DISCUSSION PAPER SERIES

IZA DP No. 4565

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November 2009

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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Discussion Paper No. 4565
November 2009

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ABSTRACT

Partially Identified Treatment Effects under Imperfect Compliance: The Case of Domestic Violence*

During the 1980s a set of randomized experiments were carried out to determine the usefulness of a mandatory arrest policy for domestic assault offenders. The first of these was the Minneapolis Domestic Violence experiment (MDVE), which was carried out in 1981. This paper re-examines the data from the MDVE and uses the recent literature on partial identification to determine the implications for a mandatory arrest policy for domestic assault offenders today. I find support for a mandatory arrest policy for domestic assault offenders, even under a set of minimal assumptions on offender and police behavior.

JEL Classification: C9, C14, K42

Keywords: partial identification, experiments, illegal behavior, policing

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* I am grateful to Marco Caliendo, Chuck Manski, Joerg Stoye and Alex Tetenov for comments and discussions. All errors are my own.

1 Introduction

Domestic violence has been an important policy concern for US lawmakers, particularly over the last few decades. According to data from the National Crime Victimization surveys, the annual rate of nonfatal intimate partner victimizations was 2.3 per 1000 persons age 12 or older in 2005. In addition, data from the FBI Supplementary Homocide Reports shows that there were 1510 intimate homicides in 2005. Policy makers have also been concerned with the percentage of nonfatal intimate partner victimizations which actually get reported to the police; in 2004-2005 only 62.1% of such victimizations were reported to the police by female victims and 64.3% were reported by male victims. The rates of nonfatal intimate partner victimizations and homicides have actually been consistently declining over the last decade while the percentage of nonfatal intimate partner victimizations which get reported to the police have been on the rise. [1]

Domestic violence was historically an area of policing where officers were reluctant to interfere. This changed in the 1980s due to Thurman v. City of Torrington (1984), a court case in Connecticut which established the right to police protection from domestic violence and the Minneapolis Domestic Violence experiment (MDVE). The MDVE randomly assigned arrest and alternative treatments to a sample of domestic violence offenders, providing evidence that arrest was associated with significantly lower recidivism or repeat incidence of domestic violence. These events contributed to the adoption of mandatory arrest policy for offenders of domestic violence: currently fourteen states and the district of Columbia have mandatory arrest laws and an additional eight states have recommended arrest laws for domestic violence offenders.

The MDVE results were extremely influential in the introduction of a mandatory arrest policy but the results of the MDVE have come under criticism due to concerns regarding internal and external validity (Binder and Meeker[7][8], Lempert[14], and Buzawa and Buzawa[9]), the lack of persistence of the effects of a mandatory arrest policy over time (Tauchen and Witte[30]) and the possible behavioral consequences of the mandatory arrest policy on reporting by victims of domestic violence (Iyenger[13]). In this paper I re-analyze the data from the MDVE to address concerns regarding internal validity (regarding non-compliance with assigned

[1]These statistics are available on the Bureau of Justice Statistics website.

treatments) while Siddique and Tetenov[24] examines the identification issues associated with carrying out a meta-analysis of the replications of the MDVE by the National Institute of Justice. In this paper I also discuss the implications of the experimental data from the MDVE for a mandatory arrest policy today.

The MDVE was influential due to its use of randomized treatment assignment. The usefulness of a random assignment of treatments to determine treatment effects can be traced to the works of Fisher[10] and Neyman[20]. Random assignment ensures that the differences in outcomes across the different treatment groups can be linked to the treatment itself; as a result, treatment effects in an experimental setting are fully identified, and may be estimated trivially. However, a frequent problem with implementation of experiments is that of non-compliance with assigned treatments. Non-compliance with assigned treatment also arose in the MDVE since police officers were allowed to deviate from assigned treatment if there was sufficient cause (for instance if the suspect assaulted the officer or if the victim demanded the suspect be arrested).[2] In the presence of non-compliance there are important counterfactual probabilities (outcome probabilities associated with the assigned treatment among non-compliers) which cannot be observed. There are different strategies that one may use to identify treatment effects when there is non-compliance. For the MDVE data, Berk and Sherman[6] make distributional and functional form assumptions to model non-compliance, incorporating this model into the analysis of experimental data. Angrist[1] uses instrumental variables to estimate the treatment effects for sub-populations of domestic violence offenders which hold provided a certain set of conditions are satisfied; since these sub-populations cannot be identified from empirical observation alone, there remains a need to use a method which would give an estimate of the average treatment effect of a mandatory arrest policy for the entire population of domestic violence offenders. This paper estimates average treatment effects for the population of domestic violence offenders by using a partial identification approach which makes no or minimal assumptions on the unknown counterfactual probabilities.

This paper makes important contributions to two different literatures; firstly it applies the literature on partially identified treatment effects to a substantive problem of interest. This

[2]Non-compliance arises frequently in experiments involving human subjects and is not specific to MDVE: Gartin (1995) provides examples of non-compliance in criminology experiments and Efron and Feldman (1991) provide a discussion of non-compliance in medical trials.

paper highlights how the estimation of partially identified treatment effects can be carried out very easily. The advantage of the approach is the greater credibility of weaker assumptions than are used in conventional analysis for the estimation of treatment effects. Secondly, the paper contributes to the broader literature within criminology of whether punishment reduces crime. Sociologists have opposing theories regarding the effects of punishment on behavior: according to **specific deterrence** punishment deters people from repeating crime whereas the **labeling** school of deviance says that punishment makes people commit more crimes due to the negative consequences that result from labeling an individual as deviant. This paper finds support for a mandatory arrest policy for domestic assault offenders; in other words, punishment in the form of arrest reduces repeat incidence of domestic violence among domestic assault offenders.

In the paper, partially identified recidivism probabilities associated with the different treatments are estimated first without making any assumptions on randomization. The recidivism probabilities associated with the different treatments are then estimated when making the assumption that assigned treatments are random but without making any assumptions on the counterfactual probabilities due to non-compliance. Finally, I make two different set of identifying assumptions in addition to random assignment of treatments: firstly, that non-compliance occurs when officers are faced with high-risk offenders only and secondly, that cases of non-compliance occur when the assigned treatment is not the treatment associated with the lowest offender recidivism.[3] Using the MDVE data, I find that a mandatory arrest policy is associated with a reduction in recidivism if treatments are randomly assigned in the MDVE. If one takes sampling variation into account, I find that a mandatory arrest policy is associated with a reduction in recidivism if in addition to randomly assigned treatments one assumes that cases of non-compliance occur when officers are faced with high-risk offenders.

Section 2 examines the experimental design of the MDVE and uses the MDVE data to estimate treatment effects of a mandatory arrest policy. Section 3 looks at the overall implications of mandatory arrest policy for domestic assault offenders today. Section 4 concludes.

[3]These identifying assumptions are similar to the 'skimming' and 'outcome optimization' models which are used in Manski and Nagin[18] and Stoye[27] to estimate partially identified treatment effects of different sentencing policies for juvenile offenders in non-experimental settings.

2 The Minneapolis Domestic Violence Experiment

The Minneapolis Domestic Violence experiment was carried out in 1981 over a period of eighteen months. Repeat incidence or recidivism of domestic violence against the same victim was measured over a six month follow up period using criminal justice reports (official data) as well as victim interviews for offenders who were randomly assigned the treatments of arrest, separation (an order to the offender to leave for eight hours) and advice (informal mediation at the officer's discretion). In the analysis of this paper I examine only official data, since the victim interview data has a large component of missing values due to attrition.

The initial analysis of the MDVE (in Sherman and Berk[22]) was carried out by modeling linear probability and logit specifications on a binary recidivism outcome and a proportional hazard specification on a time to failure outcome. The analysis showed that arrest resulted in significantly lower recidivism than separation when using official data and that arrest resulted in significantly lower recidivism than advice when using victim interview data. Since arrest was associated with the lowest recidivism, the MDVE results ended up playing an important role in adoption of a mandatory arrest policy nationwide. However, there was non-compliance with assigned treatment; different strategies have been used to deal with this. Berk and Sherman[6] model the non-compliance directly using parametric assumptions and then incorporate the non-compliance into the analysis of the experimental data. Angrist[1] uses instrumental variable approaches to estimate treatment effects for the subpopulation of domestic violence offenders who comply with the assigned treatment. The disadvantage of the first approach is that the results rely on distributional and functional form assumptions which are not motivated by offender or police behavior. The disadvantage of the second approach is that it only provides treatment effects for an unobservable sub-population of domestic violence cases in which subjects would have changed treatment status if given a different treatment assignment; therefore it does not allow one to estimate the average treatment effects for the entire population of domestic violence offenders, which are of particular importance in the analysis of a mandatory arrest policy.

2.1 Experimental Design and Sample

The MDVE was carried out, initially by a group of 33 police officers, in the two Minneapolis precincts with the highest density of domestic violence crime reports and arrests. Table 1 is reproduced from Sherman and Berk[22] and gives characteristics of part of the sample (205 of the total 312) for whom initial interviews were obtained. The couples which reported domestic violence and which formed the sample of the MDVE were disproportionately unmarried couples with high unemployment rates (a rate of 60 percent in a community for which the average was 5 percent), who were likely to have had past incidents of domestic violence and arrest as well as facing intervention by the police. They were also likely to have lower education levels and to belong to a minority race or a mixed race couple. The high proportion of Native Americans is the result of Minneapolis' proximity to many Indian reservations. The data from Table 1 indicates that except for the high representation of Native Americans the sample is likely to be fairly representative of the kind of domestic violence cases that get reported to the police.

The experiment was designed to analyze the effect of three different treatments: arrest, separation and advice on repeat incidence of domestic violence. Officers were given a pad of report forms with each form color coded for the different treatments. In order to ensure random assignment of treatments the forms were numbered and arranged in random order for each officer. When a call reporting a case of domestic violence came in the officer determined whether the case was eligible for the experiment and then applied the treatment that was topmost in the pile of color coded forms. Once a case was made eligible for the experiment the officer could still choose to deviate from the randomly assigned treatment, provided sufficient reason for deviation was recorded. This resulted in non-compliance with the randomly assigned treatment.

Once the officers had dealt with a particular case they made a brief report and gave it to the research staff for follow-up. The research staff then followed up on the cases by detailed face to face interviews as well as telephone follow up interviews every two weeks for twenty-four weeks. Criminal justice reports mentioning the suspects name during the six month follow up period were also obtained. Recidivism was measured as repeated domestic violence against the same victim. In all subsequent analysis, treatments are considered either arrest or non-arrest

Table 1: Victim and Suspect Characteristics: Initial Interview Data and Police Sheets

Unemployment	Victims	61%	
	Suspects	60%	
Relationship of suspect to victim	Divorced or Separated Husband	3%	
	Unmarried male partner	45%	
	Current husband	35%	
	Wife or girlfriend	2%	
	Son, brother, roommate, other	15%	
Prior assaults, police involvement	Victim assaulted by suspect, last 6 months	80%	
	Police intervention in domestic dispute, last 6 months	60%	
	Couple in counseling program	27%	
Prior arrests of male suspects	Arrested for any offense	59%	
	Arrested for crime against person	31%	
	Arrested on domestic violence statute	5%	
	Arrested on an alcohol offense	29%	
Mean Age	Victims	30 years	
	Suspects	32 years	
		Victims	Suspects
Education	less than High school	43%	42%
	High school only	33%	36%
	greater than High school	24%	22%
Race	White	57%	45%
	Black	23%	36%
	Native American	18%	16%
	Other	2%	3%

[1] This information was available for cases in which initial interviews were obtained, N=205.

(separation or advice). Data on assigned treatments, received treatments and recidivism outcomes as taken from official reports is given in Table 2. The deviation from assigned treatment was generally from non-arrest to arrest and hardly ever from arrest to non-arrest.

For the outcomes using victim interviews, recidivism was measured as cases in which the victim reported new violence during follow-up interviews. Due to sampling attrition the recidivism outcomes from victim interviews could be obtained on just 161 of the 312 cases. Without strong assumptions regarding the missing data due to sampling attrition, this source of data does not provide much information, and is therefore not used in the analysis.

Table 2: Official Data ($N = 312$)

Assigned Treatment	Received Treatment	% who Recidivate
Arrest	Arrest	0.110
Arrest	Non-Arrest	0.000
Non-Arrest	Arrest	0.182
Non-Arrest	Non-Arrest	0.210

[1] Recidivism is measured as repeat violence against the same victim over a six month follow up period.

2.2 Non-Compliance and Partial Identification

Following the potential outcomes notation from Rubin[29], $T_i \in \{0, 1\}$ represents the treatment given to unit i (in this setting each unit is a suspect, for $i = 1, 2, ...312$) which equals 0 for non-arrest (either separation or advice)[4] and 1 for arrest; D_i is the received treatment and Z_i is the randomly assigned treatment. $Y_i(t)$ is a binary outcome which equals 1 if the suspect commits another act of violence (recidivates) against the same victim and 0 if the suspect does not recidivate. We can observe (Y_i, D_i, Z_i).

The probability that the suspect recidivates under treatment t, $P[Y_i(t) = 1]$ is

$$P[Y_i(t) = 1] = \sum_{j=0}^{1} P[Y_i(t) = 1|D_i = j] \times P(D_i = j)$$

where the equality follows from the law of total probability. $P[Y_i(t) = 1|D = t]$ is the probability that the suspect would recidivate if he were given treatment t given that the treatment he received was also t; this can be observed from the data. $P[Y_i(t) = 1|D_i \neq t]$ is the counterfactual or the probability that the suspect would recidivate if he were given treatment t given that he is actually not given t. In the absence of identifying assumptions one does not know these counterfactuals. $P(D_i = j), j \in \{0, 1\}$ is the probability that the received treatment is j which can be observed from the data.

Initially and to provide a benchmark, suppose the data came from an observational study in which **nothing is assumed about the randomization of treatments**. In this case and without making any assumptions about the counterfactual probabilities, one can obtain an interval of values or bound for the recidivism probability by setting the unknown counterfactual probabilities equal to zero (giving the lower bound) and one (giving the upper bound). Doing

[4]The treatment non-arrest is a treatment in which it is equally likely that the suspect is either separated or the suspect is advised by the police.

so gives the following identification region for recidivism probability from treatment t

$$P[Y_i(t)=1] \in [P^L[Y_i(t)=1], P^U[Y_i(t)=1]]$$

where

$$P^L[Y_i(t)=1] = P[Y_i(t)=1|D_i=t] \times P(D_i=t)$$

and

$$P^U[Y_i(t)=1] = P[Y_i(t)=1|D_i=t] \times P(D_i=t) + [1-P(D_i=t)]$$

The no assumption bound was introduced in Manski[15]. From the experimental data, the no assumptions bounds on recidivism probabilities are $P[Y_i(0)=1]=[0.12,0.55]$ and $P[Y_i(1)=1]=[0.06,0.63]$. Given the bounds on treatment effects, arrest is not a better treatment than non-arrest in the absence of any assumptions regarding counterfactual probabilities since the upper bound on recidivism from arrest is greater than the lower bound on recidivism from non-arrest.

The experiment randomly assigned different treatments, but, it was possible for the police officers who applied the treatments to deviate from the assigned treatment given that sufficient cause was provided. This resulted in non-compliance with the assigned treatment. Given the assumption that **assigned treatment is random**, one can make an improvement on the no-assumption bound using the treatment effect observed among compliers.

Assuming randomly assigned treatments, the recidivism probability from treatment t is given by

$$P[Y_i(t)=1] = P[Y_i(t)=1|Z_i=t]$$
$$= \sum_{j=0}^{1} P[Y_i(t)=1|Z_i=t, D_i=j] \times P(D_i=j|Z_i=t)$$

The bounds on recidivism probabilities for treatment t may be obtained by setting the unknown probabilities to their maximum and minimum possible values, which gives the following identification region for recidivism probabilities

$$P[Y_i(t)=1] \in [P^L[Y_i(t)=1], P^U[Y_i(t)=1]]$$

where

$$P^L[Y_i(t)=1] = P[Y_i(t)=1|D_i=Z_i] \times P(D_i=t|Z_i=t)$$

and

$$P^U[Y_i(t) = 1] = P[Y_i(t) = 1|D_i = Z_i] \times P(D_i = t|Z_i = t) + [1 - P(D_i = t|Z_i = t)]$$

Using data from the experiment gives the identification region for recidivism probabilities as $P[Y_i(0) = 1] = [0.17, 0.37]$ and $P[Y_i(1) = 1] = [0.11, 0.12]$. The identification region for the arrest treatment has smaller length in comparison to non-arrest; this is because of the higher non-compliance with assigned non-arrest in comparison to assigned arrest treatments. Given the identification regions associated with arrest and non-arrest, arrest is the better treatment since the upper bound on recidivism from arrest is less than the lower bound on recidivism from non-arrest. Therefore as long as the assigned treatment is random, experimental data indicates that arrest is most effective in reducing repeat incidence of domestic violence against the same victim. These bounds are sharp, that is they are the narrowest bounds which can be estimated given the identifying assumptions. For the MDVE data, these bounds are also identical to those estimated using the method provided by Balke and Pearl[3] as the closed form solution of a linear programming problem for estimation of partially identified treatment effects under imperfect compliance.

It is possible to narrow the identification region even further, by making additional identifying assumptions on recidivism probabilities among the group of non-compliers. One set of identifying assumptions could be if cases of non-compliance with assigned treatment occur only when the officers are faced with **high-risk** offenders who have a higher recidivism probability under all treatments while cases of compliance with assigned treatment occur only when the officers are facing **low-risk** offenders who have a lower recidivism probability under all treatments. Such an identifying assumption would be consistent with officer and offender behavior but assumes also that officers have some knowledge of offender recidivism based on offender characteristics.

Given the assumptions outlined in the previous paragraph,

$$P[Y_i(t) = 1|D_i = Z_i] < P[Y_i(t) = 1|D_i \neq Z_i]$$

The lower bound for recidivism probability from treatment t is then

$$P[Y_i(t) = 1] = P[Y_i(t) = 1|Z_i = t]$$
$$= P[Y_i(t) = 1|D_i = Z_i] \times P(D_i = Z_i|Z_i = t) + P[Y_i(t) = 1|D_i \neq Z_i] \times P(D_i \neq Z_i|Z_i = t)$$
$$> P[Y_i(t) = 1|D_i = Z_i]$$

where the last inequality follows from the identifying assumption on non-compliers. The new bounds on treatment t are now given by

$$P[Y_i(t) = 1] \in [P^L[Y_i(t) = 1], P^U[Y_i(t) = 1]]$$

where

$$P^L[Y_i(t) = 1] = P[Y_i(t) = 1|D_i = Z_i]$$

and

$$P^U[Y_i(t) = 1] = P[Y_i(t) = 1|D_i = Z_i] \times P(D_i = t|Z_i = t) + [1 - P(D_i = t|Z_i = t)]$$

Given the data the bounds on recidivism probabilities are $P[Y_i(0) = 1] = [0.21, 0.37]$ and $P[Y_i(1) = 1] = [0.11, 0.12]$. Given the tighter bounds on recidivism probabilities, it is clear that arrest is even more effective at reducing recidivism probability than non-arrest when we make the assumption that assigned treatment is random and in addition that non-compliance with assigned treatments occurs when officers face high-risk offenders.

Consider again the case in which assigned treatments are random but that we make an alternative but equally plausible set of identifying assumptions on the recidivism probabilities among non-compliers as when officers deviate from assigned treatments when they face high-risk offenders. Suppose instead that officers deviate from the assigned treatment only when the **recidivism probability under the assigned treatment is higher than the alternative treatment**. This identifying assumption also assumes that officers have some knowledge of offender recidivism based on offender characteristics.

Given the new set of assumptions outlined in the last paragraph,

$$P[Y_i(t) = 1|D_i = t] < P[Y_i(\bar{t}) = 1|D_i = t]$$

where $\bar{t} \in T, \bar{t} \neq t$. The lower bound for recidivism probability from treatment t is then

$$\begin{aligned}
&P[Y_i(t) = 1] = P[Y_i(t) = 1|Z_i = t] \\
&= P[Y_i(t) = 1|D_i = Z_i] \times P(D_i = Z_i|Z_i = t) + P[Y_i(t) = 1|D_i \neq Z_i] \times P(D_i \neq Z_i|Z_i = t) \\
&> P[Y_i(t) = 1|D_i = Z_i] \times P(D_i = Z_i|Z_i = t) + P[Y_i(\bar{t}) = 1|D_i \neq Z_i] \times P(D_i \neq Z_i|Z_i = t)
\end{aligned}$$

The new bounds on treatment t are now given by

$$P[Y_i(t) = 1] \in [P^L[Y_i(t) = 1], P^U[Y_i(t) = 1]]$$

where

$P^L[Y_i(t)=1]$

$= P[Y_i(t)=1|D_i=Z_i] \times P(D_i=Z_i|Z_i=t) + P[Y_i(\bar{t})=1|D_i \neq Z_i] \times P(D_i \neq Z_i|Z_i=t)]$

and

$P^U[Y_i(t)=1] = P[Y_i(t)=1|D_i=Z_i] \times P(D_i=t|Z_i=t) + [1-P(D_i=t|Z_i=t)]$

Given the data the bounds on recidivism probabilities are $P[Y_i(0)=1]=[0.20, 0.37]$ and $P[Y_i(1)=1]=[0.11, 0.12]$. The recidivism probability from arrest and non-arrest treatments now lies within a narrower identification region than the case when only random assignment of assigned treatments was considered.

Table 3 gives the bounds on recidivism probabilities under different assumptions on counterfactual probabilities. In all cases except the worst case bounds, the upper bound on recidivism from arrest is less than the lower bound on recidivism from non-arrest. The size of the identification region is smaller the stronger the assumptions on the counterfactual probabilities, hence the bounds are widest when nothing is assumed about any of the counterfactual probabilities but they are shortest when identifying assumptions on non-compliance behavior of officers are made in addition to random assignment of treatment.

Table 3 also provides the 90% confidence intervals on the partially identified treatment effects which allows one to take into account sampling variation. Since I am interested in the true recidivism probabilities for policy analysis, the confidence intervals are constructed as outlined in Imbens and Manski[11]. These confidence intervals are for the parameter and not for the bounds on it, in that they converge uniformly to the true value of the parameter with the specified probability. It is easy to check whether the assumptions outlined in Imbens and Manski[11] are satisfied. The worst case bounds and the randomly assigned treatment bounds can both be reformulated as special cases of the illustrative missing data example from Imbens and Manski[11]. For the worst case bounds the width of the identification region is one minus the propensity score and for the randomly assigned treatments case the width of the identification region is one minus the probability of non-compliance. For the case in which cases of non-compliance occur when officers face high-risk offenders or when assigned treatments are not associated with the lowest recidivism, again the assumptions required to apply the results from

Imbens and Manski[11] are satisfied; specifically super-efficiency is satisfied since the length of the identification region is known and is not a nuisance parameter. Once sampling variation is taken into account, arrest is associated with lower recidivism when we make the strongest identification assumptions ie that treatments are randomly assigned and that non-compliance occurs when officers face high-risk offenders who have higher recidivism probability under all treatments.

Table 3: Recidivism probabilities, MDVE

	Treatment	Bounds	90% CI
worst case bounds	Non-Arrest	$[0.119, 0.551]$	$[0.096, 0.574]$
	Arrest	$[0.058, 0.625]$	$[0.041, 0.641]$
randomly assigned treatment	Non-Arrest	$[0.168, 0.368]$	$[0.137, 0.400]$
	Arrest	$[0.109, 0.120]$	$[0.063, 0.166]$
non-compliance for high-risk offenders	Non-Arrest	$[0.210, 0.368]$	$[0.171, 0.400]$
	Arrest	$[0.110, 0.120]$	$[0.063, 0.166]$
non-compliance for lowest recidivism treatment	Non-Arrest	$[0.205, 0.368]$	[0.158,0.400]
	Arrest	$[0.110, 0.120]$	[0.063,0.166]

The average treatment effects for the population of domestic violence offenders can also be estimated using the bounds on recidivism probabilities, as given in Table 3.[5] In order to estimate the average treatment effect of a mandatory arrest policy, partially identified treatment effects are estimated as,

$$E[Y_i(1) - Y_i(0)] \in [P^L[Y_i(1) = 1] - P^U[Y_i(0) = 1], P^U[Y_i(1) = 1] - P^L[Y_i(0) = 1]]$$

Table 4 provides the average treatment effect of a mandatory arrest policy. Another popular measure of the effectiveness of a mandatory arrest policy is the intent to treat estimate, which is also provided in table 4. In order to estimate the intent to treat measure, the observed recidivism for the population of domestic violence offenders is first estimated as

$$E[Y_i] = \sum_{j=0,1} P[Y_i(j)|D_i = j] \times P(D_i = j)$$

Then the intent to treat estimate is the causal effect of assigned treatments,

$$E[Y_i(1) - Y_i(0)] = E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]$$

Table 4 provides the partially identified treatment effects as well as the intent to treat

[5]It is also equally simple to estimate the average treatment effect on the treated and the average treatment effect on the untreated, but these are not reported in this paper.

estimate. The confidence interval around the intent to treat estimate is constructed by using 100 bootstrap replications. From the table, one can immediately see that except for the worst case partially identified bounds, a mandatory arrest policy is always associated with negative recidivism. In other words, a mandatory arrest policy will reduce repeat offenses by domestic assault offenders and this result holds under the minimal identification assumptions that I make in the analysis of treatment effects. Note also the advantage of using the partial identification approach in comparison to the intent to treat estimate. While the intent to treat estimate is negative, it is closer to the upper bound estimated using the partial identification approach. By focusing on the intent to treat estimate alone one neglects the potentially larger decreases in recidivism probabilities that are associated with a mandatory arrest policy.

Table 4: Treatment effects, MDVE

		Estimate	90% CI
ATE	worst case bounds	$[-0.494, 0.506]$	$[-0.532, 0.545]$
	randomly assigned treatment	$[-0.259, -0.049]$	$[-0.337, 0.029]$
	high-risk offenders	$[-0.258, -0.091]$	$[-0.336, -0.005]$
	lowest recidivism treatment	$[-0.259, -0.085]$	$[-0.337, 0.008]$
Intent to Treat		-0.096	$[-0.165, -0.007]$

3 A Mandatory Arrest Policy for Domestic Assault Offenders

A re-analysis of the MDVE shows that a mandatory arrest policy is effective in reducing recidivism since the bounds on the average treatment effect from mandatory arrest are negative under a set of plausible assumptions on police and offender behavior. The analysis in the previous sections of this paper addresses much of the concerns regarding internal validity of the MDVE and the case for a mandatory arrest policy. I find that even under minimal assumptions on the counterfactual distributions the mandatory arrest policy is still associated with lowest recidivism in comparison to other treatments using the MDVE data. In this section I address also some of the other recent criticisms that have been made of a mandatory arrest policy for domestic assault offenders.

Another concern of the mandatory arrest policy is to do with the effects of a mandatory arrest policy over time. Research by Tauchen and Witte[30] shows that if one uses a dynamic model for the probability of violence in the follow-up period using MDVE data then the deterrence effect of a mandatory arrest policy wears off very quickly. However, Tauchen and Witte[30] use victim interview data for their outcome measure; this data only provides an incomplete picture of recidivism associated with mandatory arrest due to the high rate of attrition and missing observations in this outcome measure. If the missing victims or those who were more difficult to locate are also the ones who fear higher reprisal from offenders and these offenders are also more likely to be affected by the mandatory arrest policy then the deterrence effects of a mandatory arrest policy over time could potentially be much higher than those estimated by Tauchen and Witte[30].

As mentioned previously, the number of incidents of domestic violence in the US has been on the decline over the last two decades. However, work by Iyenger[13] which uses data from the FBI Supplementary Homicide Reports from 1976-2003 suggests that intimate partner homicides actually increased in states which had introduced mandatory arrest laws.[6] However, since homicides only form a small subset of domestic violence incidents, this analysis does not show (and does not claim to) that the mandatory arrest policy has increased the total number of incidents of domestic violence. Iyenger[13] shows, however, using data from the National Crime Victimization Surveys, that the behavioral impact of a mandatory arrest policy on reporting of incidents by victims may be negative and important. Unfortunately, while it is important to account for the effect of a decline in reporting by victims as a result of mandatory arrest it is difficult to do so when using data from MDVE in which victims do not know about the mandatory arrest policy when they report incidents of domestic violence to the police. The work by Iyenger[13] does suggest an important improvement that needs to be made to future versions of existing experimental designs which would in some way capture also the impact of mandatory arrest on reporting by victims. This may be done, for instance, by informing a subset of potential victims in the precinct which carries out the experiment of the mandatory arrest policy in advance of reporting, but this is left as an open question for future research.

The MDVE data provides us with an opportunity to determine the treatments effects and

[6] The decline in intimate partner homicides could be the result of introduction of unilateral divorce laws across states, see Stevensen and Wolfers[25].

efficacy of a mandatory arrest policy. The MDVE data supports the introduction of a mandatory arrest policy since the data shows a reduction in recidivism even under a minimal set of plausible assumptions. There are large benefits involved in adoption of a policy that reduces the rate of domestic violence in the current generation; such a policy can have important intergenerational effects beyond the present as highlighted in work by Pollak[21].

4 Conclusion

I re-analyze the data from the Minneapolis experiment to show the effectiveness of a mandatory arrest policy in reducing repeat incidence of domestic violence. The experimental results continue to hold relevance today in the debate on mandatory arrest for domestic assault offenders; in light of recent work, there are improvements to the experimental designs that may be made which also take into account the behavioral impacts of mandatory arrest on reporting of domestic violence cases by victims.

An important contribution of this paper is the application of the recent literature on partial identification to a substantive problem of interest. The advantage of this approach is that it makes minimal assumptions on counterfactual probabilities and provides treatment effects for the entire population of domestic assault offenders. In this case, I also demonstrate that focusing on the intent to treat estimates alone may lead to an under-estimate of the potential reductions in recidivism arising from mandatory arrest.

Although much progress has been made, the study of a mandatory arrest policy to reduce domestic violence in society continues to hold importance and relevance today as it did twenty years ago.

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