Article type: Original research

Title: Policing a new domestic abuse crime: Effects of force-wide training on arrests for coercive control

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Abstract

Objective: Following a pre-registered study design, estimate the effect of police force-wide domestic abuse training on arrests for the new crime of 'controlling or coercive behaviour'.

Methods: Using data on monthly count of arrest for controlling or coercive behaviour in 33 police forces, we employed a negative binomial difference-in-difference analysis and capitalised on differences in intervention timing to undertake an event study.

Results: Training was associated with a 41% increase in arrest for controlling or coercive behaviour for trained forces compared to untrained forces (IRR 1.413, 95% CI 1.235–1.617). The event study illustrated that the increase in arrests in trained forces was consistent with the timing of the training.

Conclusions: Training entire police forces to understand the dynamics of domestic abuse, including the new offence of coercive control, is effective in increasing the rate of arrest for coercive control.

Key words: Coercive control; Difference-in-differences; Domestic abuse; Event study; Police training

Introduction

In December 2015, England and Wales became the first jurisdiction to criminalise coercive control. Police forces, which were already subject to much criticism for their performance on domestic abuse, were faced with a broader, more nuanced definition which challenged the 'violent incident' model that had previously dominated criminal justice system practice and thinking. This paper contextualises and, following a pre-registered study design, evaluates a police training intervention that was designed to change how frontline police officers understand and respond to domestic abuse, particularly the new crime of coercive control.

Criminal justice practice around domestic abuse is the subject of much criticism throughout the world (Buzawa and Buzawa, 2017; Stark 2007; Tuerkheimer 2004). In particular, it has been suggested that the dominant 'violent incident model' (Stark 2012), whereby crimes are investigated and prosecuted as discrete and unconnected acts, 'conceals the reality of an ongoing pattern of conduct occurring within a relationship characterized by power and control' (Tuerkheimer, 2004: 960-961). As well as not reflecting survivors' lived experiences of abuse as continuous, this violent incident model helps reinforce the narratives of abusive men who tend to describe their abuse in terms of discrete, situationally provoked, time-bound acts (Wangmann, 2020). It has also been suggested the violent incident model can impact the way in which survivors' future choices and experiences are shaped if agencies view historic 'incidents' as irrelevant when considering, for example, child contact arrangements (Barnett, 2017). Criminalising coercive control, characterised by a pattern of

abusive behaviour rather than a discrete incident, could facilitate a more sophisticated and holistic understanding of abuse in intimate relationships.

The introduction of the coercive control law did not meet with universal support. Commentators suggested several potential unintended consequences, including opening a further avenue for 'legal systems abuse' (see Walklate et al., 2018) and the criminalisation of women through counter-allegations, or as a result of seeking to protect themselves and their children by withholding contact from an abusive (ex) partner (Burman and Brooks-Hay, 2018). It has also been suggested that challenges faced by the police and prosecutors in evidencing non-physically abusive behaviour, in particular, may place an even greater reliance on victim testimony (Tolmie, 2018), and that the new offences could increase difficulties as to which charges to lay and prove a distraction from pursuing existing crimes of physical assault and breach of protective orders (Walklate et al., 2018).

Perhaps most significant, however, and underscoring the above concerns, is police and other domestic abuse practitioners' understanding of a concept that 'blurs the line between criminal and non-criminal behaviour' (Tolmie, 2018: 56). Stark (2007) suggested coercive control is perpetrated primarily by men against women as many of the abusive behaviours operate through the enactment of sexual inequality and gender norms. Burman and Brooks-Hay (2018: 74) suggest '[t]he gendered nature of [coercive control] makes these behaviours difficult to recognize … since they coalesce with normalized expectations of male and female behaviour'. And as Tolmie (2018: 56) questions, '[i]f abusive behaviour exploits existing gender norms when does 'normal' end and 'abuse' begin?'

Indeed, there is evidence to suggest this change in the legal archetype of domestic abuse has presented several challenges to policing in England and Wales. Firstly, the offence can feature little or no physical evidence, making it more difficult to investigate and evidence. Secondly, a feature of this crime is manipulation of the victim, which can involve normalisation of the abuse to the extent that a victim is uncertain that a crime is being committed (Bishop and Bettinson 2018; Pitman 2016; Williamson 2010). Thirdly, recent research has identified difficulties in operationalising the concept of coercive control by practitioners and policy makers, officers not situating 'incidents' of domestic abuse in a wider context and a naïve emphasis on physical violence with injury (Barlow et al. 2020; Brennan et al. 2018; Robinson et al. 2018; Stark and Hester 2019). These issues – combining practical and conceptual challenges – compounded existing difficulties in the prevention, investigation and prosecution of pre-defined types of domestic abuse and low levels of victim satisfaction that have been highlighted repeatedly by Her Majesty's Inspectorate of Constabulary (HIMC, 2014; HMICFRS, 2019).

Police training for domestic abuse

When new crimes and accompanying police powers are introduced through legislation, police officers must understand their new powers and learn when to use them. A common method to facilitate this is through in-person or online training. Researchers and practitioners have over fifty years of experience in providing specialist training to police officers about domestic abuse (Bard and Berkowitz 1967). Training still, necessarily, places an emphasis on revealing and challenging negative attitudes about domestic abuse incidents and misapprehensions about victim and offender behaviours and motivations. It remains to be seen if this approach has achieved any meaningful success as outcomes for

victims of domestic abuse through the dominant process of criminalisation have remained poor (Office for National Statistics 2019; Buzawa and Buzawa 2017).

Nonetheless, and despite reservations about the efficacy of criminalising coercive control (Walklate and Fitz-Gibbon 2018; Burman and Brooks-Hay 2018), researchers (Barlow and Walklate, 2020; Bishop and Bettinson, 2018; Eigenberg et al. 2012) and policing bodies (Houtsonen, 2019) are united in their belief that more training for police officers about coercive control and domestic abuse is necessary and desirable. However, many are pessimistic about the ability of training to create meaningful change in how police respond to domestic abuse. Pointing to a general failure of training in Australia (cf. Mason et al. 2017) to improve police recognition of and response to hate crime, Walklate et al (2018) suggest that procedure-focused training will be ineffective in improving police recognition of and response to coercive control. Although most specialist domestic abuse training goes beyond the procedural, Houtsonen (2019) has shown that entry-level training for police officers across Europe does little to educate trainees about the complexities of gender relations that give rise to offences like coercive control.

Notably, researchers are also united in their belief that training individual officers will achieve little in the absence of organisational and structural change. Barlow and Walklate (2020) suggested that coercive control may be too complicated for frontline officers to understand and evidence, while Waddington (2012) claimed that training alone will not change attitudes about the seriousness of domestic abuse: this position was well-illustrated by Hoyle's (1998) study of officer decision-making in domestic abuse cases. Accordingly, researchers have argued that individual-level training is important in breaking individual and

group-level inertia (Houtsonen 2020), but it must be supplemented by critical reflection on policies by organisational leaders (Klein 2008). In the case of coercive control, understanding procedures and new powers must be supplemented by an individual level understanding of the wider context of partner abuse and organisational response to the systemic biases and secondary harms that their policies on domestic abuse may create.

Training description

A 2014 report by Her Majesty's Inspectorate of Constabulary concluded that the police response to victims was 'not good enough' and that 'officers lacked the skills and knowledge necessary to engage confidently and competently with victims of domestic abuse' (HMIC, 2014: 7). The report was also critical of the over-reliance on e-learning and identified an urgent need to overhaul domestic abuse training. The report recommended that the College of Policing conduct a fundamental review of training and development to ensure the training reflects the fact that tackling domestic abuse is core policing business, and that 'all relevant officers and staff should be trained to understand the dynamics of different types of domestic abuse, particularly coercive control' (HMIC, 2014: 23). Of significance, the report recommended that the training should be evidence-based, aimed at tackling the specific problems of lack of knowledge and poor attitudes to domestic abuse, and that the training should be delivered face-to-face rather than provided through e-learning. In response to this report and a subsequent review of police domestic abuse training (Morgan, 2015), the College of Policing, in collaboration with the charity SafeLives and with input from Women's Aid, developed the Domestic Abuse (DA) Matters programme. The programme aims to improve the police response to domestic abuse, including the investigation of coercive control offences and to achieve national consistency in the service

police forces provide to people experiencing domestic abuse. The programme seeks to achieve this by increasing the knowledge and understanding of officers, raising awareness of the varied forms of domestic abuse – physical and non-physical – and providing strategies and skills that police officers and staff can use to improve outcomes for victims. The programme includes an assessment of organisational policies relevant to domestic abuse (a 'health check'), a one-day training event, enhanced training for a small number of selected 'champions' and follow-up support provided through an online forum and continuous professional development training events. The most substantial component of the programme and the focus of this study is the one-day training event, which is designed for police officers for whom attending or dealing with domestic abuse is part of their daily role, primarily 'first responders'. However, the training is open to all police officers and staff. Taking a critical mass approach, the programme seeks to train a minimum 75% of first responders in a police force, but in many forces, the vast majority of staff, which could be in excess of 2,500 individuals, attend, regardless of their roles. Training such a large proportion of staff is expensive, disruptive and, consequently, unusual in policing.

The one-day training aims to provide several learning outcomes for first responders including identifying coercive control and understanding its impact on victims. Key elements of the training to support the learning outcomes include: understanding coercive and controlling behaviour, abuser tactics in controlling victims and manipulating police, understanding victim decision-making and behaviour, strategies for interviewing victims, gathering and recording evidence in relation to coercive and controlling behaviour and other forms of domestic abuse, the impact of domestic abuse on families and strategies for victim safeguarding. The training is delivered by a combination of police trainers and

domestic abuse practitioners in a classroom setting of up to 25 participants. It combines single-person delivery, audio-visual material, group discussion and activities and personal reflection. Testimonials from participants recorded by licensed training providers – currently SafeLives, Women's Aid and Welsh Women's Aid – have emphasised the emotionally evocative nature of the training content and audio-visual material, which features a prolonged recording of a domestic abuse incident.

By (i) placing an emphasis on gender and overcoming stereotypical or naïve thinking about victim behaviour alongside (ii) teaching skills to recognise, collect and record evidence about coercive control and (iii) supplementing the individual training with the organisational 'health check', the programme addresses many of the limitation of typical police domestic abuse training.

Following the enactment of Section 76, forces, including those that did not adopt DA Matters training, had access to an eight-minute e-learning module provided through the College of Policing learning portal that comprised video recorded testimonies with two survivors of coercive control interspersed with factual information about the legislation and advice from senior investigators and prosecutors. While the College encouraged forces to mandate this e-learning to officers, it is not possible to know how many viewed it. As with similar mandated e-learning in police forces, it is likely that the take-up was limited. Anecdotally, some forces have devised and delivered their own training activities on coercive control and/or domestic abuse more widely, but these have not been evaluated and/or small in scale and limited in scope. The 'treatment as usual' condition, then, is an

inconsistent mix of unevaluated/self-devised training, brief e-learning, or, in many instances, no learning at all.

We hypothesised that undergoing Domestic Abuse Matters training would result in more use of the new powers relating to coercive control. Accordingly, we used number of arrests for controlling or coercive behaviour as our outcome.

Methods

Sample

The study unit was a police force. Inclusion criteria for the study was that the unit was one of the 43 territorial police forces in England and Wales. Treatment status was indicated by a force undertaking DA Matters force-wide in-person training. The treatment start date was the month in which the force-wide training began. The time frame for the study was January 2016 to September 2019 (inclusive). The legislation that gave police powers of arrest for the offence of coercive or controlling behaviour came into force in late December 2015 and January 2016 was the first full month of this new police power.

Fourteen police forces received the DA Matters training between January 2016 and September 2019. One force, Hertfordshire Constabulary, received the training in 2015, before the introduction of the legislation. As this was outside of the study timeframe, thus preventing the estimation of a change from pre- to post-training status while also preventing the force being a suitable control force, it was excluded from the study. Three forces – South Wales Police, Dorset Constabulary and Devon and Cornwall Constabulary –

received force-wide, face-to-face training that was closely related to the Domestic Abuse Matters package but did not include the additional components of a force 'health check', local trainers, Domestic Abuse Champions or ongoing online support. As this reduced package retained the key element of force-wide training, they were included in the treatment group, resulting in an eligible sample of seventeen in the trained group.

Data

The study outcome was monthly count of arrests for controlling or coercive behaviour which is lawful under Section 76 of the Serious Crime Act (2015). Arrests under this legislation are identified in police records by a unique Home Office code, which ensured consistency in the recording and extraction of data.

The outcome data were collected under Freedom of Information Act (2000) requests to all 43 police forces in England and Wales. All forces received a standardised request to provide monthly counts of arrests from January 2016 to September 2019, inclusive. Thirty-nine forces (91%) responded to the request and 34 (79%) were able to provide suitable data. In the case of the five forces that responded but were unable to provide suitable data, all five indicated that data system issues prevented the extraction of the information. No force explicitly refused to provide data, but one – Merseyside – only provided censored data for months in which there were fewer than three but more than none arrests (i.e. the count was one or two). In these 17 censored months we imputed a random count of '1' or '2' to permit the data set to be used.

The arrest data were collated into a single file describing number of arrests in 34 forces over 45 months. In addition, to permit our analysis to control for force-level differences in the availability of police officers to make the arrests and in the population available to be arrested, information on number of officers[1] and police force area population[2] were collated from Office for National Statistics open access data. Data on police officer numbers and force area populations were only available at six-month intervals, so these numbers were fixed in the data set for six-month periods. Treatment status and the start month of training for any trained forces in the sample were obtained from the training delivery organisations, SafeLives and Women's Aid. Following the exclusion of Hertfordshire, the final sample included 14 forces that were treated at some point in our observation window and 19 forces that were never not treated.

Study design and analytic strategy

Treatment effects estimation

The effect of the training on trained forces was estimated using a two-way fixed effects 'difference-in-differences' model. The staggered implementation (over time and across police forces) of the training was exploited for identification purposes: if police forces that underwent training earlier experienced an increase in the outcome compared to police forces that had not experienced training (either because they did not take part in the

[1] [1] Police workforce England and Wales statistics: https://www.gov.uk/government/collections/police-workforce-england-and-wales

[2] Crime in England and Wales: Police Force Area data tables: https://www.ons.gov.uk/peoplepopulationandcommunity/crimeandjustice/datasets/policeforceareadatatables

training, or because they have not been trained yet), time and police force variation would make it possible to isolate the effect of the training from other confounding factors.

Several potential threats to internal validity arise when estimating the difference-in-differences model. The key identifying assumption is that, in the absence of treatment, the difference in outcomes between treatment and comparison groups is constant over time (parallel trend assumption). Accordingly, a disadvantage of this identification strategy is that any police-force shock that coincided with the start of the training would bias the estimates. More generally, the plausibility of the parallel trend assumption rests on the choice of a comparable control group. We addressed these concerns in a battery of alternative specifications (specifically, the reduced panel analysis and in the event study), that lend credibility to our identification strategy and robustness to our findings.

The outcome was a count variable and variance of the outcome greatly exceeded the mean, indicating the data were over-dispersed. Consequently, a negative binomial link function was employed to model the outcome data. To adjust for force-level homogeneity and monthly autocorrelation, police force and month were included in the model along with monthly numbers of police officers per force and police force area population.

The analysis equation was:

$$\log(Y_{it}) = \alpha + \beta_1 Trained_{it} + \delta_{it}'.\beta_2 + \gamma_i + \omega_t$$

where Y_{it} was the number of arrests per force *i* per month *t*; α was an intercept. $Trained_{it}$ was a dummy variable that was 0 for forces that had not been trained in month *t* and 1 for

forces that had been trained. Covariates, police force area population (multiples of 100,000) and number of police officers, were represented by δ'_{it}, γ_i and ω_t were fixed effects for police force and month to account for differences between police forces and to account for monthly patterns in arrests within forces, respectively. The primary analysis was run with the full set of 33 trained and untrained forces. To estimate the effect of the full Domestic Abuse Matters training, without the three forces who underwent the reduced (force-wide one-day training only) programme, a second model with all the control forces and the eleven forces that completed the full programme was estimated.

Leave-one-out analysis

In order to rule out excessive influence on estimates caused by extreme change in arrest in a single force, the difference-in-difference estimation model was run 33 times with each iteration excluding one of the forces.

Reduced panel analysis

It is plausible that the forces that did not undertake the training are unsuitable as control units because of, for example, unobserved differences in their approaches to domestic abuse and coercive control, which could have affected their motivation to undertake the training. Therefore, capitalising on the different timings of training across the trained forces, the difference-in-difference analysis was re-run using only the treated forces and the results compared to the full model.

Event study

Finally, an event study was undertaken. This method capitalises on different timing of interventions to model the impact of training at common points in time: for this study, dummy variables were created to represent each of the twelve months before and the twelve months from the initiation of training at t0. Outcomes were modelled using negative binomial regression and including these dummy variables (t-1 month was excluded to act as a reference month). We also controlled for seasonality and force-level fixed effects. The resulting incident rate ratios were plotted to illustrate the trend in arrests before and after the training relative to one month before the training began.

The equation for the event study was:

$$\log(Y_{it}) = \alpha + \sum_{s-12}^{11} \beta^s I[D_{it}^s = 1] + \delta'_{it}\phi + \gamma_i + \omega_t$$

In the equation, D^s is dummy variables that indicate police force *i* being *s* periods away from initial treatment at calendar time *t*. Covariates, police force area population (multiples of 100,000) and number of police officers, were represented by δ'_{it}, and γ_i and ω_t were fixed effects for police force and calendar month.

Pre-registration

The study was pre-registered on Open Science Framework on 9th September 2019 https://osf.io/94wdk) and a revised registration was posted and time-stamped on 30th

September 2019 (https://osf.io/j4cr8), the final day of the data collection timeframe. Our study features mild deviations from the pre-registration, which are discussed below.

Reproducibility

The data were stored in .csv files and analysed using R (version 4.0; R Core Team 2020) and RStudio 1.3.959 (R Studio Team 2020). R packages used to clean, analyse and visualise the data were 'tidyverse' (Wickham et al. 2019), 'ggplot2' and 'MASS' (Venables & Ripley 2002).

To facilitate reproducibility of the study, our pre-registration documents, the raw Freedom of Information responses, the workforce, population and training timing and treatment status data sets, and the statistical code for all analyses and illustrations can be found here: (https://osf.io/vx789/)

Results

Descriptive statistics

The average number of arrests per month per police force was 8.87 (standard deviation (SD) 11.59) and the range was 0 to 77. The median number of arrests was 5 with an interquartile range (IQR) of 1 to 11. The mean rate of arrests per 100,000 population was 0.7 (SD 0.86) and the range was 0 to 6.53. The median rate of arrests per 100,000 population was 0.39 (IQR 0.14–0.92). The mean police force area populations ranged from 497,900 to 8,899,400 (mean 1,427,553; SD 1,410,263) and the number of police officers ranged from 801 to 32,125 (mean 3,042; SD 5,036; median 1,870; IQR 1,214–2,921).

As illustrated in Figure 1, regardless of training status, there was a steady increase in number of arrests for controlling or coercive behaviour over the 45 months of the study with number of arrests for this new offence approximately doubling each year. However, there was large variation in the patterns of arrest between police forces, as illustrated by the wide and overlapping interquartile ranges. The study design, having multiple time periods, does not lend itself to easy illustration of treatment effects, although this is partially addressed in Figure 2, which plots the event study estimates.

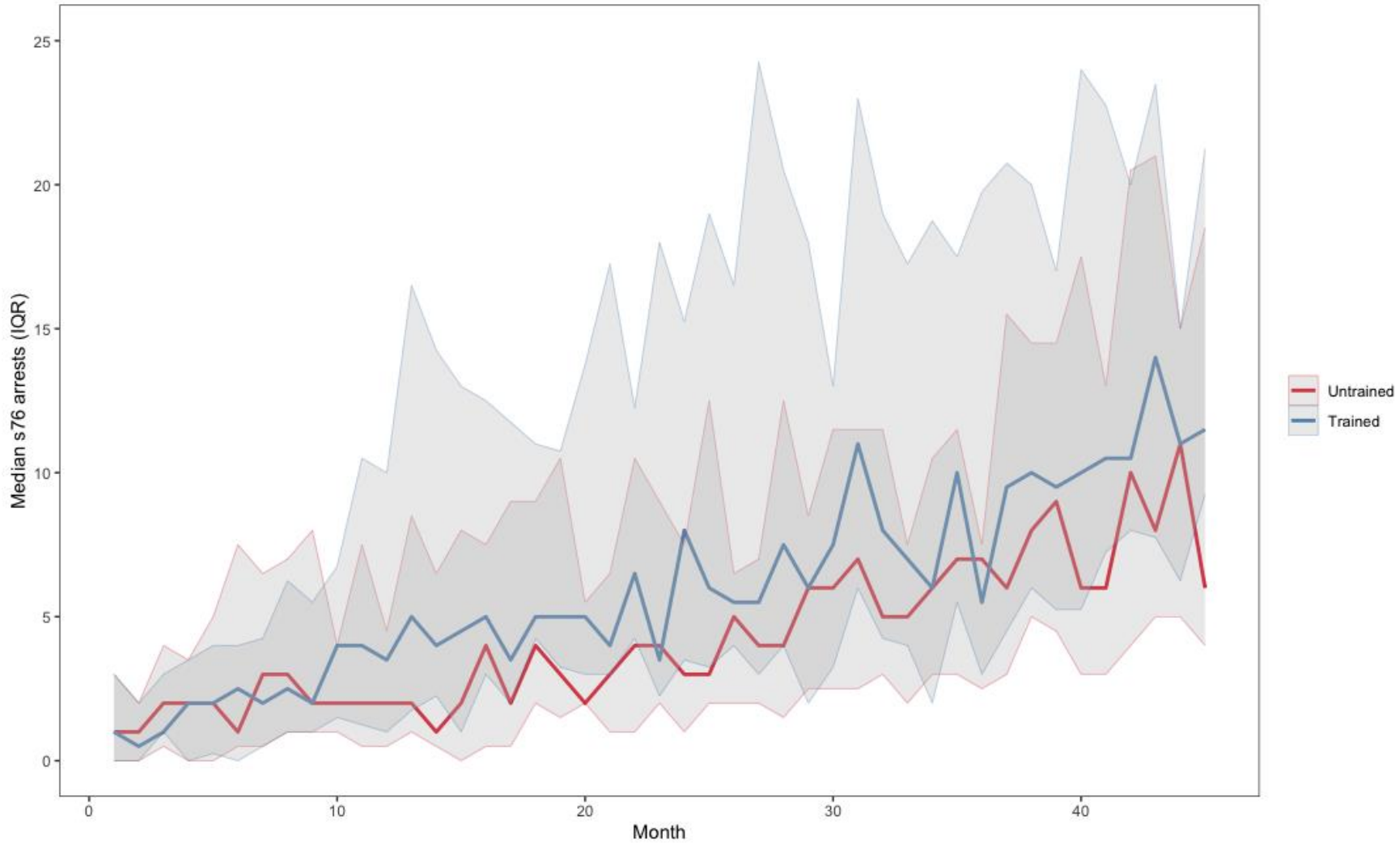

Figure 1. Trends in median number of arrests for controlling or coercive behaviour across trained and untrained police forces

Treatment effects on the treated forces

The difference-in-differences model (Table 1) demonstrates that the training was associated with a statistically significant increase in arrests for controlling or coercive behaviour. The incident rate ratio (exponential of beta coefficient) was 1.413 (95% CI 1.235–1.617) when all trained forces were included and 1.401 (95% CI 1.212–1.621) when the treated sample was restricted to those forces that underwent the full Domestic Abuse Matters training. In both cases, this was a statistically significant effect. In absolute terms, the training was associated with an average of 3.31 additional arrests per force per month[3].

As noted above, treatment effects were also estimated using a variety of checks on the robustness of the main findings. To rule out excessive influence of any one force, a leave-one-out analysis was also undertaken. The range of IRRs from this series of analyses was 1.302–1.556 (Table 2). In all models, training was associated with statistically significant effects, which are described in Table 3. A reduced panel analysis found a higher treatment effect when the sample was limited just to those forces that were trained (IRR 1.610, 95% CI 1.397–1.856; see Table 3).

Event study

Figure 2 illustrates the incident rate ratio of each time point from twelve months before initiation of training to eleven months after initiation of training relative to one month before training. In doing so, it describes the change in relative rate of arrest in the lead up to and time following training. The plot indicates that, compared to other forces that had not

[3] The mean number of arrests in the untreated group was 8.287, multiplied by 0.4, equals 3.3148

yet been trained, forces had comparatively low, but not statistically significantly different numbers of arrest up to around six months before training. That relative rate increased in the months before training. In the eight months following the initiation of training, relative rates of arrests were, in general, statistically significantly higher than yet-to-be trained forces. After approximately eight months, the relative rate of arrests began to decrease.

	Any whole-force one-day training				Full Domestic Abuse Matters package only			
	Estimate	**SE**	**IRR**	**95% CI**	**Estimate**	**SE**	**IRR**	**95% CI**
(Intercept)	-2.447***	0.587	1.914	0.186–19.620	1.233***	1.243	3.43	0.297–39.381
Treatment	0.358***	0.042	1.413	1.235–1.617	0.338***	0.075	1.401	1.212–1.621
Population	0.145***	0.032	0.977	0.859–1.111	-0.060	0.069	0.941	0.822–1.078
Work force	0.001***	0.001	1.000	0.999–1.000	0.001	0.001	1.000	0.999–1.000
Force[a]	.	.	.	.	.	.	.	.
Month[a]	.	.	.	.	.	.	.	.
N	1,485				1,350			

Table 1. Full sample model

SE: Standard error; IRR: Incident rate ratio; CI: Confidence interval; ***p<0.001

[a]Force and month were included in these models, but their coefficients have been excluded from this table for parsimony. The full model is in Appendix 1.

Excluded force	Estimate	SE	Z	IRR	95% CI	Condition
Avon & Somerset Police	0.349***	0.07	4.944	1.417	1.237–1.623	Not trained
Bedfordshire Police	0.353***	0.071	4.974	1.424	1.242–1.633	Trained
Cheshire Constabulary	0.352***	0.071	4.981	1.422	1.241–1.630	Trained
Cleveland Police	0.366***	0.07	5.269	1.443	1.262–1.650	Not trained
Cumbria Constabulary	0.290***	0.069	4.185	1.336	1.169–1.527	Not trained
Derbyshire Constabulary	0.385***	0.07	5.53	1.47	1.286–1.681	Not trained
Devon & Cornwall Police	0.345***	0.071	4.854	1.412	1.231–1.619	Trained
Dorset Police	0.373***	0.071	5.224	1.452	1.265–1.668	Trained
Durham Constabulary	0.360***	0.07	5.123	1.433	1.252–1.641	Not trained
Dyfed-Powys Police	0.294***	0.07	4.216	1.342	1.173–1.536	Trained
Essex Police	0.317***	0.07	4.525	1.372	1.200–1.570	Not trained
Gwent Police	0.352***	0.07	5.053	1.422	1.244–1.627	Not trained
Hampshire Constabulary	0.442***	0.075	5.892	1.555	1.346–1.798	Trained
Kent Police	0.442***	0.063	7.022	1.556	1.378–1.757	Not trained
Leicestershire Constabulary	0.397***	0.072	5.51	1.488	1.295–1.710	Trained
Merseyside Police	0.352***	0.07	5.018	1.422	1.242–1.627	Not trained
Metropolitan Police Service	0.396***	0.07	5.656	1.486	1.299–1.701	Not trained
Norfolk Constabulary	0.345***	0.071	4.881	1.412	1.232–1.617	Not trained
North Wales Police	0.341***	0.07	4.885	1.407	1.230–1.610	Not trained
North Yorkshire Police	0.321***	0.071	4.529	1.379	1.203–1.581	Not trained
Northamptonshire Police	0.331***	0.073	4.531	1.392	1.209–1.604	Trained
Northumbria Police	0.346***	0.071	4.912	1.414	1.235–1.620	Not trained
South Wales Police	0.311***	0.072	4.34	1.365	1.189–1.567	Trained
Staffordshire Police	0.316***	0.07	4.522	1.372	1.199–1.570	Not trained
Suffolk Constabulary	0.331***	0.072	4.579	1.392	1.211–1.600	Trained
Surrey Police	0.264***	0.071	3.72	1.302	1.136–1.492	Trained
Sussex Police	0.314***	0.078	4.041	1.369	1.179–1.590	Trained
Thames Valley Police	0.328***	0.071	4.619	1.388	1.211–1.592	Not trained
Warwickshire Police	0.329***	0.071	4.596	1.389	1.210–1.595	Trained
West Mercia Police	0.370***	0.074	5.032	1.448	1.258–1.667	Trained
West Midlands Police	0.348***	0.07	4.956	1.416	1.237–1.622	Not trained
West Yorkshire Police	0.341***	0.072	4.764	1.407	1.226–1.615	Not trained
Wiltshire Police	0.351***	0.07	5.025	1.421	1.242–1.626	Not trained

Table 2. Leave-one-out analysis

SE: Standard error; IRR: Incident rate ratio; CI: Confidence interval; ***p<0.001

	Estimate	SE	IRR	95% CI
(Intercept)	-4.176***	0.939	0.015	0.002–0.096
Training	0.476***	0.074	1.610	1.397–1.856
Population (100,000s)	0.280*	0.138	1.324	1.013–1.74
Workforce	0.002**	0.001	1.002	1.001–1.003
Force[a]	.	.	.	.
Month[a]	.	.	.	.
N	630			

Table 3. Reduced panel analysis

[a]Force and month were included in this model but excluded from the table for parsimony

SE: Standard error; IRR: Incident rate ratio; CI: Confidence interval; *p<0.05; **p<0.01; ***p<0.001

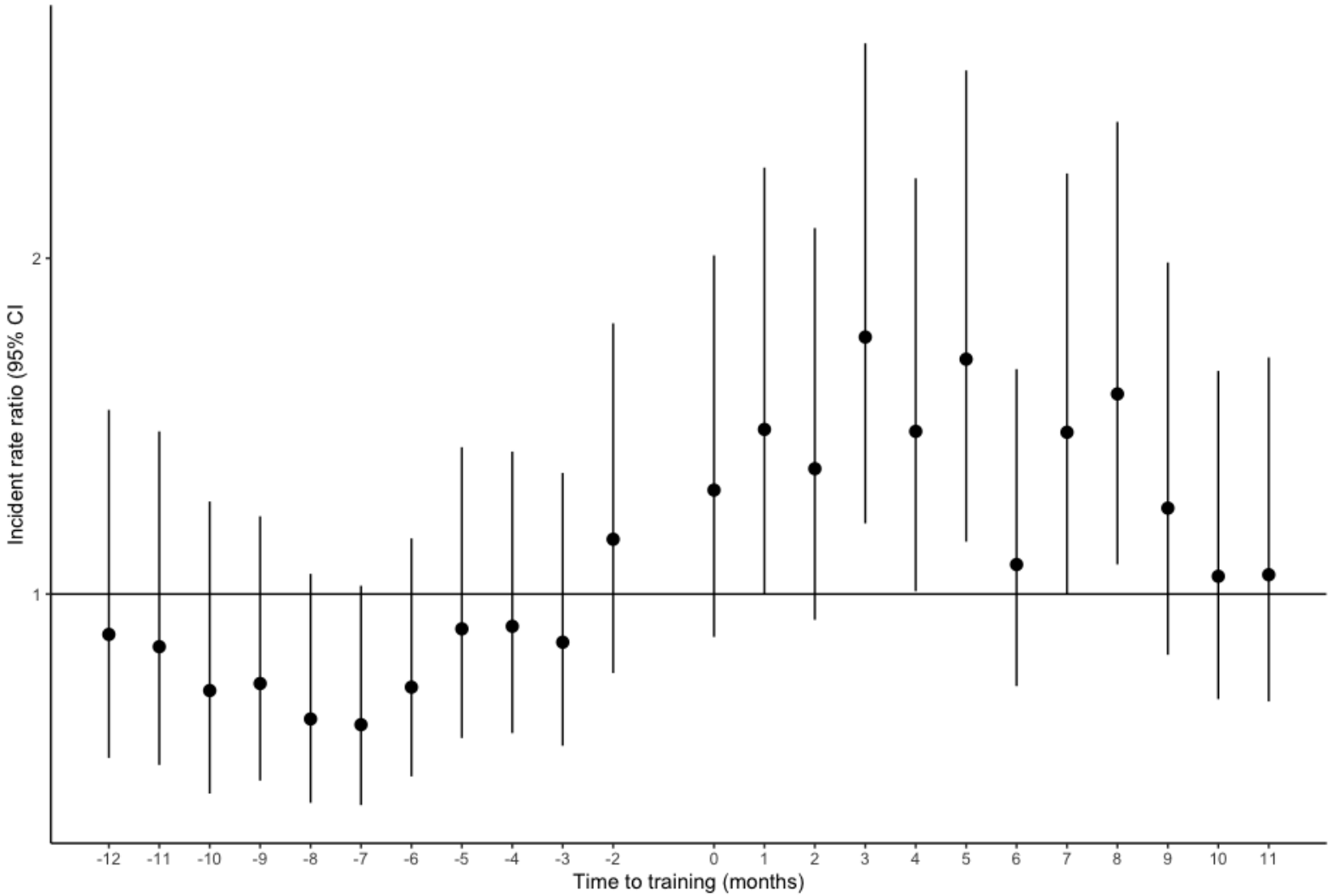

Figure 2. Event study plot (reference category: one month before training)

Discussion

As hypothesised in our study pre-registration, force-wide in-person training relating to coercive control was associated with an increase in the rate of arrest for the newly legislated crime of controlling or coercive behaviour. More specifically, training was followed by a 41% increase in arrests: in absolute terms, this was an average of three additional arrests per force per month. Several robustness checks supported the effects observed in the main analyses: no single force had a substantial impact on the robustness of the findings; the effect was consistent when the sample was limited to forces that had the training; and the timing of the relative increase in arrests was consistent with initiation of the training.

Though the study design cannot rule out entirely alternative explanations for the observed effect on arrests (see below), the findings are convincing that the programme increased arrests for coercive control. Restricting the analysis to those forces that undertook the full programme did not change the overall effect of the training, suggesting that the one-day training was the most potent feature of the programme.

The event study demonstrated that initiation of the training was associated with an increased relative rate of arrest for approximately eight months after the training. After this time, relative rates of arrest began to decline, although numbers of arrests continued to rise. This pattern – a statistically significant increase that was not sustained over the longer-term – suggests that the training had meaningful but short-term effects. This finding is consistent with the scepticism that some researchers have expressed over the potential for

domestic abuse training to deliver long-lasting change in police practice (Barlow and Walklate, 2018) and indicates that future iterations must focus on sustaining the immediate impacts of the training on knowledge and skills. An alternative possibility is that improvements in knowledge and skills realised by the training have endured, while motivation to pursue the offence specifically has declined. Commentators (Tolmie, 2018; Wangmann, 2020) have suggested improvements in policing need to be seen also in other criminal justice agencies. If officers perceive there is a low prospect of achieving a charge and conviction for controlling or coercive behaviour, they may quickly lose their initial enthusiasm and revert to pursuing offences with which they are more familiar.

In addition to the statistically significant post-initiation effect, it is noteworthy that the general pattern of relative rates of arrest was that forces had lower rates of arrest six months before the training but that rates steadily increased in the lead-in to the training, sometimes described as an 'anticipation effect'. One possible explanation for this observation is that it reflects an immediate effect of the initiation of force 'health checks' in the months before the first responder training. As noted earlier, 'health checks' involve an organisation-level examination of practice and policy in relation to domestic abuse. An alternative explanation for the increase in the time before the training is that it is regression to the mean following comparatively low relative rates of arrest. It is important to note that we did not hypothesise this 'anticipation effect' and it is not statistically significant, so we urge caution in its interpretation.

Arrests for controlling or coercive behaviour increased steadily over time across the whole sample: numbers of arrest approximately doubled each year across the 34 forces that

returned freedom of information requests. To our knowledge, this is the first research to systematically count and compare numbers of arrests for coercive control across police forces. As controlling or coercive behaviour was a new crime during the study period, it is likely that the observed increase in numbers of arrests reflected growing awareness of this crime among police officers and victims rather than its true incidence. The generally increasing trend, coupled with the observations from the event study, suggests that forces that did not undertake the training will achieve similar rates of arrest to those that did undergo the training, albeit more slowly. In terms of the overall criminalisation of coercive control, this continued steady trend indicates that police may be gaining enthusiasm for their new powers and that they appear competent to identify and investigate coercive control, in some cases. However, the apparent success of the training should be interpreted in the wider context of domestic abuse crimes (cf. Walby and Towers, 2018). Absolute numbers of arrests, even in DA Matters forces, are still comparatively low, lending support to research that has suggested the coercive control legislation is underutilised (Barlow et al., 2020). Research suggests that coercive control is far more prevalent than official statistics and administrative data reveal: Stark (2012) suggests coercive control is the form of oppression that drives 'most' women to seek outside assistance and Myhill and Hohl (2019) found in an analysis of primary risk assessments in one English force that coercive control was the dominant profile of abuse disclosed. Examination of data on domestic abuse crimes shows that only 2.3%[4] of the recorded domestic abuse crimes in the 34 forces included in the study were recorded as controlling or coercive behaviour under Section 76 in 2018/19. This total appears to represent a small fraction of cases that are likely to contain ongoing

[4] 13,809 of 605,947 crimes (Office for National Statistics, 2019)

coercive control (see also Barlow et al., 2020). This finding supports the view that realising the full potential of the legislation likely requires wider organisational change on top of upskilling individual officers (Walklate et al., 2018).

Controlling or coercive behaviour legislation was introduced to give the police and courts the legal powers to respond to this harmful behaviour. The DA Matters training had several aims and increasing competence in recognising and investigating coercive control was just one. Arrest was selected as a measure for the evaluation as it was a proxy for familiarity with and willingness to use this law in favour of other less appropriate reasons for arrest or not arresting at all. However, arrest should not necessarily be taken as the best possible outcome of police training. Future research could perhaps consider the impact of the training on officers' primary assessments of risk, the quality of their incident and crime reports, and the primary safeguarding measures they take, including referrals to relevant support services.

In employing use of a new law as an indicator of training effectiveness, this paper avoids the question of whether the training provided better outcomes for victims and does not include any information on the proportion of arrests that led to charge or summons. Nor does it examine if enthusiasm for coercive control has affected rates of arrest or prosecution for domestic abuse more generally. There is much controversy over the extent to which criminalising coercive control will benefit victims (see for example Walklate et al., 2018), the desirability of a criminal justice-heavy response to domestic abuse more generally (Goodmark, 2018), and the role of arrest specifically (Barlow and Walklate, 2020). Potential unintended consequences notwithstanding, however, the coercive control legislation does

provide the opportunity to address a key historical problem with prosecuting domestic abuse: it offers a meaningful punishment for a pattern of abusive behaviour as opposed to the previous amnesiac system of trivial outcomes for discrete incidents. The findings presented here offer some encouragement that officers' use of discretion may be enhanced in order that they employ arrest in cases where it is most often justified (ongoing violence and/or coercion and control) and not in cases where it might be considered disproportionate, or even counter-productive.

The study was pre-registered and time-stamped on the Open Science Framework platform. It is important to note where the study was faithful to and where it deviated from the protocol. It is also noteworthy that pre-registrations on the OSF platform are not peer-reviewed at the point of registration or at the point of study manuscript completion. In that sense, this method of pre-registrations lacks the oversight of a registered report. At the time of submission, only one journal that accepts articles directly relevant to these topics of policing and domestic abuse (the generalist journal PLoS One) has a registered report facility and no such facility was available at the time of our pre-registration in September 2019.

The initial pre-registration, submitted on 9th September, described the overall aims of the projects, study hypothesis, proposed study design and analytic plan. After 9th September, we learned that the Home Office, our named data provider, would be unable to provide arrest data for each police force as they do not collate this information on a monthly level. Consequently, we revised the registration (time-stamped 30th September) to use number of charges and summons per force per month as the study outcome. This measure was not our preferred indicator of police force activity as decisions to charge and summons are made, in

England and Wales, in concert with the Crown Prosecution Service and are subject to a range of organisational and external influences that contaminate its validity as an indicator of police practice (Hester 2006; Porter 2019). Following reflection, we decided that to collect arrest data from individual police forces via Freedom of Information requests would be viable and preferable. Although the use of this measure deviates from the final pre-registration, it is the most suitable outcome and is consistent with our original intent.

Our statistical plan stated that we would use all statistical points available. However, data for one force (Hertfordshire) was excluded on the grounds that the timing of its treatment (before the study timeframe) made it impossible to use as a treated or control force. The pre-registration was deliberately written before the dates of force training had been collected, which made this necessary exclusion unforeseeable. The Hertfordshire data is included in the raw data files, but the force was excluded from all illustrations and analyses. In addition, we were unaware of the reduced programme that was delivered in three forces and we added an unplanned supplementary analysis that estimated the effect of the full programme only.

Finally, our analysis methods are more sophisticated than stated in the pre-registration. A team member with expertise in econometrics was added after the study data had been collected but before treatment effect estimation had been conducted. On the advice of this team member, an event study was added to the analytic strategy and, following a reading of Goh (2020), we added additional robustness checks. We also stated that we would compare the suitability of negative binomial and Poisson models to estimate the treatment effects. As the outcome measure is highly over-dispersed, which we could not have known prior to

data collection as no arrest data had ever been collated on this new offence, we did not undertake this comparison and simply selected the correct generalised linear model, negative binomial regression.

The study has several limitations that should be acknowledged. Firstly, the way in which arrest is used may vary across police forces, which could explain between-force differences, although assuming these differences are constant, they are managed by including forces as fixed effects. Furthermore, proportion of domestic abuse incidents that result in arrest is used as a performance measure by the police inspectorate for England and Wales (HMICFRS 2019), which suggests that its variability is not severe. The use of Freedom of Information Requests to obtain measures of police performance is becoming common (Bows, 2017), but it is still subject to response bias. For example, the introduction of new data systems in Humberside Police, Lancashire Constabulary and South Yorkshire Police prevented their providing data in response to the Freedom of Information request and three forces (City of London, Gloucestershire Police and Lincolnshire Police) did not respond to the requests at all. It is possible that data issues or non-response to these requests may be correlated with performance relating to the policing of domestic abuse. The study is also vulnerable to selection bias. The forces self-selected into a treatment condition by paying for and hosting the training. Therefore, forces with larger training budgets or those who placed a higher priority on domestic abuse – due to intrinsic or extrinsic factors – may have been more motivated to undertake the training and better placed to increase arrest for coercive or controlling behaviour. In addition, by controlling for fixed effects in police forces and time, we adjusted for potential confounding factors that either vary at the police force level, but not in time, or vary in time, but in a way that is common across police forces.

It is difficult to attribute the observed effects exclusively to the training events. It is possible that organisation-wide training, the costs of the training and disruption caused by all staff members undertaking a full day of training may have focused force attention on the issue of coercive or controlling behaviour. In turn, this could have led to unmeasured organisational changes that affected number of arrests. Indeed, the DA Matters package includes components of force-wide policy review and organisational follow-up support that seeks to complement their individual-level training. It may never be possible to separate and estimate the relative contributions of these aspects of DA Matters, but the comparison of models based on forces that received the full DA Matters package and the forces that received just the force-wide training (Table 1), suggests that the majority of change was attributable to the training.

Conclusion

A difference-in-difference analysis demonstrated that force-wide training to raise awareness of issues in the policing of domestic abuse and of the new crime of coercive or controlling behaviour was associated with a statistically significant 41% increase in arrests for coercive or controlling behaviour. In absolute terms, this effect was an average addition of 3.3 arrests per force per month. The relative effect of the training was sustained for approximately eight months. While arrest may not be the only or even most desirable long-term outcome of training to improve outcomes for victims, training a large majority of police force members to understand the context as well as the procedure surrounding coercive control was effective in increasing use of this new police power.

Acknowledgements

The origins of this study were in a project funded by Hampshire Constabulary to understand the impact of their Domestic Abuse Matters training. We are grateful to SafeLives and Women's Aid for providing information on the timing of interventions, for providing contextual information that informed our understanding of the training and for respecting the need for our evaluation to remain independent. We are grateful to Open Science Framework for providing and maintaining a platform to pre-register research and to host study materials, which are available for re-use under a CC-BY 4.0 license https://osf.io/vx789/.

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Appendix 1 – 'Any whole-force face-to-face training' difference-in-differences model with all estimates

	Any face-to-face training		
	Estimate	**Standard error**	***z***
(Intercept)	-2.447***	0.587	-4.172
Treatment	0.358***	0.042	8.419
Population (1000s)	0.145***	0.032	4.472
Workforce	0.0002401***	0.000	5.579
Bedfordshire	0.7804*	0.374	2.086
Cheshire	-0.308	0.243	-1.269
Cleveland	0.6277	0.415	1.511
Cumbria	2.077***	0.438	4.747
Derbyshire	0.9631***	0.240	4.019
Devon & Cornwall	-0.554***	0.100	-5.516
Dorset	1.498***	0.334	4.483
Durham	1.236**	0.379	3.265
Dyfed-Powys	0.4324	0.418	1.035
Essex	0.3732***	0.091	4.083
Gwent	0.545	0.417	1.307
Hampshire	0.4673***	0.120	3.909
Kent	-0.09216	0.105	-0.881
Leicestershire	0.02673	0.244	0.110
Merseyside	-0.5319***	0.126	-4.236
Metropolitan	-15.57***	2.992	-5.203
Norfolk	2.467***	0.289	8.538
North Wales	-0.521	0.407	-1.279
North Yorkshire	1.695***	0.307	5.528
Northamptonshire	1.696***	0.339	5.006
Northumbria	-0.2741*	0.118	-2.323
South Wales	0.2542.	0.130	1.953
Staffordshire	-0.567*	0.242	-2.347
Suffolk	2.568***	0.334	7.693
Surrey	1.977***	0.194	10.188
Sussex	1.104***	0.074	14.916
Thames Valley	-1.12***	0.270	-4.156
Warwickshire	1.147**	0.416	2.759
West Mercia	0.602***	0.174	3.470
West Midlands	-3.551***	0.493	-7.200
West Yorkshire	0.1959	0.268	0.732
Wiltshire	0.6469.	0.357	1.813
Month 2	-0.05972	0.173	-0.345
Month 3	0.3386*	0.158	2.150

Month 4	0.3312*	0.158	2.103
Month 5	0.3568*	0.156	2.289
Month 6	0.5839***	0.149	3.908
Month 7	0.7069***	0.146	4.836
Month 8	1.012***	0.140	7.232
Month 9	0.8946***	0.142	6.284
Month 10	0.7285***	0.145	5.014
Month 11	0.8966***	0.142	6.324
Month 12	0.8398***	0.143	5.876
Month 13	1.167***	0.137	8.530
Month 14	0.9225***	0.141	6.545
Month 15	1.124***	0.138	8.175
Month 16	0.9672***	0.140	6.902
Month 17	1.085***	0.138	7.854
Month 18	1.271***	0.135	9.388
Month 19	1.275***	0.135	9.419
Month 20	1.012***	0.139	7.273
Month 21	1.154***	0.137	8.415
Month 22	1.371***	0.134	10.213
Month 23	1.425***	0.134	10.672
Month 24	1.41***	0.134	10.539
Month 25	1.492***	0.133	11.219
Month 26	1.245***	0.136	9.156
Month 27	1.322***	0.135	9.784
Month 28	1.462***	0.134	10.957
Month 29	1.308***	0.135	9.674
Month 30	1.46***	0.134	10.933
Month 31	1.608***	0.132	12.184
Month 32	1.534***	0.133	11.559
Month 33	1.532***	0.133	11.542
Month 34	1.595***	0.132	12.065
Month 35	1.583***	0.132	11.960
Month 36	1.557***	0.133	11.746
Month 37	1.649***	0.133	12.445
Month 38	1.708***	0.132	12.941
Month 39	1.695***	0.132	12.835
Month 40	1.669***	0.132	12.619
Month 41	1.723***	0.132	13.064
Month 42	1.789***	0.131	13.619
Month 43	1.842***	0.131	14.063
Month 44	1.799***	0.131	13.700
Month 45	1.838***	0.131	14.032

Appendix 2 – 'Full Domestic Abuse Matters training programme only' difference-in-differences model with all estimates

Estimate	**Estimate**	**Standard error**	**z**
(Intercept)	1.233	1.243	0.992
Treatment	0.338***	0.075	4.497
Population (1000s)	-0.06	0.069	-0.882
Workforce	0.000	0.000	1.33
Bedfordshire	-1.503.	0.781	-1.925
Cheshire	-1.623***	0.475	-3.413
Cleveland	-1.938*	0.857	-2.262
Cumbria	-0.506	0.922	-0.549
Derbyshire	-0.566	0.492	-1.15
Durham	-1.114	0.791	-1.408
Dyfed-Powys	-2.213*	0.86	-2.572
Essex	0.826***	0.167	4.934
Gwent	-1.986*	0.858	-2.315
Hampshire	1.08***	0.238	4.53
Kent	0.047	0.202	0.232
Leicestershire	-1.32**	0.486	-2.715
Merseyside	-0.993***	0.208	-4.78
Metropolitan	2.01	6.557	0.306
Norfolk	0.661	0.614	1.076
North Wales	-2.786***	0.792	-3.518
North Yorkshire	-0.118	0.642	-0.184
Northamptonshire	-0.48	0.713	-0.673
Northumbria	-0.718***	0.196	-3.663
Staffordshire	-1.729***	0.449	-3.846
Suffolk	0.573	0.707	0.81
Surrey	0.833*	0.401	2.074
Sussex	1.048***	0.125	8.403
Thames Valley	0.492	0.569	0.865
Warwickshire	-1.477.	0.872	-1.693
West Mercia	-0.35	0.345	-1.015
West Midlands	-0.703	1.061	-0.662
West Yorkshire	1.707**	0.568	3.006
Wiltshire	-1.501*	0.737	-2.037
Month 2	-0.15	0.227	-0.661
Month 3	0.309	0.212	1.459
Month 4	0.257	0.213	1.204
Month 5	0.292	0.212	1.377
Month 6	0.379.	0.21	1.808
Month 7	0.551**	0.205	2.681

Month 8	0.88***	0.199	4.424
Month 9	0.786***	0.201	3.917
Month 10	0.718***	0.202	3.557
Month 11	0.737***	0.202	3.656
Month 12	0.607**	0.204	2.972
Month 13	1.078***	0.196	5.498
Month 14	0.836***	0.2	4.18
Month 15	0.941***	0.198	4.75
Month 16	0.942***	0.198	4.754
Month 17	0.953***	0.198	4.813
Month 18	1.293***	0.193	6.7
Month 19	1.202***	0.194	6.187
Month 20	0.934***	0.198	4.707
Month 21	1.039***	0.197	5.275
Month 22	1.291***	0.193	6.675
Month 23	1.306***	0.193	6.757
Month 24	1.276***	0.194	6.591
Month 25	1.408***	0.193	7.31
Month 26	1.213***	0.196	6.206
Month 27	1.219***	0.195	6.236
Month 28	1.393***	0.193	7.207
Month 29	1.316***	0.194	6.775
Month 30	1.418***	0.193	7.347
Month 31	1.62***	0.191	8.494
Month 32	1.47***	0.192	7.643
Month 33	1.398***	0.193	7.237
Month 34	1.537***	0.192	8.021
Month 35	1.588***	0.191	8.299
Month 36	1.576***	0.191	8.228
Month 37	1.73***	0.193	8.983
Month 38	1.795***	0.192	9.348
Month 39	1.795***	0.192	9.346
Month 40	1.773***	0.192	9.214
Month 41	1.817***	0.192	9.463
Month 42	1.912***	0.191	9.996
Month 43	1.975***	0.191	10.353
Month 44	1.933***	0.191	10.117
Month 45	1.985***	0.191	10.41