model to show that access to non-police services may help victims of DV through increasing their threat points to leave the relationship. Our paper contributes to this literature by specifically focusing on the effect of removing existing access barriers to non-police services and its effect on both, the demand for non-police and police services, a focus so far neglected in the literature. We show that removing these unintended access barriers reduces the demand for police services where they are least productive—in cases where statements are often withdrawn following an incident of DV.

Finally, this study makes an important methodological contribution to the analysis of interventions involving DV. Previous studies of similar interventions focused on repeat police callouts to the same household as the primary outcome (see Davis, Weisburd, and Taylor (2008) for a summary of these studies). As a measure of program effectiveness, repeat police visits are ambiguous. A change in the frequency of police callouts may be affected by a change in reporting, with no change in household violence, or by a change in the frequency of violence in the household. In this study, we focus on the provision of a statement by victims to police as an unambiguous outcome not subject to reporting biases and we demonstrate the importance of using such unambiguous measures to evaluate the effect of interventions involving victims of DV.

The paper is structured as follows. In Section 2, we present a simple model to provide a

conceptual framework for thinking about the effect that the intervention will have on service demand and provide the institutional background information. In Section 3, we discuss the details of RCT design and implementation, followed by data sources and collection in Section 4. The main results of the paper are presented in Section 5. This is followed by a brief discussion of results in Section 6.

## 2 Conceptual framework and background

### 2.1 Access barriers and service use

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:

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

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 1, we depict service utilization at different values of  $p_i$  and  $n_i$  in the case

when  $b$  is positive (1a, 1b) and when  $b$  is negative (1c, 1d). Figures 1a and 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  $n_i$ .

Figure 1 about here

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 1b and 1d by a movement from  $c^n$  to  $c^{n'}$ . In both cases,  $b > 0$  and  $b < 0$ , there will 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 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 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 complimentary or substitutes.

## 2.2 Services available to victims of DV

The intervention we study aligns with the stylized framework discussed above. In the UK and many other contexts, access barriers (reflected by  $c^n$  in Equation (1)) are non-trivial. In this section, we discuss the non-police and police services available for victims of domestic violence, highlighting the sources from which barriers arise.

### 2.2.1 Non-police services available to victims of DV

In the police force area that we study, and with most UK police forces, DV services are available through a number of publicly funded and voluntary service providers. The delivery of these services ranges from national coverage (for example, through national helplines) to local support groups. Services may be available to all victims or focus on specific groups such as by ethnicity/language, male victims, or the LGBT community. Local authorities outsource the delivery and administration of many services to non-profit organisations. Through a series of online searches and information provided by the police, we identified 24 different agencies in the police force area in which our study is based (Leicestershire, UK)<sup>6</sup> at the time of the intervention. In Appendix C we provide a table summarizing these service providers. In Appendix C, we provide the pamphlet, containing information on a number of these services, that police provided to all victims following a domestic incidence.

In Table 5, we list the categories of services most accessed by the treatment group. Services can be grouped largely into three categories. The first is refuge housing, in which victims move into temporary accommodation away from a perpetrator. While not the most commonly used service (9.2% usage), it is one of the most far-reaching options available to victims. Second, several different health services are available to victims of domestic violence. This includes consulting a general practitioner (12.3% usage),<sup>7</sup> emotional support and counselling (48.0% usage), and drug and alcohol support programs. Third, several safety planning services are available. These often focus on measures to improve victim safety, such as having intruder alarms installed or new locks fitted (60.5% usage).

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<sup>6</sup>This police force area covers three local councils Leicester city, Leicestershire County, and Rutland.

<sup>7</sup>Many of the victims in this study were not registered with a general practitioner. In these cases, the caseworker will encourage and assist in the GP registration processes.

### 2.2.2 Barriers to non-police services

Despite the availability of these services, barriers exist that make it costly for victims to access them. Barriers can arise from four non-exclusive sources. First, victims may lack information about the availability of these services or the process to access them. This may include lack of knowledge of restrictions on service use or restrictions on the hours in which services can be accessed. 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). From a victim's perspective, the identification of the different services and the different agencies responsible for service administration is challenging. The complexity of deciding between the different services poses a barrier to accessing any of the services, similar to choice overload. Third, barriers may originate at the individual level from psychological and/or language barriers. An incident of domestic abuse or constant exposure to DV induces stress to victims and feelings of being overwhelmed. Domestic abuse is associated with depression, anxiety, and substance abuse (Dutton, et al. 2006). For victims with a migration background, language barriers may also play an important part in the underutilization of services. Fourth, service providers often put formal barriers in place. The purpose of these barriers is to ensure the safety of the services users and to control the demand for scarce resources. Formal barriers often take the form of a gatekeeper. For example, refuge housing is often only accessed through a referral by a social worker or the police. Finding the location of, or contacting a refuge house can be challenging; location information is not explicitly made public for the safety of the service users.

While we do not explicitly distinguish between different sources of barriers, they are widely recognised as an impediment to service uptake. *Her Majesty's Inspectorate of Constabulary* (HMIC 2014) reports anecdotal evidence, based on victim interviews, that victims of DV felt that they did not know where to turn for help after an initial callout. A report by the Northamptonshire Police and Crime Commissioner finds that the information available

about DV victim support services is inadequate, and "victims are confused about which support organizations do what" (Northamptonshire PCC, 2013).

These barriers to services are not unique to the UK context. In the US and Canada, Jaffe et al. (2002) find that "women reported feeling let down and confused by the [community and social services support] process." They find that many women removed their application for services out of frustration with the number of barriers. In interviews with DV victims in Chicago, Fugate et al. (2005) find that perceived barriers to access, particularly lack of information, are an important explanation for why victims do or do not contact social and counselling services, but not important for explaining why they contact police services.

The intervention we study is specifically designed to help victims of DV overcome any of these barriers and reduce the cost of victims to access services by providing information on existing services, by signposting victims to the appropriate service, by helping them overcome psychological and language barriers, and by providing referrals to existing services.

### **2.2.3 Police services available to victims of DV**

Throughout the paper, we refer to police attending a domestic violence incident in response to an emergency call made by a victim or a third party as the *initial callout*. When police officers attend an initial callout, they have two tasks. The first task is to defuse the immediate, and potentially volatile, situation and ensure the safety of all individuals. Police have the power to arrest and temporarily detain a perpetrator for up to 24 hours solely for this purpose.<sup>8</sup> The second task is to collect evidence within the initial investigation to determine whether to initiate further investigations for the purpose of pursuing criminal sanctions against the perpetrator. Evidence can be direct, such as police observing and recording a physical assault, for example, through body-worn cameras. More often, however, evidence is indirect in the form of statements made by witnesses. A statement is a recorded recollection of events

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<sup>8</sup>This arrest may be made for 24 hours independent of the victim's willingness to make a statement. After 24 hours, the Crown Prosecution Service must press charges, or the perpetrator must be released.

by a witness that can be used as evidence in court. In the majority of DV cases, the victim is the primary witness, and the victim’s statement is the major piece of available evidence. On the basis of the available evidence, the *Crown Prosecution Service* (CPS) makes a decision whether to charge the perpetrator for the purpose of pursuing criminal sanctions or not, and consequently, and in case of a decision to charge, police may arrest the perpetrator.

In contrast to the two tasks outlined above, which are performed at every reported case, a further investigation by police is only performed if there is reason to believe that there will be sufficient evidence for the CPS to pursue charges. Throughout this paper, we will use *police services* to refer to the further investigative work done to pursue criminal sanctions against a perpetrator.

Even when a victim initiates the initial callout, we cannot assume that the victim wants the police to pursue criminal sanctions against the perpetrator. For example, a victim may solely be interested in the police temporarily removing the perpetrator from the premises when dealing with the initial callout. This is consistent with previous work that has found that victims often want attending police officers to defuse escalated household tensions or coerce the perpetrator into better behavior, without taking punitive action (Hoyle and Sanders, 2000). This can be due to a number of reasons, including manipulative behavior by the perpetrator in the aftermath of an incidence. However, it could also be because penalties and prosecution in domestic cases also may have a direct negative impact on the victim (Hoyle and Sanders, 2000). For example, a permanent separation from the perpetrator may require victims to shoulder an extra burden in household production.

#### **2.2.4 Barriers to police services**

In this study, all victims are already interacting with the police at the time of the intervention. In case victims do not make a statement at the time of the initial callout, they receive information how to make a statement at a later point. For this reason, the effect of the

intervention on the cost of accessing police services is likely to be negligible compared to the effect of the intervention on the cost of accessing non-police services. This is consistent with Fugate et al. (2005), who find that following an incident of DV, significant barriers exist for accessing non-police services, but not for further use of police services.

### 2.2.5 Statements and police services

It is through the decision to provide a statement that the victim can influence the progression of the case toward criminal sanctions against the perpetrator. A victim can provide a statement at the initial callout (in our data, 50.1% of victims who provide a statement do so at the initial callout), or a victim can contact the police and provide a statement any time after the initial callout. Once a statement is provided, the victim may decide to withdraw the statement at any time (in our data, 17.0% of all statements are withdrawn). If this happens, the statement cannot be used as evidence in the case against the perpetrator.<sup>9</sup>

Figure 2 about here

In our data, the correlation between victim statement provision and perpetrator arrest and charge is strong (see Figure 2). Overall, 27% of victims in our sample provide a statement to police. In the 743 cases for which no statement was provided, the perpetrator was arrested in 10.0% of cases. In the 272 cases for which a statement was provided, the perpetrator was arrested in 68.2%. Likewise, charges are laid in 3.0% of cases with no statement versus 37.6% of cases with a statement. Of course, this does not tell us anything about the causal effect of statements on arrests and charges because victims may select into making a statement based on their subjective expectation of probability of an arrest. However, this correlation provides evidence to reinforce the importance of statement provision in pursuing punitive action against a perpetrator.

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<sup>9</sup>An information pamphlet published by the charity *Rights of Women* states, in reference to victim statement provision in domestic violence cases, "Without a witness statement from you it is unlikely that the police will continue" (Rights of Women, 2013).



In cases in which no further action is taken, even if an arrest is made initially, there is no progression towards further investigation and subsequent criminal sanctions. In cases in which the decision is made by the CPS to charge the perpetrator, the case progresses to the courts to determine sentencing. 62.9% of all cases in which charges are laid, result in a sentence by the courts.<sup>10</sup> This is slightly higher in cases in which a statement is made (63.7% versus 59.0%), but the difference is not statistically distinguishable from zero. This is consistent with the role of the CPS in filtering cases that proceed to the courts based on the strength of the evidence.

The intervention studied in this article, discussed in greater detail in Section 3.3, takes place after the initial callout. Therefore, if it influences the victim’s demand for criminal sanctions against the perpetrator, it will be observed either through changes in statement provision for those who do not provide a statement at the initial callout or through changes in the propensity of a victim to withdraw his or her statement.

### 3 Experimental design

We set up a randomized controlled trial in the Leicestershire Police Force area (*Leicestershire* hereafter), UK, jointly with Leicestershire Police and the three local authorities in Leicestershire, Leicester City Council, Leicestershire County Council, and Rutland County Council. Leicestershire (see Figure 3 for map) covers a population of approximately one million people, and Leicestershire Police is one of 43 police forces in England. One-third of the population in Leicestershire is concentrated in the city of Leicester, with the remaining population distributed across approximately 300 towns and villages. The experiment ran between November 2014 and April 2015.

Figure 3 about here

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<sup>10</sup>Sentencing includes prison time (24.7%), fines (43.6%), restraining orders (39.7%), and mandatory rehabilitation programs (17.6%).

### 3.1 Allocation of cases into the subject pool

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

After responding to a domestic incident, officers file a standardized report that includes details of the household.<sup>11</sup> This information is recorded as a domestic incident report in the Leicestershire Police database and is assigned a case number. Our automated application performs a daily scheduled search through all recorded incidents and recovers domestic incident cases that meet several conditions: 1) the report is filed as a *domestic incident*; 2) in the previous 365 days, the victim has shown up in at least three previous DV reports (including the current one) and fewer than seven DV reports;<sup>12</sup> 3) the victim has not been in the subject pool previously (as either treatment or control); and 4) the victim is not assessed by responding officers as *high risk*.<sup>13</sup> Cases that meet these criteria are assigned to the subject pool. The application automatically allocates subject pool cases to treatment or control groups, each with a 50% probability. Leicestershire Police received more than 50 reported domestic incidents daily during the trial period. Of these approximately, 7 qualify for the subject pool. For the purpose of examining statements and conducting the survey described in Section 4.3, the person labeled *victim* in each case is assigned as the subject.

The final sample consists of 1,017 cases (each case refers to a unique victim). Of these, two cases were dropped due to restrictions on access to police data.<sup>14</sup> There are a small number of cases for which we do not have information for all control variables. For the purpose of regression analysis, these missing values will be given a value of 0 and a variable-

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<sup>11</sup>This report is known as the *Domestic Incident and Vulnerable Child Working Sheet*.

<sup>12</sup>The initial interest of this intervention was to assist victims of repeat domestic violence. The minimum of three offenses was based on predicted capacity constraints of the trial. If there are more than 7 DV incidents in the households, the case is classified as high-risk and is referred to a Multi-Agency Risk Assessment Conference that provides a separate intervention.

<sup>13</sup>Cases categorized as *High* were excluded as these are transferred to a multi agency risk assessment conference (MARAC) and treated separately.

<sup>14</sup>This would happen in the case in which individuals in the case are under investigation for a serious offense such as sexual assault involving a minor.

specific dummy will be used to indicate missing values.<sup>15</sup> The final data set 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 receive standard police procedure as described in Section 2.2. Upon attending the initial callout, responding officers leave victims with a pamphlet that lists, describes and provides contact information for some of the available DV services in Leicestershire (see Appendix C). Victims are able to contact the services on this pamphlet at any point.

As previously mentioned, victims may also provide a statement to police at any point during or after the initial callout. In some cases the attending officers may provide a follow-up visit after the initial visit, but these cases are rare and are uncorrelated with treatment.

## 3.3 Treatment

Three dedicated caseworkers were employed for the trial of the intervention. The caseworkers were female and between the ages of 25 and 35. Caseworkers all had previous training and experience as domestic abuse support workers. Specifically, all had previous experience in working with DV support services in Leicestershire and had specialized knowledge of the various local services available and how to access them. They also received training specific to the service provided through the intervention in this study.<sup>16</sup> Caseworkers were provided with desk space and IT support in a large Leicestershire Police station.

At the beginning of each day, the automated selection application e-mails details about the new cases assigned to the treatment group to the caseworkers. Cases in the treatment

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<sup>15</sup>All reported results are robust to the exclusion of missing variables from the analysis.

<sup>16</sup>One of this study's authors was present during these training sessions.

group are non-randomly allocated among the caseworkers according to workload and availability. The caseworker attempts to contact subjects via telephone within 24 hours<sup>17</sup> of the initial police report. Once contact is made, the caseworker describes to subjects the publicly provided support services that are available locally. If the subject expresses a wish to access a specific support service, the caseworker assists in initiating access. This includes organizing initial contact with the relevant support service, helping complete any paperwork, and providing a referral when necessary. All contacted subjects are offered a face-to-face meeting with the caseworker to go through the options available. If the victim wishes to leave the perpetrator, the caseworker also assists in preparing an escape plan. The intervention ends when either the victim declines to participate in the intervention or a relevant support service has taken up the case.

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

We define a victim as having *engaged* with the intervention if they are successfully contacted by a caseworker and they accept some form of assistance, ranging from the provision of advice during a one-time phone conversation to face-to-face follow-up meetings. While an effort was made to deliver the intervention to all 501 victims assigned to the treatment group, 240 (48%) of treatment group victims did not engage.<sup>18</sup> Of these, 143 were contacted

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<sup>17</sup>While caseworkers were on duty and attempted to make contact on Saturdays, victims of incidents occurring between Saturday evening and Monday morning were all contacted on Monday, thus extending the period of first contact to 36–48 hours in these cases.

<sup>18</sup>A maximum of 5 attempts, at different times of the day across 5 days, was made to contact victims by phone.

by a caseworker by telephone but refused both phone-based assistance and a face-to-face meeting. For the remaining 97, caseworkers were unable to make contact given the available contact information.<sup>19</sup>

Among victims whom the caseworkers were able to contact, the engagement rate was 65%. Considering that caseworkers "cold-called" the victims, this is a notable take-up rate. Of the 261 victims who did engage, 128, or 49%, had at least one face-to-face meeting with the caseworker. Just under 35% of all home visits took place within 24 hours of the initial police callout (the same day that caseworkers made first contact), with another 20% taking place within three days. 33% of home visits took place after three days but within a week, and the remaining 13% took place more than one week after the initial callout.<sup>20</sup>

### 3.4 Internal validity

Several design features of the trial safeguard the internal validity of this study. Most importantly, all assignments to the treatment and control groups were automated and randomized. Unlike previous RCTs of similar second responder interventions (Davis and Taylor, 1997; Davis, Weisburd, and Hamilton, 2007), caseworkers or police officers could not override assignment to treatment.<sup>21</sup> Furthermore, the timing of treatment status—after the initial police callout—ensures that the actions taken by police at the initial callout were not influenced by knowledge of treatment assignment.

Only caseworkers received information on cases in the treatment group. While caseworkers could of their own initiative search police reports for other reported DV cases, we are

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<sup>19</sup>For the victims' safety, the caller ID was not displayed, which may have led some victims to not answer the call.

<sup>20</sup>In Appendix A we provide and test an alternative rationalization of our main results based on the timing between the initial callout and the visit by the engagement worker creating a cooling off period, which decreases statement provision. We show that the data do not support this rationalization.

<sup>21</sup>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 members of the police force were not aware of the intervention during the trial.

confident this did not happen. Every access to a report in the police information system is recorded and monitored, and unauthorized access to cases not in the treatment group by the caseworkers could result in disciplinary action.

## 4 Data

This study is built around a unique and innovative data set that we constructed from three unique sources. The primary data for this study are drawn from administrative records of the Leicestershire Police Force. We combine this with data on prosecution and sentencing from the UK Police National Database and complement the data collection with data specifically collected for this study through a designated victim survey. Here, we will briefly discuss the data sources in turn.

### 4.1 Leicestershire Police Database

We match cases in the subject pool with administrative records (from a number of internal databases of Leicestershire Police) using a unique crime reference number. The administrative records from these databases provide information on the initial incident (date, time, location, attending police officers, DASH score, and action taken by police) and a wealth of information on the victim and perpetrator, including demographic characteristics, household information, and previous and subsequent police records. We linked victims and perpetrators across different incidents over time using personal identifiers, including name, date of birth, and address. From the individual case files, we also collected information on the provision of statements and status of any action taken by police.

The information was collected by three research assistants<sup>22</sup> who did not have information

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<sup>22</sup>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

on the treatment status of individual cases. A fourth research assistant checked the recorded information for consistency and accuracy from a random draw of approximately 30% of the cases.

Appendix B provides further details on the administrative data systems accessed.

## 4.2 Police National Database

We are particularly interested in outcomes of the criminal justice process, not contained in the administrative records from Leicestershire Police. This information is only available from the Police National Database (PND), which is designed to share intelligence across all police forces and criminal justice agencies throughout the UK. The PND holds over 3.5 billion searchable records with information about individuals who have been arrested, cautioned, and convicted. The nationwide coverage allows us to track individuals beyond the Leicestershire Police Force area and access information on all convictions of individuals.

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

Because access to the PND is highly restricted, even within the police force, the data were collected by a specially trained and licensed police officer for whom every access to the PND was authorized for the research project. This officer was blind to the treatment status background checks. All research assistants were undergraduate students at the University of Leicester with a background in law or criminology.

of individual cases.

### 4.3 Victim survey

The administrative datasets used in this study are supplemented with data collected through a victim survey specifically designed for this project. The victim survey was conducted by the Leicestershire Police Information Services Unit using researchers specifically trained in surveying victims of DV. 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.<sup>23</sup> Interviewers conducted the survey blind to the treatment status of the interviewee. Broadly speaking, the survey collected stated information on a) subjective well-being and safety, b) use of non-police services by the victim, and c) satisfaction with police services and willingness to report incidents in the future.<sup>24</sup> The survey was administered to a 25% random sample of the full subject pool.<sup>25</sup> From this sample, we received an 84% response rate, resulting in complete surveys for 105 treatment group subjects and 109 control group subjects.

### 4.4 Descriptive statistics and treatment/control group balance

In Table 1, we report descriptive statistics for the 1,015 cases in our sample. Mean characteristics are reported for victims ( $A$ ), perpetrators ( $B$ ), and the household overall ( $C$ ). We report, for each characteristic, the mean values for the treatment and control groups with sample standard deviation in parenthesis. In the column labelled *Difference*, we report the

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<sup>23</sup>Researchers followed strict procedure to ensure the safety of victims of DV at any moment and conducted the interview only if the interviewee ensured the researcher that the perpetrator was not in the premises and after the location of the victim had been recorded. In case the connection to the victim’s mobile phone was interrupted, a rapid response police unit was sent to the premises to ensure safety of the interviewee.

<sup>24</sup>The full survey can be found on the author’s project website, [https://prj360.org/wp-content/uploads/2019/04/P360\\_VictimSurvey.pdf](https://prj360.org/wp-content/uploads/2019/04/P360_VictimSurvey.pdf)

<sup>25</sup>This sample was negotiated with the Leicestershire Police Information Services Unit based on their resource constraints.



difference between treatment and control means with the corresponding standard error in parenthesis.

Table 1 about here

To confirm random assignment, the means reported in Table 1 should not systematically differ between the treatment and control groups. Based on these observable characteristics, treatment and control are well balanced; most observables do not differ significantly between the two groups.<sup>26</sup> Some important characteristics reflecting incident severity and the state of household violence are worth highlighting. Specifically, the average number of cases over the last year (2.33 and 2.26) and the responding officer’s victim risk assessment score (1.28 and 1.28) do not differ significantly between treatment and control. Furthermore, we do not observe a significant difference according to intimate partner status of victim and perpetrator or the presence of children in the household. We therefore interpret Table 1 as evidence that allocation to the treatment or control group was random.

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

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<sup>26</sup>Two exceptions should be noted. First, at the time of the initial callout, perpetrators in the treatment group have 1.16 more registered instances of domestic violence than do perpetrators in the control group. Second, victims and perpetrators are 6 percentage points more likely to be living together in the treatment group than in the control group. At the 5% and 10% levels of significance, the number of significant differences is roughly what one would expect to occur by chance. The remaining differences are both statistically insignificant and small in magnitude.

## 5 Results

In this section, we present the results for the effect that the intervention had on a number of outcomes, including statement provision, criminal justice outcomes, the use of non-police services, and the well-being of the victim. We also exploit a natural falsification test arising from the timing of the intervention.

As discussed, the voluntary nature of the program means that not all victims in the treatment group received the intervention. For this reason, we focus on two treatment effects. The first is the intention to treat (ITT), denoted by  $\gamma_1$  in the linear probability regression (2).

$$S_i = \gamma_0 + \gamma_1 treat_i + X_i' \Gamma + e_i \quad (2)$$

$S_i$  is a binary indicator equal to 1 if victim  $i$  provided a statement to police and 0 otherwise.  $treat_i$  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, and the number of police-reported domestic incidents in the previous year.<sup>27</sup>  $e_i$  captures all other influences on the respective outcome  $y_i$  that are unobserved by the researchers. We assume that  $e_i$  and  $treat_i$  are uncorrelated, justified by random assignment of treatment.

Second, we look at the local average treatment effect (LATE),  $\phi_1$  in the system of equations (3) below,

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<sup>27</sup>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.

$$\begin{aligned}
Engagement_i &= \pi_0 + \pi_1 treat_i + X_i' \Pi + u_i \\
S_i &= \phi_0 + \phi_1 Engagement_i + X_i' \Phi + v_i.
\end{aligned} \tag{3}$$

$Engagement_i$  is a binary indicator equal to 1 if  $i$  took up the intervention and 0 otherwise. Assignment to the treatment group,  $treat_i$ , is the first-stage instrument for  $Engagement_i$ .  $\phi_1$  denotes the LATE effect of interest.  $u_i$  and  $v_i$  denote the unobservables for each of the respective equations, both assumed to have zero mean conditional on assignment to the treatment group.

## 5.1 Program effect on statement provision

In Table 2, we report the estimated treatment effects for the provision of victim statements to police. The unconditional difference between treatment and control (Column 1) shows that there is a 6.2 percentage point decrease in statement provision between the treatment and control group. The treatment effect is very similar when control variables are added, suggesting that the intervention leads to a 5.4 percentage point decrease in the provision of statements by victims to the police. This is a 18.1% decrease relative to statement provision in the control group.

The LATE estimate suggests that victims who engaged with the intervention are 10.6 percentage points less likely to provide a statement to the police. This is a large effect, a 35% decrease relative to statement provision by the control group.<sup>28</sup>

Table 2 about here

The timing of the intervention is such that we should not observe an effect on statements that are provided to the police prior to contact with the caseworker. We test this by esti-

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<sup>28</sup>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.

mating the ITT for making a statement at the initial police callout ( $t = 0$ , before treatment) and (conditional on no statement at  $t = 0$ ) making a statement at least one day following the initial police callout ( $t > 0$ , after treatment). As expected, the treatment and control group make statements at  $t = 0$  at approximately the same frequency (Column 4, Table 2). The estimated treatment effect is a statistically insignificant difference of -0.4 percentage points. The treatment group is less likely than the control group to make a statement at  $t > 0$  (Column 5, Table 2). The estimated treatment effect is -5.6 percentage points, confirming that the difference in statement making estimated earlier arises solely from any difference arising after the initial police callout as expected.

Figure 4 about here

We examine the timing of statements further in Figure 4 by estimating a treatment effect relative to days,  $t = \{0, 1, \dots, 10\}$ , since the initial callout ( $t = 0$ ). In Figure 4(a), we plot the probability of a statement (conditional on no statement provided in previous days) against days since the initial incident. In Figure 4(b), we plot the treatment effect corresponding to each day with 95% confidence intervals. These figures draw attention to several points. First, both the treatment and the control group exhibit a similar pattern of the propensity of early statement making that dissipates rapidly over time. By  $t = 4$ , the propensity to make a statement on a given day is less than 1%. Second, consistent with Table 2, we do not observe a significant difference at  $t = 0$ , the day that sees the most statements being made. Third, a negative treatment-control statement gap persists from  $t = 1$  to  $t = 4$  days following the initial callout; we do not observe a distinguishable statement difference in days for which statement making is relatively infrequent ( $t > 4$ ).

## 5.2 Program effect on perpetrator arrest, charge, and sentencing

Given the decrease in statement provision due to the intervention, we might be concerned that this also leads to a reduction in punitive actions taken against the perpetrators. We examine this possibility here, calculating ITT and LATE estimates (corresponding to equations (2) and (3)) for perpetrator arrest by police, prosecution by the Crown Prosecution Service, and subsequent sentencing by the courts. Table 3 reports the estimates.

Table 3 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 point reduction in arrest, a 0.4 percentage point reduction in the perpetrator being charged, and a 0.4 percentage point reduction in sentencing of the perpetrator. These magnitudes correspond to a 3.8%, 3.0%, and 4.8% decrease relative in the respective outcomes. These results suggest that there was little effect of the reduction in statement provision on punitive actions against the perpetrator.

### 5.2.1 Interpretation

The results so far suggest that the intervention led to a significant decrease in the provision of victims' statements to police, but not a significant change in perpetrator arrests or prosecution. Given the strong correlation between arrests and statement provision, this result might be surprising. Here we provide evidence that the change in statement provision was non-random. Specifically, victims who do not provide a statement as a result of the treatment have, on average, a lower statement productivity than other victims. We say that statement  $A$  is more *productive* than statement  $B$  if the probability of  $A$  leading to an arrest and other further actions is higher than  $B$ .

Victims can be classified into four types (corresponding to the familiar label of *compliers* and *defiers*), labeled  $d \in \{-1, 0^+, 0^-, 1\}$ . 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  $d = 0^+$  always provides a statement, and  $d = 0^-$  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).<sup>29</sup>

The relationship between the intervention and a perpetrator arrest (ignoring control variables) can be written as

$$P_{id}(treat_i) = \alpha_0^d + \alpha_1^d S_d(treat_i) + \mu_{id} \quad (4)$$

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 if the victim provides a statement to police, and 0 otherwise, and is a function of treatment status and type.  $\mu_{id}$  reflects unobserved heterogeneity in the outcome. We assume that  $E(\mu_{id}|treat_i, S_d) = 0$ , treatment affects  $P_{id}$  only through statement provision.<sup>30</sup> The coefficient  $\alpha_1^d$  reflects the type-specific effect of statements on punitive actions.<sup>31</sup> We make an additional assumption 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  $w^{-1} + w^{0^-} + w^{0^+} +$

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<sup>29</sup>Assumption a) follows from the argument in Section 5.1 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.

<sup>30</sup>This rules out, for example, caseworkers directly influencing the decision of police to make an arrest.

<sup>31</sup>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.

$w^1 = 1$ , the ITT corresponding to equation (4) can be written as:

$$E(P(1)) - E(P(0)) = (\alpha_1^1 - \alpha_1^{-1})w^1 + \alpha_1^{-1}(w^1 - w^{-1}) \quad (5)$$

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 (2).  $\alpha_1^1 - \alpha_1^{-1}$  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.

To summarise, the results suggest that while the intervention had a negative effect on the demand for police services, as measured by statement provision, it did not result in a significant decrease in the output of those services, as measured by outcomes of the criminal justice process. This suggests that either statement provision is less effective in leading to criminal justice outcomes for those who forgo statements due to treatment ( $d = -1$ ) than for those who make statements due to treatment ( $d = 1$ ), or the effect of statements in leading to criminal justice outcomes is close to zero for those who forgo statements due to treatment.

### 5.3 Productivity in police services

In this section, we explore the interpretation of sections 5.1 and 5.2 further, looking at the implications of the intervention for the productivity of police services in domestic violence cases. Once a statement is made, it requires investigative efforts on the part of police to determine whether police should put together a case for prosecution. In this way, the correlation between statements and arrests, prosecution or sentencing provides a measure of

productivity. Table 4 reports our key results.

Table 4 about here

Statement retraction is a plausible channel through which the findings presented in sections 5.1 and 5.2 may arise. Any time following the provision of a statement, a victim has the right to retract that statement. If a statement is retracted, it is inadmissible as evidence against the perpetrator.

We find a significant decrease in the retraction of statements that are provided after the initial callout, (Statements at  $t > 0$ , Panel A, Table 4). This suggests that statements made after initial callout are 10.1 percentage points less likely to be retracted in the treatment group than they are in the control group. Considering retraction of these statements for the control group is 12.2%, this is an 83% reduction, leaving treatment group statement retractions at only 1.9%. Furthermore, we do not see a similar reduction for statements made at the initial callout (Statements at  $t = 0$ , Panel A, Table 4), which is smaller in magnitude and statistically insignificant.

We find that the correlation between statement and perpetrator arrest is 10.5 percentage points higher for the treatment group relative to the control group (*Any statement*, Panel B, Table 4). Consistent with previous findings, this is due to a 14.1 percentage point increase in the correlation for statements made after the initial callout (Statement at  $t > 0$ , Panel A, Table 4). There is not a significant difference between the treatment group and control group in this correlation for statements made at the initial callout.

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

We also look at differences of treatment and control in the correlation between statement



provision and perpetrator charges and sentencing (panels *C* and *D*, Table 4). The estimated differences between treatment and control are similar in sign compared to the estimates for arrest, but not statistically different from zero.

## 5.4 Program effect on the use of non-police services and victim well-being

In this section, we look at information from the one-month follow-up survey to shed some light on two additional questions. The first asks whether the intervention leads to a greater use of non-police services. The second asks whether the intervention leads to a change in victims' well-being. The latter question is of particular interest given the decrease in use of police services.

Non-police services cover a number of different forms of assistance. In Table 5, we summarize the different types of services that we observe the engaged victims in the treatment group to be using. Approximately 9.2% of victims used refuge services. Furthermore, 12.3% of victims received assistance to register with a general practitioner. The most popular forms of support are counseling services (48.4% take-up) and personal safety planning (60.5% take-up).

Table 5 about here

As discussed in Section 2, non-police services are administered by a large number of independent agencies. This made the collection administrative data for our sample infeasible. Instead, we used information from the one-month follow-up survey conducted across a sample of the treatment and control group. In this survey, a number of questions will provide information, for the treatment and control group, on the use of non-police services, including visiting general practitioners, visiting accidents and emergency, whether victims have accessed one or more non-health/non-police service, and whether they are currently in

contact with the perpetrator. All questions are framed relative to, and asked approximately one month following, the initial callout.

In Figure 5, we show the difference in survey response between treatment and control group. Control group means are shown in parentheses. Outcomes corresponding to each question are binary variables equal to 1 if an affirmative response was given, and 0 otherwise. Our ability to get precise estimates is limited by the survey’s small sample. Given the sample of 214, for variables with a mean of 50%, we will require a treatment effect of over 11 percentage points to be statistically significant at the 10% level.<sup>32</sup> Treatment effect estimates for survey questions are reported in Figure 5. Dots reflect point estimates, and bars reflect the 90% confidence interval. We divide the questions into three panels: non-police service use (A), well-being and police service satisfaction (B), and stress level (C).

Figure 5 about here

The results reported in panels A and B suggest that both non-police service use and well-being measures are higher for the treatment group relative to the control group. Notably, the proportion of respondents who state that they are no longer in contact with the perpetrator is significantly higher for the treatment group (61.5%) relative to the control group (41.7%). Note that the proportion of cohabiting control group respondents is similar to the proportion of cohabiting victims and perpetrators prior to the initial callout (Table 1).

Treatment effect estimates for use of non-police services, including health services, are positive and non-trivial in magnitude. The treatment group is 12.1 percentage points more likely than the control to state they have visited their general practitioner as a result of the initial incident. The treatment group is 8.7 percentage points more likely to state they used a non-police service other than health services. However, we find no treatment effect for the proportion of victims who state they are confident in accessing help and support services.

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<sup>32</sup>The necessary magnitude of treatment effect is calculated as the product of the critical t value and the standard error corresponding to a regression of a binary outcome on a binary treatment.

This is difficult to interpret because it may reflect differences between treatment and control in awareness of what services are available due to treatment.

The results for well-being and police service satisfaction suggest that it is unlikely that victims, on average, were made worse off by the intervention, consistent with our interpretation on selective use of statement making by victims. If anything, the results suggest that victims' well-being improves following the intervention. There is a higher proportion of respondents in the treatment group, relative to the control group, reporting improvements in personal safety (6.8 pp), family life (3.6 pp), and quality of life overall (10.1 pp). The treatment group also reports higher levels of satisfaction with the service they received from police (6.7 pp). Finally, the treatment group is 15.3 percentage points more likely than the control group to state being more likely to report a future incident as a result of the police service they received.

In panel C of Figure 5, we present the results from the survey questions that we interpret as reflecting changes in stress. The treatment group is significantly less likely to state that stress levels have improved since the initial incident (-17.1 pp) and also less likely to state improvements in life control (-5.4 pp) and quality of sleep (-3.6 pp). We do not find a significant treatment/control difference for reported improvements in mental health. The results for stress are consistent with the intervention having led to victims taking action in the form of help from outside services.

## 6 Conclusions

Barriers to services often deter service users from accessing available services even when there are potential benefits. When two different services are viewed as imperfect substitutes, barriers to access in one service may have a negative externality on other service. In this paper, we study the use of a number of different available services to victims of domestic

violence. We demonstrate how a relatively simple change in the way victims are provided with information and assistance in accessing non-police services can potentially result in a more efficient use of police services.

We provide evidence from a randomized controlled trial on the effect of improving access to non-police DV support services on the distribution of users across police and non-police services.

The intervention was designed to help repeat victims of domestic abuse with support after an incident, decreasing barriers to accessing services such as refuge housing, counseling and practical support, without changing existing police resources or access to punitive action through police, the Crown Prosecution Service, or the courts.

We measure demand for access of police services through statement making, which forms an integral part of any further police engagement. We find that the intervention led to a 5.4 percentage point decrease in the provision of statements to police, or a 18.1% reduction relative to the control group. Following a simple conceptual framework described in Section 2.1, this result suggests that, for the majority of victims who took up treatment, police and non-police services are substitutes. Consistent with the model, we further argue that the decrease in statement provision can be attributed to victims for whom a statement was unlikely to lead to further police action. We investigate this by looking at statement withdraw and the advancement of cases through the criminal sanctions process. Relative to the control group, treatment group statements are 84% less likely to be withdrawn. We find that statements made in the control group have a much higher association with arrest compared to statements made in the control group. We interpret these results as an increase in the productivity of police services due to treatment.

Our results suggest that, on the margin, victims of domestic violence substitute between police and non-police services. Making non-police services easier for victims to access will hence alleviate some of the pressure on scarce police services. For our trial of 1,015 cases,

we estimate a decrease of 55 statements. When a victim statement is provided, the police are required to start further investigations. A back-of-the-envelope calculation (assuming a conservative 10 investigative hours per statement)<sup>33</sup> suggests that the intervention freed up 550 hours of police time to be allocated elsewhere. We are cautious about generalizing our results beyond the specific population involved in the trial. However, if similar results were found for all cases of domestic violence (two million in the UK annually), that would suggest an increase of approximately 1.08 million police man-hours annually, equivalent to 564 full-time officers across the UK.

An important limitation of this study is our imperfect view of non-police services. While we provide evidence that the intervention leads to higher utilization of non-police services—including medical services—we cannot calculate service-specific effects of the treatment. Therefore, it is not possible to talk about the general distributional efficiency across all public services from a cost-benefit perspective.

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.

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<sup>33</sup>Because of the large variation in the time spent on further investigation of DV cases, it is difficult to quantify the average number of hours spent by the DV investigative team, Leicestershire police provided us with a benchmark based on their professional experience of an average of 20 hours investigative time per further investigation.

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Table 1: Descriptive statistics

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

*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, \*, \*\*, and \*\*\* indicate statistical significance at a 10%, 5% and 1%.

<sup>†</sup>Binary variable equal to 1 if the same perpetrator is observed in the victim's first recorded cases, 0 otherwise.

<sup>‡</sup>Number of children conditional on having at least one child.

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

	Treatment effects			Falsification test	
	(1)	(2)	(3)	(4) t=0	(5) t>0
Treatment	-0.062** (0.028)	-0.054** (0.027)		-0.004 (0.022)	-0.056** (0.024)
Engagement			-0.106** (0.053)		
Victim female		0.017 (0.045)	0.020 (0.046)	0.008 (0.036)	0.015 (0.038)
Perp female		-0.100** (0.044)	-0.109** (0.045)	-0.073** (0.035)	-0.038 (0.037)
Victim white		0.102** (0.049)	0.097** (0.049)	0.057 (0.038)	0.066 (0.044)
Perp white		-0.093** (0.047)	-0.098** (0.047)	-0.070* (0.037)	-0.046 (0.043)
Cohabitation		0.154*** (0.029)	0.157*** (0.029)	-0.004 (0.023)	0.181*** (0.025)
Child in household		-0.003 (0.029)	0.001 (0.029)	0.023 (0.023)	-0.020 (0.025)
Previous DV <sup>†</sup>		-0.001 (0.009)	-0.001 (0.009)	0.002 (0.007)	-0.002 (0.008)
Constant	0.299*** (0.020)	0.148* (0.085)	0.147* (0.086)	0.091 (0.067)	0.046 (0.073)
<i>N</i>	1015	1015	1015	1015	878

Notes: This table reports linear probability estimates for a binary outcome, equal to 1 if the "victim" provided police with a statement, and 0 otherwise. Columns (1) and (2) report estimates of the intention to treat unconditional and conditioning on the reported control variables. Column (3) reports estimates using two-stage least squares for which assignment to the treatment group as an instrument for program uptake (excluded F = 537,  $R^2=0.367$ ). In Column (4), the outcome is equal to 1 if the person identified as "victim" provided police with a statement within 24 hours of the initial police callout, and 0 otherwise. In Column (5), the sample excludes cases for which the person identified as "victim" provided police with a statement within 24 hours of the initial police callout. Estimates in columns (2)–(5) include victim and perpetrator age and binary indicators corresponding to missing variables (coded 0). Robust standard errors are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at a 10%, 5% and 1% level.

Table 3: Treatment effect for perpetrator arrest, arrest with charges and conviction

	Arrested			Charged			Sentenced		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	-0.016 (0.027)	-0.010 (0.027)		-0.009 (0.021)	-0.004 (0.021)		-0.009 (0.017)	-0.004 (0.017)	
Engagement			-0.020 (0.052)			-0.008 (0.041)			-0.007 (0.033)
Victim female		0.067* (0.040)	0.068* (0.040)		0.010 (0.033)	0.010 (0.033)		-0.028 (0.028)	-0.028 (0.028)
Perp female		-0.131** (0.037)	-0.132*** (0.037)		-0.049* (0.029)	-0.050* (0.029)		-0.067*** (0.020)	-0.068*** (0.019)
Victim white		0.045 (0.051)	0.044 (0.051)		0.033 (0.039)	0.033 (0.038)		0.017 (0.031)	0.017 (0.030)
Perp white		-0.095* (0.051)	-0.096* (0.051)		-0.034 (0.041)	-0.035 (0.041)		-0.014 (0.033)	-0.014 (0.033)
Cohabitation		0.118*** (0.028)	0.118*** (0.027)		0.096*** (0.021)	0.096*** (0.021)		0.067*** (0.017)	0.067*** (0.016)
Child in household		-0.017 (0.029)	-0.016 (0.028)		0.007 (0.022)	0.008 (0.022)		0.013 (0.017)	0.013 (0.017)
Previous DV <sup>†</sup>		-0.001 (0.009)	-0.001 (0.009)		-0.002 (0.007)	-0.003 (0.007)		-0.004 (0.005)	-0.005 (0.005)
Constant	0.263*** (0.020)	0.197** (0.079)	0.197** (0.078)	0.133*** (0.015)	0.110* (0.061)	0.110* (0.061)	0.083*** (0.012)	0.087* (0.047)	0.087* (0.047)
N	1015	1015	1015	1015	1015	1015	1015	1015	1015

Notes: This table reports linear probability estimates for three binary outcomes. 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. For each outcome, the first two columns report estimates of the intention to treat, unconditional and conditioning on the reported control variables, as well as binary indicators corresponding to missing observations and variables for victim and perpetrator age (coefficients are small and insignificant). The third column reports two-stage least squares estimates, using assignment to the treatment group as an instrument for program uptake (first stage, excluded  $F = 537$ ,  $R^2 = 0.367$ ). Robust standard errors are reported in parentheses, \*, \*\*, and \*\*\* indicate statistical significance at a 10%, 5%, and 1% level.

Table 4: Outcomes conditioning on statement provided by victim

	Treatment	Control	Difference
<i>A. Statement retracted by victim</i>			
Any statement	0.140	0.192	-0.053 (0.046)
Statement at $t = 0$	0.235	0.275	-0.038 (0.072)
Statement at $t > 0$	0.019	0.122	-0.101** (0.049)
<i>B. Perpetrator arrested by the police</i>			
Any statement	0.744	0.636	0.105** (0.056)
Statement at $t = 0$	0.765	0.725	0.034 (0.078)
Statement at $t > 0$	0.717	0.561	0.141* (0.079)
<i>C. Perpetrator charged by the CPS</i>			
Any statement	0.397	0.371	0.022 (0.059)
Statement at $t = 0$	0.382	0.406	-0.050 (0.084)
Statement at $t > 0$	0.415	0.341	0.054 (0.084)
<i>D. Perpetrator sentenced in court</i>			
Any statement	0.240	0.245	-0.005 (0.052)
Statement at $t = 0$	0.221	0.290	-0.069 (0.075)
Statement at $t > 0$	0.264	0.207	0.057 (0.076)

Notes: This table depicts the difference between treatment and control group for perpetrator arrests, charges laid against the perpetrator and victim retraction of statements, conditioning on the provision of a statement by victim. Columns labelled *treatment* and *control* report the mean for each conditional outcome for the treatment and control groups; column difference reports the difference between these two values. Rows labelled *Statements at  $t = 0$*  condition on statement provided within the first 24 hours following the initial police visit, rows labelled *Statements at  $t > 0$*  condition on statement provided after 24 hours period.  $N = 272$ , with 137 for statement at  $t = 0$  and 135 for statement at  $t > 0$ . Robust standard error on difference reported in parenthesis. \*, \*\*, and \*\*\* indicate difference is statistically significant at a 10%, 5% and 1% level of significance.

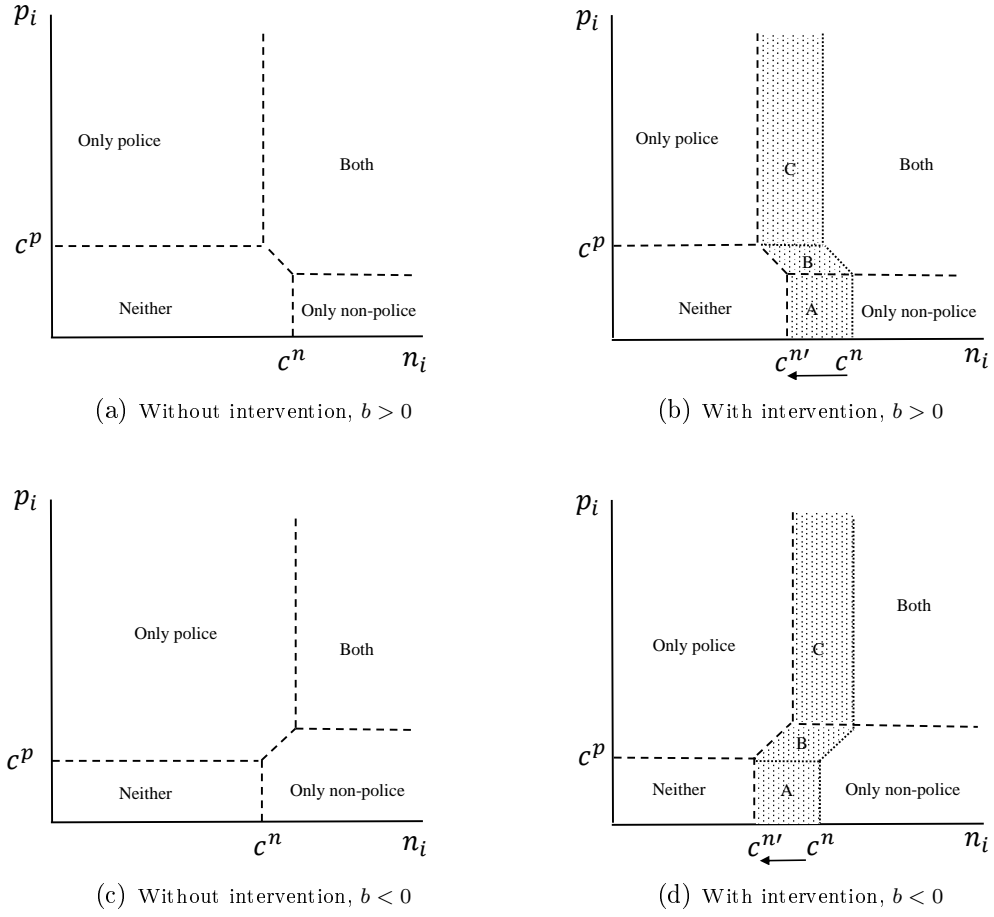
Table 5: Non-police service accessing among the engaged treatment group

Type of service	Details	% accessed <sup>†</sup>
Refuge housing		9.20
Register with GP		12.3
Grants	Supplemental support for basic household goods	16.2
Organize a solicitor		19.8
Counselling 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.

<sup>†</sup>Reflects the proportion of the 261 subjects in the treatment group who engaged with the intervention.

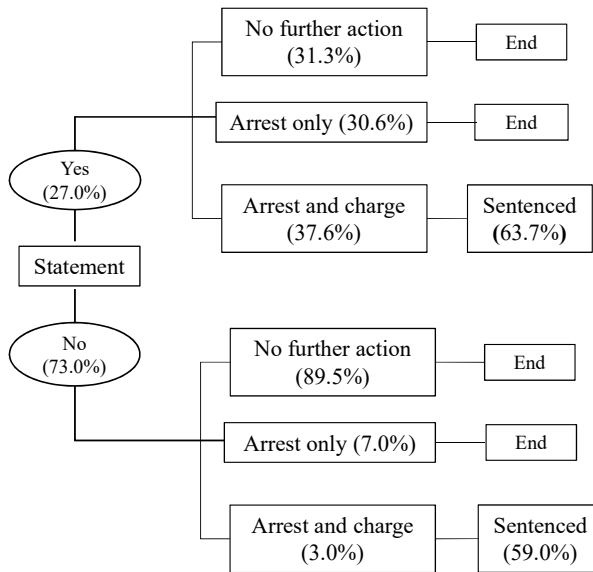
Figure 1: Access frictions and service use



*Notes:* This figure is based on equation (1) in Section 2.1 of the main text.



Figure 2: Tree representing the life of a case



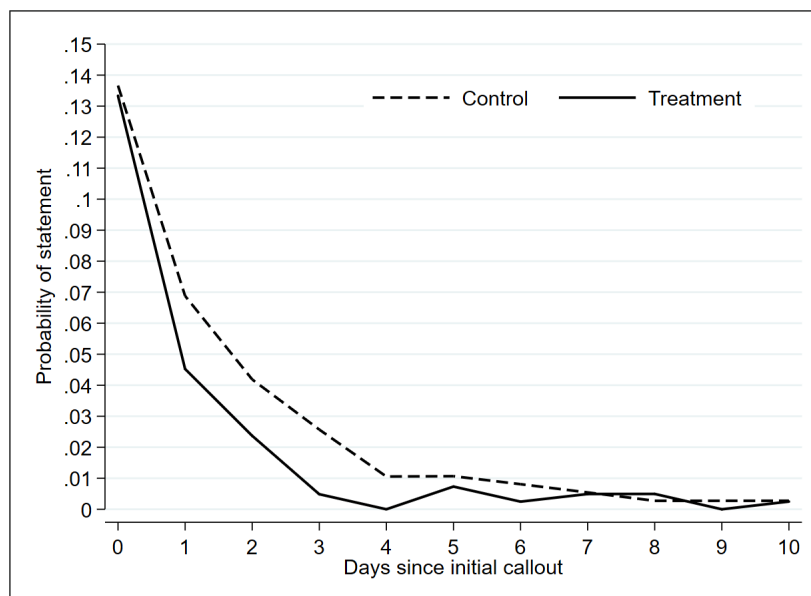
*Notes:* Percentages correspond to the probability of event conditional on position in the tree, based on subject pool data. *End* nodes indicate that no further action is taken with respect to the case.

Figure 3: Leicestershire Police Force area

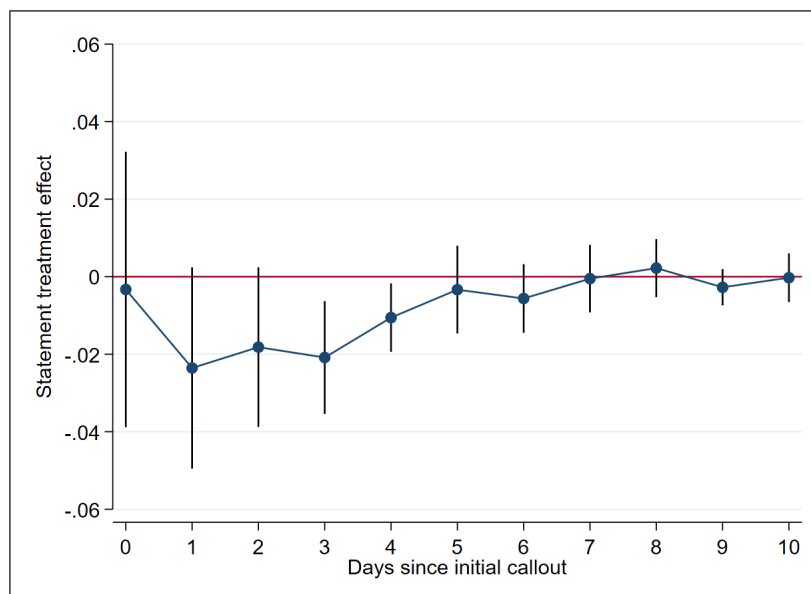


*Notes:* Map sections indicate counties for the country of Great Britain. Area in red is the Leicester Police Force area.

Figure 4: Probability of victim statement by days since initial callout and treatment



(a) Probability of statement, conditional on no previous

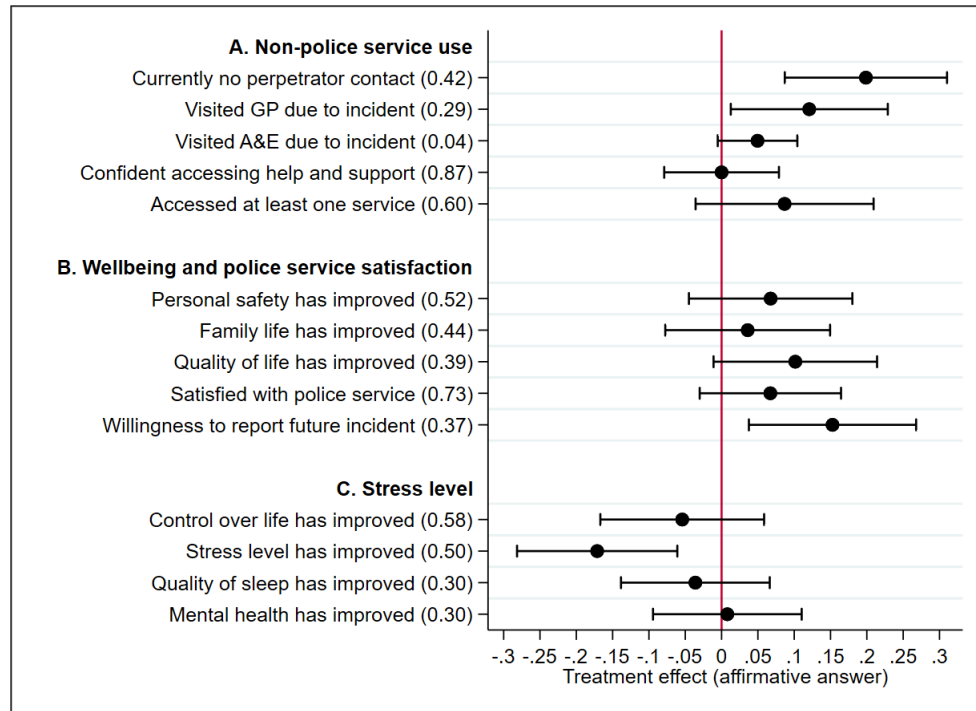


(b) Treatment effect, statement provided by days since initial callout<sup>†</sup>

*Notes:* These figures show a) the probability a statement is provided on day  $t$ , conditional on having not provided a statement previously, and b) the corresponding treatment effect by days since the initial callout. Treatment effect estimates are intention to treat, corresponding to equation (2) in the main text.

<sup>†</sup>Bars reflect 90% confidence interval.

Figure 5: 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 made into binary variables equal to 1 if the answer is affirmative, and 0 otherwise. Points show the difference between the treatment and control group for affirmative responses. Bars reflect 90% confidence interval on the difference. Mean outcome for the control group is reported in parenthesis.  $N = 214$ , with 105 in treatment and 109 in control. *Services* are defined as any non-police services, excluding health services (GP or A&E), available specifically for domestic violence.

## Appendix A Investigating 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) can 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.

During their initial phone contact with the caseworker, some victims will choose to schedule a face-to-face visit for further assistance (127 treatment group victims altogether). This meeting will often take place several days following the phone call (see Table 6). 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 6, we report the frequency of statements conditional on the length of time between the initial incident and the meeting with the caseworker.<sup>34</sup> We fail to reject the null hypothesis that the proportion of statements observed in columns (1) to (6) are statistically equivalent ( $F\text{-test } 1 = 0.430$ ), 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 ( $F\text{-test } 2 = 1.130$ ). If anything, we see an increase in the magnitude of statement making at 4–7 days relative

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<sup>34</sup>All estimates are conditional on being in the treatment group and having a face-to-face meeting.

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 see a positive correlation between time to statements and time to meeting. In Figure 6 we plot—for victims who both had a face-to-face visit and made a statement<sup>35</sup>—the correlation between 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, the relationship between meeting and statement timing is unclear (dashed red line).

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<sup>35</sup>This results in a sample of 35 observations, so results should be interpreted with caution.

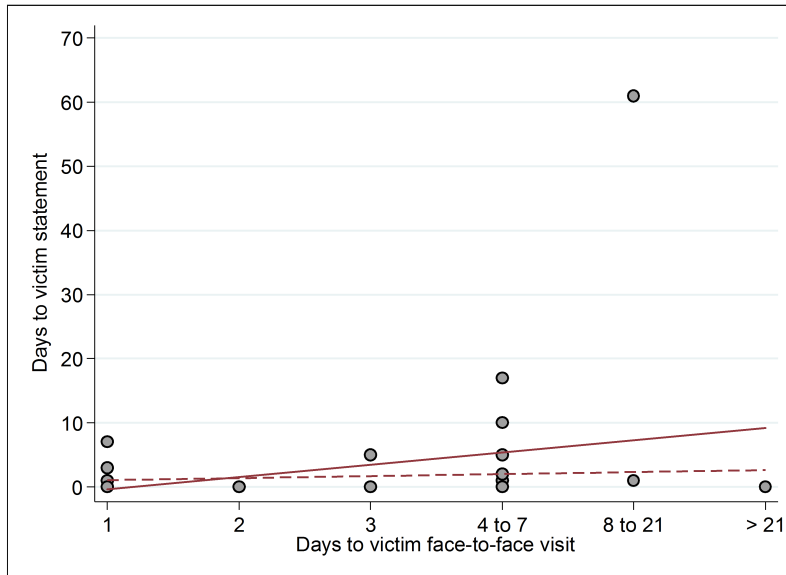
Table 6: Correlation between statement provision and time until face-to-face meeting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Days passed <sup>†</sup>	1	2	3	4 to 7	8 to 21	>21	All
Statement	0.244 (0.071)***	0.308 (0.125)**	0.250 (0.131)*	0.349 (0.069)***	0.167 (0.131)	0.250 (0.226)	0.276 (0.040)***
N	44	13	12	42	12	4	127
F-test 1	0.430 [0.830]						
F-test 2	1.130 [0.290]						

*Notes:* This table reports estimates of the probability a statement is provided conditional on the number of days between initial contact and face-to-face meeting with caseworker. Data are for the sub sample of the subject pool which is treatment group and scheduling a face-to-face meeting. \*, \*\*, and \*\*\* indicates statement probability is statistically significant at a 10%, 5% and 1% level of significance. F-test 1 corresponds to a test of the null hypothesis that estimates across columns (1) to (6) are equal. F-test 2 corresponds to a test of the null hypothesis that estimates across columns (1) and (4) are the same.

<sup>†</sup> Number of days between the initial incident and the face-to-face meeting with the case worker.

Figure 6: 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 B Details on data collection

Here we discuss in greater detail the collection of information from the Leicestershire Police administrative records.

### Administrative data

Administrative data was collected between 1<sup>st</sup> October 2014 and 30<sup>th</sup> September 2017. This data collection comprised of searching in various administrative police databases for crime numbers related to our subject pool, reading the full file for that specific case and recording relevant variables in an excel sheet specifically created for the project. The data were collected in three stages. In the initial data collection stage, we gathered the following information:

- socio-demographic data about the victims, perpetrators and the children in the household;
- data related to the domestic incident (date, classification).

In the second stage, we augmented the existing data by collecting the following information:

- data related to the domestic incident (action taken by police, DASH risk assessment);
- past history of police incidents for victims and perpetrators, and;
- for those who received treatment, details about their engagement in the program.

In the third stage, we collected the following information:

- whether the victim was involved in a police incident 3, 6 and 12 months after the initial report was filed, the nature of the incident(s);
- whether it was the same perpetrator who was involved;
- action taken by the police;
- risk assessment score.

This administrative data was collected from two main sources: the crime and intelligence system (CIS) and the general information enquiry system (GENIE). CIS was replaced by the Niche police records management system from end of April 2015. For cases in the treatment group, details about engagement were recorded from the caseworker reports. The 3, 6, 12 and 24 month police incidents were recorded from GENIE and the Niche systems. Data collection was done by research assistants hired purposefully for this task, and

overseen by the lead researcher (one of the authors). A separate research assistant checked a 30% sample of all records to ensure no systematic errors.

Data collection and data merging is based on the unique crime reference numbers assigned to the case corresponding to the initial callout. After data collection was completed, the dataset was anonymized and this number was replaced by a unique ID given by the lead researcher.

The final dataset comprised of 1,017 cases (507 control and 510 treatment).

## **Victim surveys**

Data was collected via telephone survey from victims in both the treatment and the control group. The sample proportions were 21.6% for the treatment and 20.6% for the control group, having received in total 214 responses (21.3%). The primary researcher compiled a dataset containing victim's contact details and information about the incident for the Leicestershire Police survey team on a monthly basis. The data gathered asked about aspects of how the victim's life has been affected (quality of sleep, safety, stress levels, family life, mental health, etc.) by the incident, about their opinion and satisfaction with how the police handled their case, what (if any) agencies were contacted. The completed surveys were sent back to the primary researcher who then merged these responses with the administrative data based on the unique crime reference number.

The full survey can be found on our project website:

<https://prj360.org/the-evaluation-of-project-360>

## Appendix C Support services



### Emergency numbers

**999** Police  
Ambulance  
Fire

**24hour number**

**0808 2000 247**  
National Domestic  
Violence Helpline

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

<p><b>Domestic Violence Integrated Response Project (DVIIRP)</b> <b>0116 255 0004</b> (helpline)</p> <p><b>DV specific agency or post</b> Agency</p> <p><b>Client Group and Remit</b> Women and men aged 16 and over in the city and the county who are affected by domestic violence.</p> <p>Men who self refer as perpetrators of domestic violence.</p> <p>Specialist service for members of faith communities aged 16 and over, particularly members of black and minority ethnic communities.</p> <p>A specific network of workers offering therapeutic support to children and young people who have experienced domestic violence.</p> <p><b>Referral Process</b> Self referral</p> <p><b>Service Offered</b></p> <ul style="list-style-type: none"> <li>• Listening Ear</li> <li>• Safety Planning and Options</li> <li>• Support Advocacy</li> <li>• Information</li> <li>• Contact with religious scholars</li> <li>• Face to face support</li> <li>• Therapeutic support with young people (individual and group) and with their carer/parent</li> </ul>	<p><b>Women's Aid Leicestershire Ltd Community Outreach Service</b> <b>0116 285 8079</b> (Voluntary)</p> <p><b>DV specific agency or post</b> Agency</p> <p><b>Client Group and Remit</b> 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.</p> <p>Aged 16 and over.</p> <p><b>Referral Process</b> Self referral, referral through any agency, including housing, and also through family and friends.</p> <p><b>Service Offered</b></p> <ul style="list-style-type: none"> <li>• Emotional Support either face-to-face or by telephone</li> <li>• Housing related support</li> <li>• Legal advice</li> <li>• Practical (including finance) advice &amp; information</li> <li>• Advocacy</li> <li>• Group work</li> <li>• Social/cultural events</li> <li>• Access to safe accommodation</li> </ul>	<p><b>Leicestershire Women's Aid Refuge Service</b> <b>0116 244 0169</b> (Voluntary)</p> <p><b>DV specific agency or post</b> Agency</p> <p><b>Client Group and Remit</b> Women with or without children who need somewhere safe to stay due to fear of domestic violence.</p> <p>Aged 16 and over (housing benefit needs to be secured, or alternative finance for rent).</p> <p>Male children over 14years may not be considered appropriately placed within the refuge.</p> <p><b>Referral Process</b> Self referral, referral through any agency, including housing, and also through family and friends.</p> <p><b>Service Offered</b></p> <ul style="list-style-type: none"> <li>• Housing</li> <li>• Individual case - work support including help with finances, legal matters and re-housing</li> </ul>	<p><b>Suruksha</b> <b>0116 274 0422</b> (Voluntary)</p> <p><b>Client Group and Remit</b> Supported housing project for asian women who have fled their home due to domestic violence.</p> <p><b>Referral Process</b> Self referral, agency referral</p> <p><b>Service Offered</b></p> <ul style="list-style-type: none"> <li>• Housing support</li> <li>• Individual casework</li> </ul>	<p><b>Panahghar Shantighar Outreach Support Service</b> <b>0116 270 5320</b> (Voluntary)</p> <p><b>DV specific agency or post</b> Agency</p> <p><b>Client Group and Remit</b> Asian Women with or without children who have experienced domestic violence who are living independently and would like support.</p> <p><b>Referral Process</b> Self referral, referral through other refuge projects</p> <p><b>Service Offered</b></p> <ul style="list-style-type: none"> <li>• Individual face to face support</li> </ul>	<p><b>Panahghar Shantighar Outreach Support Service</b> <b>0116 270 5320</b> (Voluntary)</p> <p><b>DV specific agency or post</b> Agency</p> <p><b>Client Group and Remit</b> Asian women with or without children who have experienced domestic violence and need somewhere safe to stay.</p> <p><b>Referral Process</b> Self referral, referral through other refuge projects, police referral.</p> <p><b>Service Offered</b></p> <ul style="list-style-type: none"> <li>• Housing</li> <li>• Individual case-work support including help with finances, legal matters and re-housing</li> </ul>	<p><b>Family Welfare Association FLIP Project</b> <b>0116 255 3738</b> (Voluntary)</p> <p><b>DV specific agency or post</b> Posts</p> <p><b>Client Group and Remit</b> 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.</p> <p><b>Referral Process</b> Self-referral</p> <p><b>Service Offered</b></p> <ul style="list-style-type: none"> <li>• Agency referral where parent has given consent</li> <li>• 12 week 'freedom' programme</li> </ul>
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### 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 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.

# 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

\* 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.

Table 7: 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	Counselling and emotional support services.
Juniper Lodge	Charitable	Sexual assault counselling 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.
Panahgarh 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	Counselling 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. Reflects service provision in the Leicestershire Police Force area for the period November 2014 to July 2015.