

For Online Publication

Appendices to accompany “The impact of improving access to support services for victims of domestic violence on demand for services and victim outcomes.”

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Appendix A Background information on support services

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

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

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

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

Domestic Violence

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

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 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
 - Video recorded interviewing facilities
 - Information

Witness Cocoon

0116 222 9886
(Voluntary)

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

Referral Process
Self referral or referral from any agency

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

Victim Support & Witness service

0116 253 0101
(Voluntary)

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

Referral Process
Self referral, Police referral and other agencies

Service Offered

- Face to face support, information and advice

Bridge House

0116 222 1845
(Voluntary)

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

Referral Process
Self referral, agency referral

- Service Offered**
- Safe supported housing

New Futures

0116 255 9696
(Voluntary)

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

Youth work project

Referral Process
Self referral

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

* This list is not 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.



Source: Leicester City Council, permissions granted for reproduction. Information provided in this leaflet was correct as of August 2014.

Table A.2: Leicestershire non-police service providers

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

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

Appendix B Conceptual framework

B.1 Service use and barriers to services

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

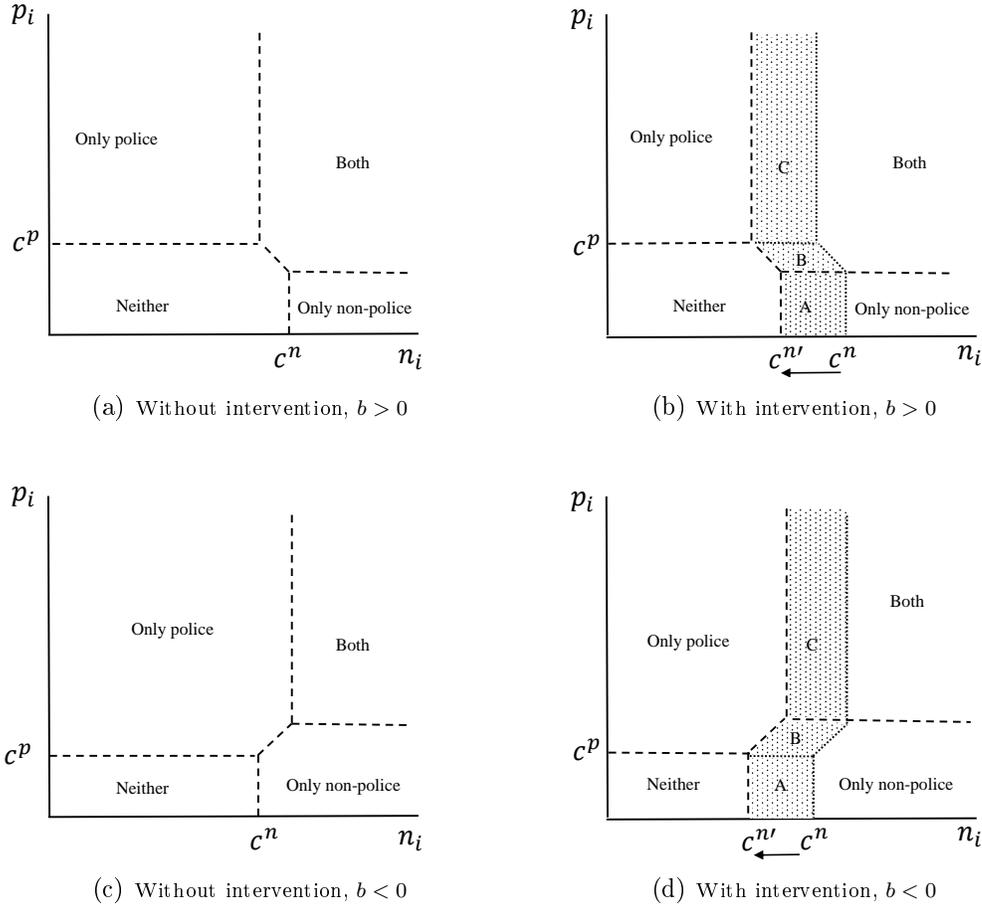
Consider a model in which individuals, denoted by i , choose between police and non-police services. Each service results in individual-specific benefits denoted by $p_i \geq 0$ from the police services and $n_i \geq 0$ from the non-police services. If both services are accessed, individuals also receive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

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

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

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

Figure B.1: Access frictions and service use



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

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

police services. In examining the demand for police services, we learn about the sign of b , reflecting whether the two types of services are complements or substitutes.

B.2 Statement making and productivity of police services

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

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

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

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

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

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

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

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

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

B.3 A cooling off period as an alternative hypothesis

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

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

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

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

We propose two tests of TIP using our data. First, if TIP is driving the change in statements, we expect to see a negative correlation between the length of time between the cooling off period (time between the phone call and the meeting) and statement provision. In Table B.1, we report the frequency of statements conditional on the length of time between the initial incident and the meeting with the caseworker.⁴ We fail to reject the null hypothesis that the proportion of statements observed in columns (1) to (6) are statistically equivalent (F -test = 0.460, $p=0.803$), suggesting statement probability does not vary with meeting times. We also fail to reject that the proportion of statements for 1-day meetings and 4–7 day meetings, the lengths of time with the most observations, are equivalent (F -test = 1.410, $p=0.237$). If anything, we see an increase in the magnitude of statement making at 4–7 days relative to 1-day.

We can also check, among victims who make statements, if scheduling later face-to-face meetings means their statement is made later. If this is true, we expect to see a positive correlation between time to statements and time to meeting. In Figure B.2 we plot—for victims who both had a face-to-face visit and made a statement⁵—the correlation between

⁴All estimates are conditional on being in the treatment group and having a face-to-face meeting.

⁵This results in a sample of 35 observations, so results should be interpreted with caution.

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

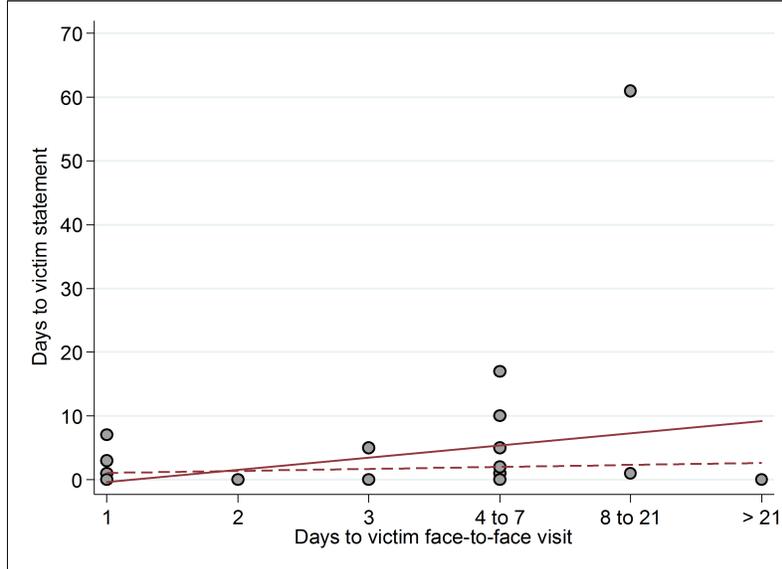
Table B.1: Correlation between statement provision and time until face-to-face meeting

	Days from initial incident [†]						
	1 (1)	2 (2)	3 (3)	4 to 7 (4)	8 to 21 (5)	>21 (6)	All (7)
Statement provided	0.233 (0.069)	0.308 (0.126)	0.250 (0.131)	0.349 (0.069)	0.167 (0.131)	0.250 (0.226)	0.276 (0.040)
N	43	13	12	43	12	4	127
<i>F-stat</i> (Columns 1–6 equal)	0.460 [0.803]						
<i>F-stat</i> (Columns 1 and 4 equal)	1.410 [0.237]						

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

[†] Number of days between the initial incident and the face-to-face meeting with the caseworker.

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



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

Appendix C Internal Review Board approval

The research protocol of the evaluation of the intervention was reviewed and approved by the Research Ethics Committee of the University of Leicester under application number mk332-5e3e and has been registered with the AEA RCT Registry number AEARCTR-0000537. The protocol has also been reviewed and approved by an internal review board of the Leicestershire Police Force. As the intervention was run by the Leicestershire Police Force, no informed consent was required from individuals in the subject pool regarding their participation. The IRBs also agreed that the collection of anonymized data from police administrative records (Leicestershire Police Database and the Police National Database) would therefore not require informed consent. Collection of the Victim Survey data was completed by the Leicestershire Police Information Services Unit for the evaluation of the trial. Using a police

embedded survey team ensured that no sensitive information was shared with the external university researchers and that all interactions with victims were through a team member trained in dealing with victims of domestic abuse. Survey team members followed a dedicated safety protocol during the interview, a condition set by the IRBs. The protocol required that:

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

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

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

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

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

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

Are you happy for me to continue with the survey?

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

Appendix D Administrative data

D.1 Collection of administrative data

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

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

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

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

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

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

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

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

Appendix E Survey data

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

E.1 Collection of the survey data

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

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

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

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

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

E.2 Survey balance and representativeness

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

The first concern with this type of study is that treatment may affect survey participation, giving rise to non-random selection of the type addressed in Lee (1995). For example, it is reasonable to be concerned that treatment leads to victims feeling more engaged with the police and therefore more willing to participate in the survey. If this is the case, we expect to see the treatment group over-represented as a proportion of the total completed surveys. The surveyed sample consists of 214 observations (21.3% of the total sample), 105 in the treatment group (20.6% of total treatment) and 109 in the control group (21.3% of

total control). The difference in the proportion of individuals in the treatment and control groups who completed the survey is small and not statistically significant ($p = 0.698$). We also compare stats about survey completion across the treatment and control groups. The number of days between the initial callout and the survey for the treatment group (38.3 days) and control group (38.6 days) are not significantly different ($p = 0.906$). The difference in time spent completing the survey, 13.1 minutes for the treatment and 13.8 minutes for the control group, is also not statistically significant ($p = 0.566$). The similarity between the two groups is consistent with random sampling from the pool of study cases and selection into the survey being uncorrelated with treatment status.

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

A second concern is that, although balanced across treatment and control, the surveyed cases may not be representative of all cases in the administrative data. If this is the case, the treatment effect that we estimate from the survey outcomes may over- or under-represent the ITT that we would get for the outcomes from the full dataset. To explore this, we first compare the mean pre-treatment characteristics of cases in the full dataset to the same characteristics for cases for which we have a completed survey (Table E.2). A number of characteristics differ across the two groups (Column 3). Specifically, victims in the surveyed

cases are more likely to be female, have fewer total recorded cases of domestic violence (although the number in the previous year is the same), and are significantly more likely to be living with the perpetrator. We further investigate this by regressing an indicator dummy, equal to 1 if the observation has a completed survey and 0 otherwise, on the pre-treatment characteristics. The coefficients of this regression are reported in column (4) of Table E.2. The number of previous domestic cases and cohabitation status remain significant predictors of a completed survey (the regression F-stat is 1.21, $p=0.094$).

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

	Control	Treatment	Difference	N
<i>A. Victim characteristics</i>				
Female	0.89 (0.314)	0.943 (0.233)	0.053 (0.038)	214
Age	34.22 (12.800)	35.56 (12.370)	1.340 (1.721)	214
White	0.89 (0.314)	0.829 (0.379)	-0.061 (0.048)	209
Domestic cases (365 days)	2.514 (1.507)	2.267 (1.325)	-0.247 (0.194)	214
Registered domestic cases	8.917 (7.975)	11.190 (11.580)	2.273 (1.364)	214
Risk assessment score	1.202 (0.590)	1.267 (0.683)	0.065 (0.087)	201
<i>B. Perpetrator characteristics</i>				
Female	0.128 (0.336)	0.086 (0.281)	-0.042 (0.042)	214
Age	31.680 (10.720)	33.050 (11.380)	1.370 (1.512)	214
White	0.853 (0.356)	0.705 (0.458)	-0.148 (0.056)	198
Domestic cases (365 days)	2.780 (2.428)	2.219 (1.901)	-0.561 (0.298)	214
Registered domestic cases	10.170 (9.375)	11.280 (9.935)	1.110 (1.321)	214
<i>C. Household characteristics</i>				
Same victim and perpetrator [†]	0.587 (0.495)	0.438 (0.499)	-0.149 (0.068)	214
Intimate partner	0.780 (0.416)	0.752 (0.434)	-0.028 (0.058)	210
Cohabitation	0.706 (0.458)	0.676 (0.470)	-0.030 (0.063)	209
Children in household	0.716 (0.453)	0.600 (0.492)	-0.116 (0.065)	214
Number of children [‡]	1.910 (0.885)	1.921 (1.067)	0.011 (0.168)	141
<i>F-stat*</i> [<i>p-value</i>]			0.968 [0.556]	

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

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

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

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

Table E.2: Characteristics of cases with and without a completed survey

	Mean values no survey cases (1)	Mean values surveyed cases (2)	Difference in means (3)	Regression of survey dummy (4)
<i>A. Victim characteristics</i>				
Female	0.861	0.916	0.054 (0.023)	0.068 (0.037)
Age	34.341	34.879	0.538 (0.961)	0.002 (0.001)
White	0.829	0.880	0.052 (0.026)	0.061 (0.043)
Domestic cases (365 days)	2.269	2.393	0.124 (0.112)	-0.001 (0.010)
Registered domestic cases	11.541	10.033	-1.508 (0.786)	0.000 (0.001)
Risk assessment score	1.268	1.313	0.046 (0.045)	0.002 (0.027)
<i>B. Perpetrator characteristics</i>				
Female	0.147	0.107	-0.039 (0.025)	-0.026 (0.040)
Age	33.442	32.350	-1.091 (0.867)	-0.002 (0.001)
White	0.802	0.843	0.042 (0.030)	-0.001 (0.045)
Domestic cases (365 days)	2.165	2.505	0.340 (0.165)	0.018 (0.009)
Registered domestic cases	11.474	10.715	-0.759 (0.758)	-0.002 (0.001)
<i>C. Household characteristics</i>				
Same victim and perpetrator [†]	0.428	0.514	0.086 (0.039)	0.058 (0.029)
Intimate partner	0.779	0.781	0.002 (0.032)	-0.070 (0.038)
Cohabitation	0.523	0.708	0.185 (0.036)	0.140 (0.027)
Children in household	0.556	0.659	0.103 (0.037)	0.070 (0.047)
Number of children [‡]	1.966	1.915	-0.051 (0.094)	-0.007 (0.019)

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

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

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

We check the sensitivity of our results reported in Section 4.2 of the main paper by rerunning the survey results using alternative estimators. These estimates are reported in Table E.3. The first three columns in Table E.3 report OLS and weighted OLS estimates: Column (1) reports the unconditional treatment-control difference in mean values for each of the survey outcomes; Column (2) reports the preferred estimates (corresponding to Figure 4 in the main paper); Column (3) reports estimates weighted to match means of the full sample across previous cases, sex of the victim, and cohabitation⁷. Across all estimates, the survey results are notably stable. There is little difference between the magnitude of weighted and unweighted estimates.

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

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

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

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

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

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

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

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

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

Table E.3: Survey results, alternative estimates

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

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

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

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

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

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

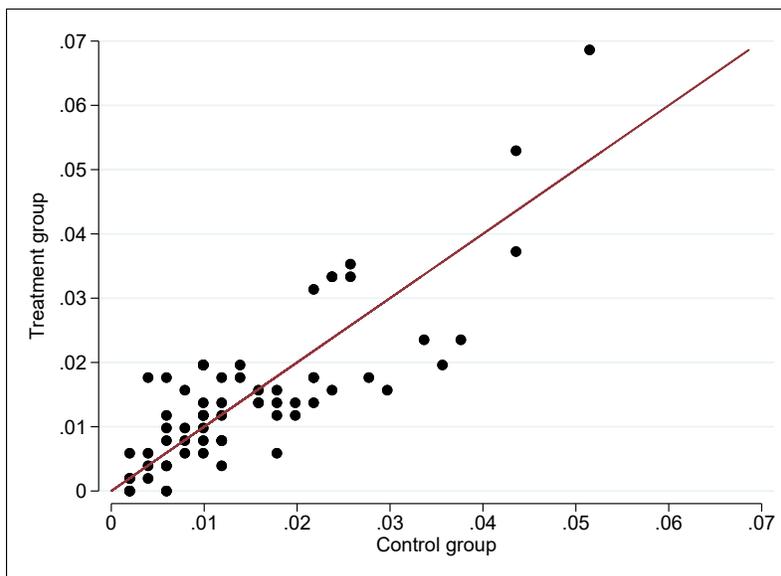
We report these bounds in Table E.4. While we cannot rule out these extremes based on the information we have, intuitively they seem highly unlikely. For this reason we also report point estimates based on the more plausible assumption that there was no treatment effect for the $survey = 0$ group. For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

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

	Lower bound		Upper bound
	$ITT(survey = 0)$	$ITT(survey = 0)$	$ITT(survey = 0)$
	$= -1$	$= 0$	$= 1$
	(1)	(2)	(3)
A. Non-police service use			
Visited GP due to incident	-0.011	0.150	0.311
Visited A&E due to incident	-0.107	0.054	0.215
Accessed at least one service	-0.053	0.108	0.268
B. Decrease in risk of repeat victimization			
Currently no perpetrator contact	0.002	0.163	0.324
Willingness to report future incident	-0.025	0.136	0.297
Personal safety has improved	-0.157	0.004	0.164
C. Other well-being measures			
Family life has improved	-0.199	-0.038	0.123
Quality of life has improved	-0.197	-0.037	0.124
Control over life has improved	-0.230	-0.069	0.092
Stress level has improved	-0.356	-0.195	-0.034
Quality of sleep has improved	-0.243	-0.082	0.079
Mental health has improved	-0.241	-0.081	0.080

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

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



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

Appendix F Additional analysis

F.1 Treatment-control group balance across geography

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

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

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

Several characteristics stand out as noteworthy. Sex of the perpetrator is significantly associated with engagement. Engagement rates are lower in cases where the perpetrator is female. However, this appears to be due to a significant decrease in the likelihood of a caseworker making contact ($p=0.059$); conditional on making contact, sex of the perpetrator does not have a significant association with engagement ($p=0.755$). It is also interesting that the sex of the victim does not appear to be as significant a determinant for engagement. Contact by the caseworker is independent of age, but a one standard deviation increase in either victim or perpetrator age (approximately 12 years) is associated with more than a 5 percentage point increase in the engagement rate when contact is made ($p=0.074$ and

$p=0.080$ for victim and perpetrator). Victims with more previous cases are less likely to engage when contacted; a standard deviation increase in previous cases (approximately 1.5 cases) is associated with a 5.0 percentage point decrease in engagement ($p=0.075$). Finally, a higher risk assessment score of the responding officers is significantly associated with an increase in engagement. This is both through a greater likelihood of making contact ($p=0.011$), and to a lesser extent, through greater engagement once contact is made ($p=0.148$).

F.3 Timing of repeat domestic incidents

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

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

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

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

F.4 Local average treatment effects

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

$$engage_i = \lambda_0 + \lambda_1 treat_i + X_i' \Lambda + v_i \quad (\text{F.1})$$

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

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

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

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

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

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

effect on outcomes.

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

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

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

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

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

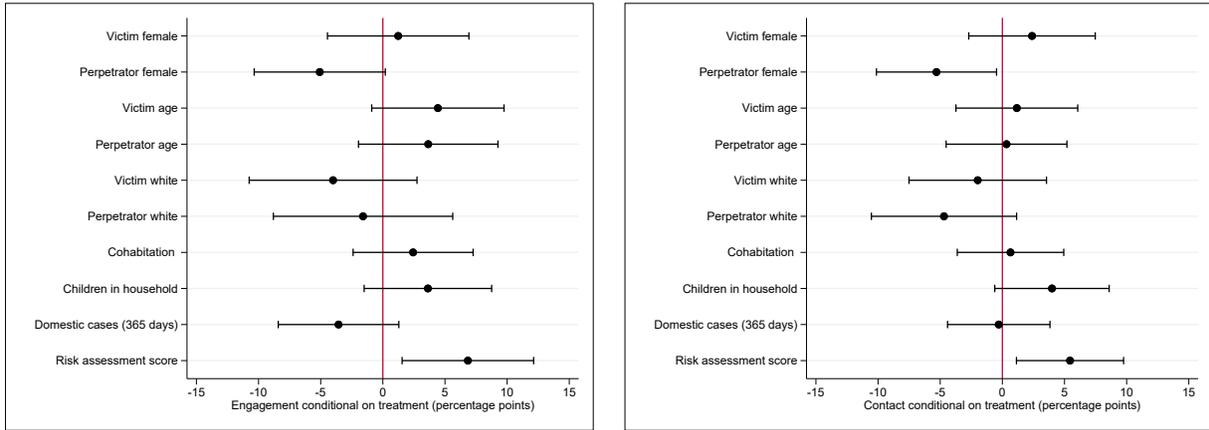
The first assumption is satisfied from the randomization of cases into treatment and control. The second assumption requires that it is only through the intervention that treatment status affects the outcomes. As discussed in Section 3.4 of the main paper, the design features of this RCT ensure that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

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

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

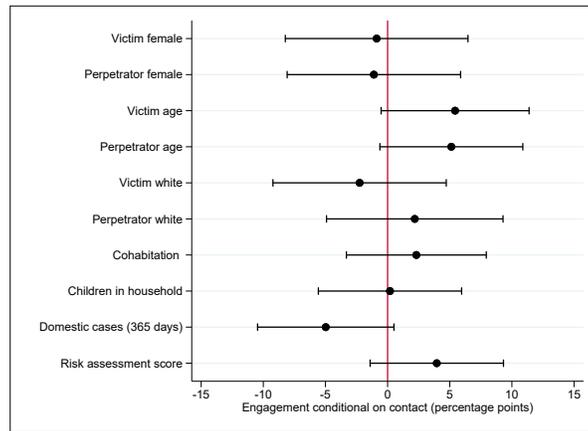
¹⁰Of course, we cannot determine how large this effect is relative to statement provision among the unobservable subset of the control group that would take up the intervention had they been offered.

Figure F.2: Characteristics by intervention engagement



(a) Contacted and engaged

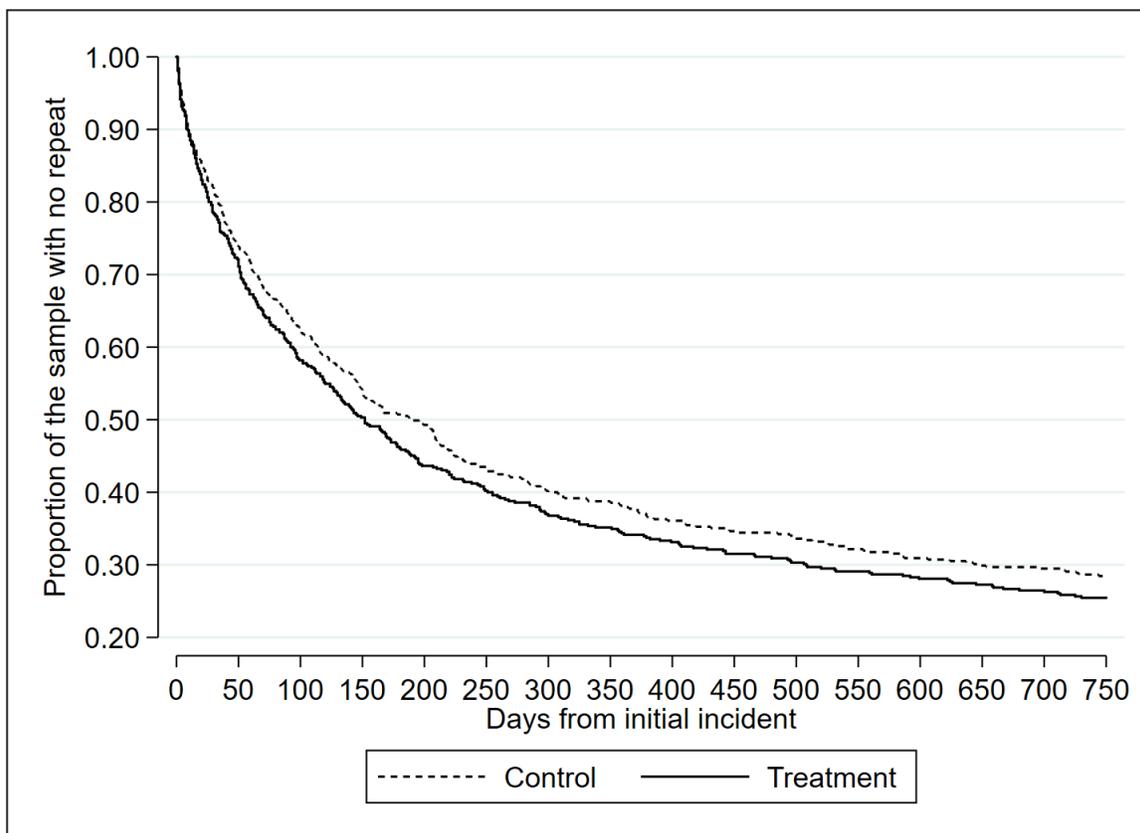
(b) Contacted



(c) Engaged conditional on contact

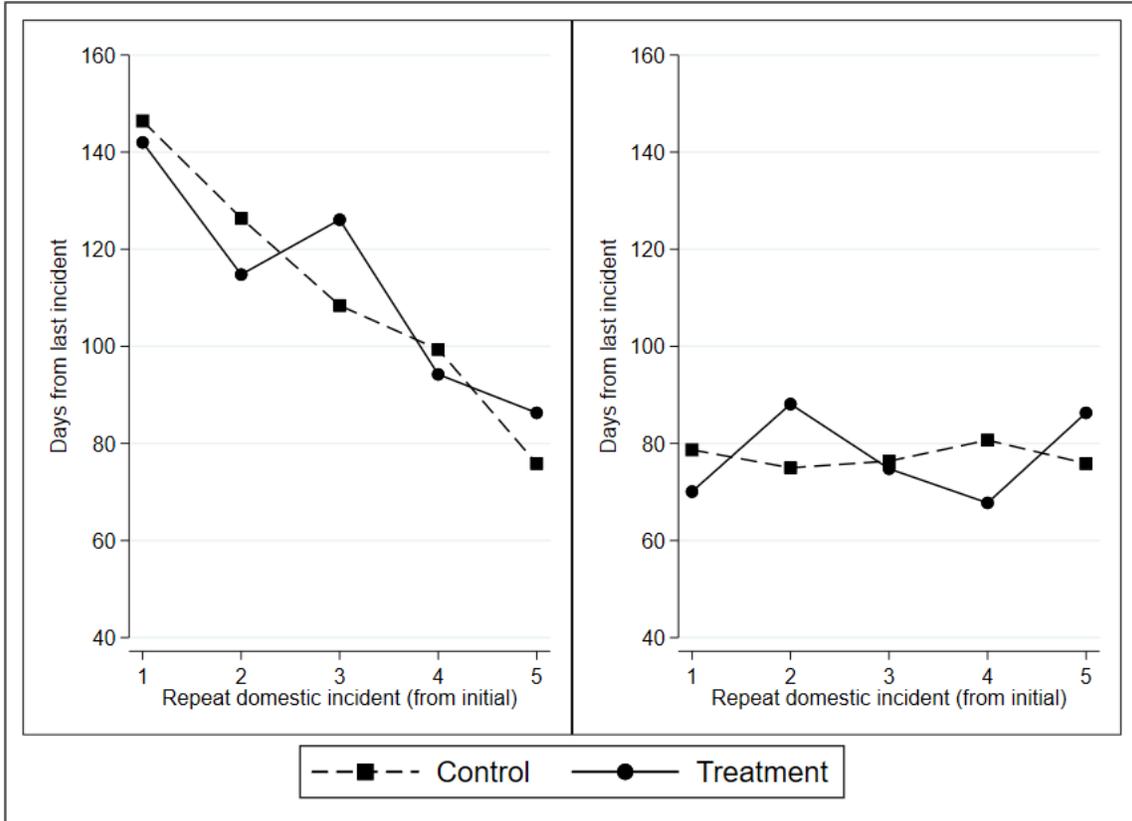
Notes: These figures report the coefficients corresponding to a regression of binary indicators for three outcomes for cases in the treatment group: a) Contacted by the caseworker and engaged with the intervention ($R^2 = 0.1693$, $N = 510$); b) Contacted by the caseworker ($R^2 = 0.1986$, $N = 510$);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

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



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

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



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

Table F.1: Local average treatment effect estimates

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

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

F.5 Treatment effect heterogeneity

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

$$S_i = \lambda_0 + \lambda_1 \text{treat}_i + \lambda_2 \text{highrisk}_i + \lambda_3 \text{treat}_i \times \text{highrisk}_i + X_i' \Lambda + u_i, \quad (\text{F.7})$$

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

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

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

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

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

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

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

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

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

age points higher for high-risk subjects than subjects who are not high-risk ($p=0.215$).

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

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

Table F.3: Survey outcomes, heterogeneous treatment effects

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

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

Appendix G Details of intervention cost analysis

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

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

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

We calculate the cost of police time based on official figures from the National Police Chief Council (NPCC, 2019) on the costing of police services. The full cost of a full-time officer at the rank of police constable (the lowest rank) in 2017 is £88,662; and £107,517 for a police sergeant (the next higher rank). These cost include employer National Insurance

contributions, and the police-specific allowances and pension contributions, and direct overheads, for example police uniform, insurance etc. Based on 208 net working days and 7.25 productive hours per shift this equates to an hourly cost of a full-time officer at the rank of police constable in 2019 of £58.99 and £71.50 for police sergeant, respectively.

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

Appendix H One month victim survey

Leicestershire Pilot Domestic Abuse Survey

Before contacting the victim, complete questions 0 to 3

Q1 Name

Q2 Reported

Q3 Crime Number:

Q4 Is there a safe telephone number

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

Q5 Is the phone number a....

- Land Line number
- Mobile number

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

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

My name is ____ from Leicestershire Police.

Q6 Is it safe to speak to you now?

- Yes
- No

Q7 For the purpose of ensuring your safety, can I ask is there any possibility that this call could be overheard by the person who caused you harm?

- Yes
- No

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

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

Text to introduce the survey:

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

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

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

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

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

telephone - XXXXX
or email XXXXX

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

Q8 Are you happy for me to proceed and ask you some questions?

(PAUSE FOR RESPONSE)

- Yes
- No

Reason for not taking part (DO NOT ASK) _____

Q9 In case we get cut off can I check your current location - are you at home?

- Yes
- No

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

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

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

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

- Q10 Since making this report, my safety has...
- Improved a lot
 - Improved a little
 - No Difference
 - Declined a little
 - Declined a lot
 - Don't know
- Q11 Since making this report, my control over my life has...
- Improved a lot
 - Improved a little
 - No Difference
 - Declined a little
 - Declined a lot
 - Don't know
 - Partially Completed*
- Q12 Since making this report, my stress levels have...
- Improved a lot
 - Improved a little
 - No Difference
 - Declined a little
 - Declined a lot
 - Don't know
 - Partially Completed*
- Q13 Since making this report, my quality of sleep has...
- Improved a lot
 - Improved a little
 - No Difference
 - Declined a little
 - Declined a lot
 - Don't know
 - Partially Completed*
- Q14 Since making this report, my mental health has....
- Improved a lot
 - Improved a little

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

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

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

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

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

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

Q17 I currently have ongoing contact with this person

- Agree
- Disagree
- Partially Completed*

Q18 The reason for the ongoing contact is:

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

Please specify:

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

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

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

- Q20 As a direct result of this report, I have...
- | | Yes | No | Prefer not to say |
|---|--------------------------|--------------------------|--------------------------|
| Visited my GP | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> |
| Visited A&E (Accident and Emergency Department) | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> |

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

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

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

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

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

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

Please specify

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

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

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

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

Q26 Why do you say that?

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

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

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

Q28 Why do you say that?

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

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

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

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

Q31 And has your opinion changed to:

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

Q32 Why do you say that?

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

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

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

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

- Yes
- No

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

- Yes
- No

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

- Telephone
- Email
- Text Message
- Letter
- Other

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

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

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

If *Partially Completed*, please state why.

Close interview

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

Q38 LPU

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

Q39 Researchers Collar Number

Q40 Investigating Officers Collar Number

Appendix I DASH risk assessment tool

CAADA-DASH Risk Identification Checklist (RIC)¹ for MARAC Agencies

Aim of the form:

- To help front line practitioners identify high risk cases of domestic abuse, stalking and 'honour'-based violence.
- To decide which cases should be referred to MARAC and what other support might be required. A completed form becomes an active record that can be referred to in future for case management.
- To offer a common tool to agencies that are part of the MARAC¹ process and provide a shared understanding of risk in relation to domestic abuse, stalking and 'honour'-based violence.
- To enable agencies to make defensible decisions based on the evidence from extensive research of cases, including domestic homicides and 'near misses', which underpins most recognized models of risk assessment.

How to use the form:

Before completing the form for the first time we recommend that you read the full practice guidance and Frequently Asked Questions and Answers². These can be downloaded from http://www.caada.org.uk/marac/RIC_for_MARAC.html. Risk is dynamic and can change very quickly. It is good practice to review the checklist after a new incident.

Recommended Referral Criteria to MARAC

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

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

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

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

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

What this form is not:

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

¹ For further information about MARAC please refer to the 10 Principles of an Effective MARAC:

http://www.caada.org.uk/marac/10_Principles_Oct_2011_full.doc

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

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

CAADA-DASH Risk Identification Checklist for use by IDVAs and other non-police agencies³ for identification of risks when domestic abuse, 'honour'-based violence and/or stalking are disclosed

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

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

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

Bail conditions <input type="checkbox"/> Non Molestation/Occupation Order <input type="checkbox"/> Child Contact arrangements <input type="checkbox"/> Forced Marriage Protection Order <input type="checkbox"/> Other <input type="checkbox"/>				
24. Do you know if (.....) has ever been in trouble with the police or has a criminal history? (If yes, please specify.) DV <input type="checkbox"/> Sexual violence <input type="checkbox"/> Other violence <input type="checkbox"/> Other <input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Total 'yes' responses				
<p>For consideration by professional: Is there any other relevant information (from victim or professional) which may increase risk levels? Consider victim's situation in relation to disability, substance misuse, mental health issues, cultural/language barriers, 'honour'- based systems, geographic isolation and minimisation. Are they willing to engage with your service? Describe:</p> <p>Consider abuser's occupation/interests - could this give them unique access to weapons? Describe:</p> <p>What are the victim's greatest priorities to address their safety?</p>				
<p>Do you believe that there are reasonable grounds for referring this case to MARAC? Yes / No</p> <p>If yes, have you made a referral? Yes/No</p> <p>Signed: _____ Date: _____</p>				
<p>Do you believe that there are risks facing the children in the family? Yes / No</p> <p>If yes, please confirm if you have made a referral to safeguard the children: Yes / No</p> <p>Date referral made</p>				
<p>Signed: _____</p> <p>Name: _____</p>			<p>Date: _____</p>	

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

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