



The RASOR'S Edge: Focused Deterrence in Cambridge, Everett, and Somerville

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Introduction and Background

The Smart Policing Initiative (SPI) is a national program funded by the Bureau of Justice Assistance (BJA). SPI is based on a combination of three components: evidence-based, data-driven, and problem oriented policing. For three Massachusetts police agencies -- Cambridge, Everett, and Somerville – this translated into a focused deterrence initiative.

The purpose of focused deterrence is to discourage criminal behavior by maintaining direct communication and contact with chronic offenders. Law enforcement personnel engage these individuals to inform them about their negative impact on the community and then provide alternative approaches and innovative efforts to combat the social harm they cause. To further cultivate relationships with the offenders, social service agencies provide community resources to match the prevention efforts by law enforcement. However, if the offender fails to comply with the prevention efforts made on his or her behalf, law enforcement has the authority to administer punitive sanctions.

Led by the Cambridge Police Department, the three agencies established RASOR (pronounced razor) - Regional Analytics for the Safety of Our Residents. Figure 1 shows the three jurisdictions.

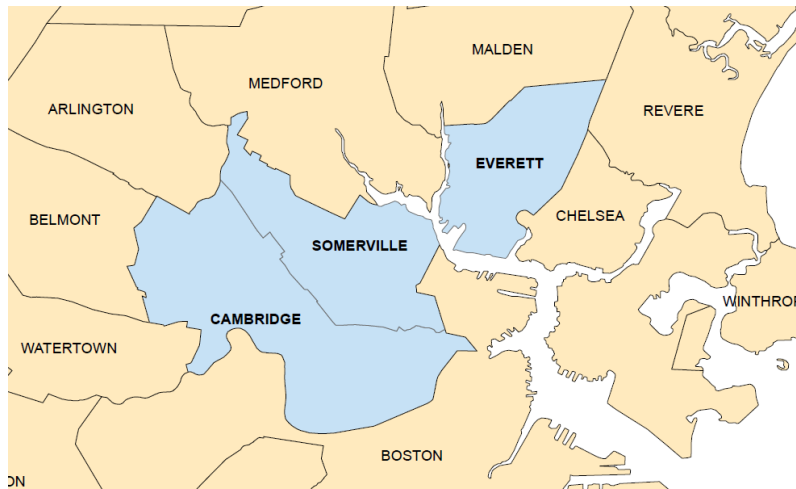


Figure 1. Map of the three jurisdictions

Background and Prior Research

Focused deterrence, or “pulling levers,” is an interagency approach to crime control and prevention designed to target repeat offenders by exploiting the vulnerabilities created by chronic offending (Kennedy, 1997). Often used to address gangs, drug markets, and other groups of offenders, focused deterrence strategies bring together a working group of criminal justice agencies that may include police, probation, parole, state and federal prosecutors, and federal law enforcement (Braga & Weisburd, 2012b). The working group applies a “varied menu of sanctions and incentives” designed to steer offenders toward social services such as substance abuse treatment, mentoring, housing, vocational training, and employment assistance, and away from committing additional crimes (Braga & Weisburd, 2015; Kennedy, 1997, p. 451).

The threat of sanction that is at the heart of focused deterrence is possible because chronic offenders are likely to be uniquely vulnerable to increased surveillance and enforcement. Those

targeted by focused deterrence interventions may have outstanding warrants, are often on probation or parole, are frequently in violation of the terms of their probation or parole, and may be habitually trespassing, drinking or using drugs in public, or selling drugs (Kennedy, 1997). Braga (2008) explained that focused deterrence differs from zero tolerance strategies in that focused deterrence targets specific individuals or groups that are behaving in violent or problematic ways, whereas zero tolerance represents an indiscriminate crackdown “on minor crimes committed by all offenders” (p. 340).

A final aspect of focused deterrence strategies is frequent and direct communication between offenders and law enforcement. This communication takes the form of meetings, also known as “call-ins” or “forums,” in which law enforcement and community officials deliver a clear message to offenders (Braga & Weisburd, 2015). These meetings serve to notify offenders that they are being closely monitored and make them aware first, that law enforcement knows about their illegal activities, that no further law-breaking, however minor, will be tolerated, and that all transgressions will be prosecuted to the fullest extent possible, and second, that help is available to those willing to take advantage of it.

Since the success of the seminal pulling levers intervention, Boston’s Operation Ceasefire, in the mid-1990s, many cities have implemented focused deterrence strategies and many have seen successes (Kennedy, 1997). Primarily, evaluations of focused deterrence interventions in Indianapolis and Boston found reductions in homicides of youth ages 24 and under (Corsaro & McGarrell, 2010; Kennedy, 1997). Next, the cities of Indianapolis, Boston, and Stockton, California all implemented interventions targeting street gangs, and all found a reduction in gun homicides (Braga, 2008; Corsaro & McGarrell, 2010; Kennedy, 1997). Finally, two evaluations of focused deterrence interventions, one targeting open-air drug markets in High Point, North Carolina, and one examining the effects Project Safe Neighborhoods (PSN) in 82 treatment cities across the country, found declines in violent crime in target areas (Corsaro, Hunt, Kroovand Hipple, & McGarrell, 2012; McGarrell, Corsaro, Kroovand Hipple, & Bynum, 2010). These last two interventions could not be classified as complete successes, however, as the High Point intervention actually appears to have led to an increase in crime citywide, and the PSN evaluation found modest effects in high-dose treatment cities, but no effects in low-dose treatment cities (Corsaro et al., 2012; McGarrell et al., 2010).

In a meta-analysis conducted in 2012 (Braga & Weisburd, 2012a), eleven recent focused deterrence interventions were evaluated to determine whether they produced reductions in crime. To be included in the meta-analysis, the interventions had to be randomized controlled trials or quasi-experiments with comparison groups (Braga & Weisburd, 2012a). Of those 11 studies, 10, including the single randomized controlled trial, found “strong and statistically significant crime reductions” (Braga & Weisburd, 2012a, p. 347). The remaining study found no effect (Braga & Weisburd, 2012a).

Another pulling levers intervention stands out as having been unsuccessful. An experimental evaluation of pulling levers was conducted in Indianapolis from 2002-2005 that evaluated the recidivism of individual probationers (Chermak, 2008). The experiment included two treatment groups and a third group that received regular probation. The first group met with federal and local law enforcement and received “primarily a deterrence-based message[,]” although they also got information about employment and treatment options, while the second group met with

community leaders and service providers and received information on available services and on the ways in which violence impacts the community (Chermak, 2008, p. 5).

Upon conclusion of the experiment, the treatment groups were no different from the control group in terms of recidivism and number and type of technical violations. Probationers in all three groups went on to be arrested for similar offenses. These results led to the “unfortunate conclusion . . . that ‘lever pulling’ as implemented in Indianapolis had little effect on the probationers that attended the meetings” (Chermak, 2008, p. 12).

Examining the possible reasons for the failure of pulling levers to change the behavior of Indianapolis probationers, several possibilities emerge. First, there is no evidence that probationers in either treatment group were treated differently from those in the control group in terms of levers pulled during post-meeting follow-up. All three groups of probationers were contacted a similar number of times by probation officers and had a similar number of urine screens ordered. Contacts by police, community leaders, and faith leaders were also consistent across the treatment and control groups. Finally, charges were dismissed at a similar rate for those in the treatment groups compared to those in the control group, and treatment probationers were not sentenced more frequently nor did they receive heavier sentences. Thus, the failure of this intervention may have been due to a lack of follow-through on the part of criminal justice professionals, who, for the most part, did not act on the promises made in the group meetings. It is also a possibility that the Indianapolis study played out differently due to its focus on individuals rather than groups and, unlike in the Boston Ceasefire evaluation, one individual’s actions did not trigger consequences for the entire group.

Thus far, studies of focused deterrence as a strategy for crime control and reduction have shown much promise. Few ill effects have been observed, and many cities have seen significant reductions in youth homicides, gun homicides, and violent crime. At the same time, many questions still remain and scholars have called for more evaluations and in particular for more experiments to assess the effects of focused deterrence interventions (Braga & Weisburd, 2012a; Corsaro et al., 2012; McGarrell et al., 2010).

Operation RASOR

Focused deterrence was implemented over the course of three years through the collaborative efforts of the three Massachusetts police agencies. They created partnerships with case managers, social service providers, other criminal justice agencies, and research staff to collectively cultivate a focused deterrence strategy. RASOR’s goals included preventing future victimization and social harm of the offenders; a reduction of crime within the three cities; and finally, determining whether a focused deterrence approach provided a successful strategy within the region.

While other agencies have implemented focused deterrence strategies, RASOR includes many distinct characteristics and creates an advanced data-driven approach for a focused deterrence program. Because RASOR also operates across three jurisdictions with distinct police departments and District Courts, this unique collaboration exemplifies how regional partnerships may be beneficial for focused deterrence strategies. The agencies worked together to share information about impact players, habitual offenders, crime data, known associates, and other material concerning these individuals. They recognized that their jurisdictions have set

boundaries, but chronic offenders may cross these boundaries and have impacts across the region.

Further, RASOR uses a data-driven approach and implements a research-based process. While many focused deterrence efforts rely on individual officers to identify chronic offenders within their communities, RASOR employs a regional database to select the targeted participants. More information on this data-driven approach is provided in the *Methodology* section.

Additionally, RASOR assigns a social harm index score to each offender. Traditionally, agencies employing these strategies only target specific violent offenders or those who commit a specific type of crime. RASOR relies on various sources of data to not only examine individuals who have extensive violent criminal and arrest histories, but also those who cause social harm to the communities. This includes individuals who use a great deal of police resources, such as the chronic addict or transient individual who is responsible for numerous calls for service or a high number of complaints from businesses and other community members. Since officers are responding to the frequent calls and encounter these same individuals, implementing RASOR allowed law enforcement to also help these individuals by providing opportunities for service delivery and case management. This is very different from other focused deterrence approaches where law enforcement assumes only the traditional enforcement efforts.

The final component of RASOR was to use an experimental design to measure the effects of focused deterrence using science. Because of the distinctive and innovative focused deterrence efforts within Cambridge, Everett, and Somerville, RASOR employed a randomized control test (RCT) to examine the impact of the approach on 150 offenders. In April 2014, the lead agencies began implementing the program and reaching out to the potential participants.

Implementation

After establishing the collaboration, evaluating prior research on focused deterrence strategies, and conducting extensive trainings, the three departments implemented their regional and data-driven focused deterrence strategy.

Phase 1: Identification

Potential candidates were ascertained by identifying individuals with the highest calculated social harm index. (Please see the *Methodology* section on page 7) for further details on how this composite score was obtained.

Phase 2: Outreach Phase

During this time, crime analysts, police officers, and detectives developed detailed case profiles on the potential candidates. Each candidate was assigned a police case manager who would then notify the candidate of his or her status as an impact player, explaining the opportunity to attend a notification meeting. If the candidates were hesitant or unresponsive, the case manager engaged family or friends to facilitate and assist with the initial contact.

Phase 3: Notification Meeting (Call-ins)

Once the police case managers convinced the candidates to participate in RASOR, they arrived at the notification meetings or call-ins. During Operation RASOR, they held three established call-ins: April 2014, August 2014, and finally, November 2014. Candidates were again informed of the harm they cause to the three communities. They were told of the partnership between the agencies and the available resources to assist in changing their behavior. Substance abuse programs, employment opportunities, housing options, and other available services were provided. Individuals were informed that continued criminal behavior would result in a combined effort to hold the candidate accountable through arrests, prosecutions, and enhanced sanctions. Therefore, the risk of punishment was higher because of the intense supervision they received.

Phase 4: Resource Delivery Phase

The individuals who decided to participate in RASOR met with the police detectives and case manager for assessment and treatment options. These individuals began to build trust with the participants and assisted in developing individual goals and action plans. The social service coordinator and police case manager worked together to provide services and deterrence efforts that included:

- a) Develop Individual Treatment Plan/Goals list;
- b) Utilize required and follow-up meetings with offenders as an additional resource for support alongside of community resources.
- c) Work together with Detectives;
- d) Coordinate meeting times and attend Court Dates;
- e) Reach out to individuals in jail to maintain constant communication regarding activity and contact with participants;
- f) Engage and inform criminal justice partners (including probation, parole, prosecutors, judges, defense bar, and public defender's office) to ensure adherence to desisting from criminal activity or providing necessary information for enhanced enforcement efforts;
- g) Conduct community involvement by reaching out to providers (ROCA, Transition House, Cambridge Works Program);
- h) Conduct site visits, meetings, follow-ups, and outreach to providers to keep them engaged;
- i) Collaborate with providers to obtain feedback and information regarding participants.

5. Relentless follow up

It is imperative that the social service coordinator and the police case managers engage and maintain constant communication with the participants. Texts, calls, check-ins, and drive-by visits are all important to maintaining that relationship and level of trust. Furthermore, it also reminds the participants of their constant supervision.

Another effort made to ensure relentless follow up is to include other members of the law enforcement organizations in the operation. For RASOR, crime analysts presented information

through bulletins and roll-call meetings to inform other officers within the three cities and the broader metropolitan region about the operation. This created a department wide approach which allowed other officers to assist the police case managers by keeping them in the loop. Additionally, this approach cultivated buy-in from other officers on the importance of a focused deterrence strategy.

For a flow chart and additional information of the implementation process, please see Appendix A.

Impact Evaluation

Methodology

Sample and Research Design. This evaluation used a randomized experiment to assess the impact of focused deterrence on individual reoffending. The randomized experiment constitutes the *gold standard* for research design in social sciences and allows for an unbiased estimate of the treatment effect using the Intent to Treat (ITT) analysis.

The Cambridge, Everett, and Somerville Police Departments provided detailed information on approximately 280,000 unique individuals compiled from their regional databases. Of these individuals, approximately 60,000 have an offense history that included being listed as a suspect or defendant. Arrest and incident data for these individuals were used to compute a weighted “serious offender” score that incorporates:

- The nature of the offense
- The role of the individual (defendant, suspect, involved person, etc.)
- A decay factor that adjusts for offense

These social harm scores were computed for all offenders in the database and then the individuals were rank ordered according to their offense histories. From this master list, the top 150 scoring individuals were selected for inclusion in the intervention. These 150 individuals were stratified into three groups of 50 according to their weighted scores. In each strata, 25 individuals were randomly assigned to the treatment group and 25 individuals were assigned into the control group which resulted in a total of 75 individuals in the treatment and 75 individuals in the control group. Descriptive statistics for all 150 cases are provided in Table 1.

For individuals in the treatment group, the police case managers and social service workers began the implementation of the strategy through the initial contact, notification meetings, and then with the resource delivery. Since participation in case management and outreach is voluntary, it was expected that a subset of cases assigned to the

Table 1. Descriptive Statistics for Selected Variables

Variable	Mean	SD	Min	Max
Risk Score	26.54	10.02	8.07	74.36
Age	33.42	12.67	19.00	77.00
	Count	Pct		
Sex				
Male	119	82.64		
Female	25	17.36		
Lead Agency				
Cambridge	105	72.92		
Somerville	16	11.11		
Everett	21	14.58		
Race				
White	61	42.36		
Black	56	39.72		
Hispanic	24	17.02		
Unknown	3	2.08		

treatment condition would participate with regular meetings with the case management team and develop action plans for treatment. Non-participants would still receive more intensive supervision and enforcement activities in the community during the intervention. The control group were only subject to standard law enforcement activities during the intervention period.¹

Analytic Strategy

The outcome for this analysis is the length of time between the start of the experiment to the time of arraignment. Arraignment is when a defendant is formally charged with a crime and was selected in lieu of arrest as many arrests may not lead to formal charges against an individual. The start time for each cohort differs as the intervention began on 4/9/2014 for Cohort 1, 8/12/2014 for Cohort 2, and 11/13/2014 for Cohort 3. The observation period concluded on 5/31/2015 for all cohorts. We hypothesize that members of the treatment group will have a longer average time until arraignment for a new offense compared to the control group.

Because the outcome is a measure of time until arraignment, survival analysis was selected as the appropriate method for analyzing these data. Survival analysis is used to examine: 1) time until an event has a naturally skewed distribution, and 2) many of the individuals observed in the analysis are “right censored” meaning that no arraignment occurred during the observation period. Survival analysis techniques specifically address both issues and allow for estimates of the impact of treatment on time until arraignment on a new offense. Research staff conducted the analysis using Stata 13 to estimate both non-parametric (Kaplan-Meier Survival Curves) and semi-parametric (Cox Regression) models.

There are a number of important considerations in these data for survival analysis. First, unlike traditional survival models (where the outcome is usually death), individuals could be arraigned multiple times across the observational period. Adjustments for recurring events were made by altering the computations of the empirical survival curves and using cluster adjusted standard errors. Second, a number of individuals were “unobserved” or no longer at risk for one or more periods of time within the observational period. This can occur for a number of reasons:

- Incarceration for offenses that occurred before the observational period but continued beyond the start of the observational period
- Post-conviction incarceration for offenses that occurred during the observational period
 - Released prior to the end of the observational period
 - Released after the observational period
- Pre-trial incarceration for offenses that occurred during the observational period
 - Convicted and incarcerated
 - Not convicted or convicted and sentenced to time served
- Subject’s death that occurred prior to the end of the observational period
- Unrecorded start or end dates for incarceration during the observational period

¹ Treatment contamination effects are often a crucial consideration when assessing the impact of interventions as members of the control group may inadvertently be exposed to the treatment. This was not an issue in the current evaluation as the case management team was not provided with a list of individuals constituting the control group. In fact, the case management team was unaware of the members of the control group until data collection for the follow-up period was conducted.

For individuals who were incarcerated at the beginning of the observational time period, the observed time span began at the date of their release rather than the date of beginning of the experiment. For six individuals (all in the control group), their release date occurred after the end of the observational period. These individuals were dropped from the analysis. For individuals who were incarcerated during the observational period, they were considered not at risk for the time period spanning their start and end dates of incarceration.² If the end date of their incarceration exceeded the end date of the observational period, they were considered right censored at the start of their incarceration period. For individuals incarcerated pre-trial and subsequently incarcerated, the end of the observational period was set at the beginning of the pretrial incarceration rather than the arraignment date. For individuals incarcerated pre-trial and subsequently released, they were considered not at risk between the start and end dates of their pretrial incarceration. Two subjects (both in the treatment group) died prior to the end of the observational period. These individuals were considered right censored at the date of their death. Finally, there was one individual who was missing the end dates of incarceration and was removed from the current analysis. The final sample consists of 142 subjects at risk over a total of 41,405 days (average 291.58 days at risk per subject) with 125 observed failures during the risk periods.

Results

T-test for independent samples and Z-tests for differences in proportions were used to assess the extent to which the treatment and control groups differed after randomization. These tests were conducted for each cohort, as well as for the entire sample. These results suggest that randomization was successful at ensuring comparability between the treatment and control groups. However, statistically significant differences were detected in cohort 2 as the treatment group had a higher average risk score and were more likely to have Everett as the lead agency. In order to ensure that these variables do not adversely impact the findings, both the total risk score and the assigned agency will be included as additional independent variable in multivariate models.

Table 2. T-tests for Comparing Randomization in the Treatment and Control Groups.

Variable	Cohort 1			Cohort 2			Cohort 3			Total		
	Diff	SE	p	Diff	SE	p	Diff	SE	p	Diff	SE	p
Risk Score	-1.032	2.931	0.726	2.971	1.363	0.035	0.212	1.297	0.871	0.536	1.650	0.746
Age	-1.560	3.425	0.651	2.000	4.000	0.619	2.480	3.293	0.456	0.973	2.056	0.637
<i>Cambridge Lead</i>	0.040	0.098	0.684	-0.200	0.131	0.136	0.240	0.131	0.077	0.027	0.073	0.716
<i>Everett Lead</i>	0.000	0.055	1.000	0.240	0.108	0.034	0.000	0.113	1.000	0.080	0.057	0.166
<i>Somersville Lead</i>	-0.040	0.085	0.637	-0.080	0.091	0.384	-0.160	0.089	0.082	-0.093	0.051	0.071
<i>Sex</i>	-0.120	0.107	0.270	-0.120	0.116	0.306	-0.040	0.085	0.637	-0.093	0.060	0.125
<i>White</i>	0.080	0.138	0.564	-0.183	0.140	0.199	-0.250	0.138	0.079	-0.115	0.081	0.161
<i>Black</i>	-0.080	0.138	0.564	0.228	0.136	0.104	0.125	0.140	0.376	0.090	0.080	0.267
<i>Hispanic</i>	0.000	0.113	1.000	-0.045	0.087	0.603	0.125	0.111	0.267	0.025	0.061	0.682

Italics indicate that a Z-test for proportions was conducted.

Bold indicates $p < .05$

² It is theoretically possible that these individuals would commit new offenses while incarcerated, but this was not observed in the current data.

Following this, descriptive information of survival times for the treatment and control group were computed for each cohort as well as the entire sample. The median survival time is defined as the amount of time (in days) it takes for 50% of the sample to fail. Because of the natural robustness of the median to skewness, it is customary to consider median survival times when comparing survival times between two groups. However, the median survival time is only defined so long as at least 50% of all subjects have failed and cannot be computed if a smaller proportion does not fail within the observation window. One alternative, the restricted mean survival time, measures the average survival time among only the cases that fail. This measure unfortunately does not consider the impact of cases that do not fail within the observation window and therefore underestimates the average survival time. A useful alternative is the extended mean, which estimates an exponential survival time distribution to approximate the survival times of cases outside of the observation window. Obviously, the validity of this measure depends on the appropriateness of the exponential distribution. The median, restricted mean, and extended mean survival times are presented in Table 3.

Table 3. Median, Restricted Mean, and Extended Mean of Survival Times by Cohort.

	Median Survival		Restricted Mean		Extended Mean
	Median	SE	RM	SE	
Total	259.00	13.65	246.15	11.04	329.03
Treatment	237.00	31.20	227.10	15.34	286.07
Control	279.00	28.83	267.71	15.62	385.50
Cohort 1	266.00	23.10	245.39	18.66	329.96
Treatment	292.00	40.51	271.78	25.74	375.47
Control	191.00	67.88	218.01	26.39	281.86
Cohort 2	241.00	30.67	198.48	14.09	319.29
Treatment	183.00	42.59	166.20	19.68	227.42
Control	NA ¹		243.46	15.47	537.60
Cohort 3	NA ¹		160.01	8.25	392.74
Treatment	183.00		146.30	13.12	282.15
Control	NA ¹		174.40	9.13	613.98

1. Statistic cannot be computed because 50% of the sample did not fail.

For the total sample, the median survival time is 259 days. The restricted mean survival time is slightly lower at 246.15 days and the extended mean is considerably longer at 329.03 days. The treatment group had a lower survival time across all three measures compared to the control group (median: 237 vs. 279; restricted mean: 227.10 vs. 267.71; extended mean: 286.07 vs. 385.50). This suggests that the treatment group had a shorter time to arraignment, which is contrary to the hypothesized effect. Descriptive information is also presented by cohort. The restricted mean survival time for Cohort 1 is considerably higher than Cohort 2 and Cohort 3 (245.39 vs. 198.48 vs. 160.01 respectively) which suggests that those that fail take longer to fail in Cohort 1. However, the extended mean survival time is considerably higher for Cohort 3 than

Cohort 1 or 2 (392.74 vs. 329.96 vs. 319.29). This is due to the lower number of failures in Cohort 3, which is sensible as this is the lower risk group according to their risk scores.

When comparing treatment and control groups for each cohort, a striking pattern emerges. For Cohort 1, the treatment group has a longer survival time across all measures (median: 292 vs. 191; restricted mean: 271.78 vs. 218.01; extended mean: 375.47 vs. 281.86), suggesting a possible treatment effect consistent with the hypothesized direction. However, Cohort 2 (restricted mean: 166.20 vs. 243.46; extended mean: 227.42 vs. 537.60) and Cohort 3 (restricted mean: 146.30 vs. 174.40; extended mean: 282.15 vs. 613.98) suggest that members of the treatment group are both more likely to fail and have lower survival times upon failing compared to the control group. This is contrary to the hypothesized treatment effect and may indicate differences in treatment delivery across cohorts.

After examining descriptive statistics for survival time, Kaplan-Meier empirical survival functions were generated. As previously mentioned, these survival functions are non-parametric as they make no a priori assumptions about the shape of the survival function. These survival functions are presented in Figure 1. The x-axis corresponds to the number of days after the start of the experiment and the y-axis corresponds to the probability of survival (no arraignment). Well-behaved survival functions start at 1.0 at time = 0 and decrease over time.

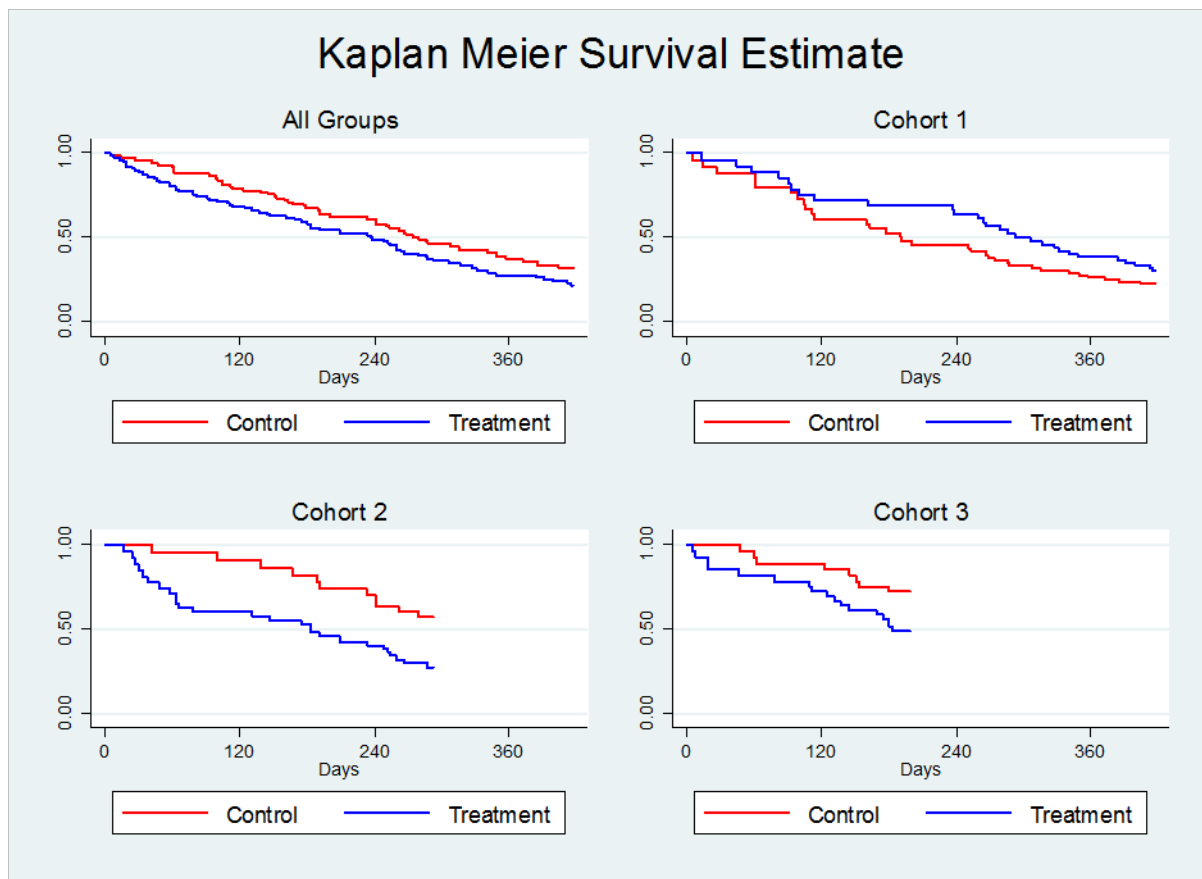


Figure 1. Kaplan-Meier Survival Curves by Cohort

The top left of Figure 1 presents the survival curves for all groups with the treatment group in blue and the control group in red. It is readily apparent that the two groups diverge quickly and the survival curve for the control group remains above the treatment group across the span of the x-axis. This indicates that at every point in time over the observation window, the control group had a lower probability of failure compared to the treatment group. This confirms the earlier suspicion that the control group is performing better than the treatment group. The top right presents the survival functions for Cohort 1. In contrast to the pattern observed across all cohorts, the survival functions for Cohort 1 appear close past 100 days, but the treatment group has a consistently lower probability of failure. After 100 days, the gap between the treatment and control groups widens considerably and the treatment group performs substantially better than the control group after this. The survival functions for Cohort 2 and 3 are presented in the bottom left and right respectively. These curves do not span the extent of the x-axis as the start times for these cohorts occurred later and therefore have shorter observational periods. For Cohort 2, there is an immediate and sizable drop in the survival function of the treatment group due to a large number of quick failures and then the control group outperforms the treatment group by a considerably margin thereafter. A similar pattern is seen for Cohort 3, and although the drop in the survival function is not quite as dramatic as with Cohort 2, the control group still outperforms the treatment group by a considerable margin.

In order to evaluate whether the survival functions are significantly different between treatment and control groups, Stata provides a number of global non-parametric significance tests: the Log-rank, Wilcoxon, Tarone-Ware, and the Peto-Peto-Prentice tests (see Cleves et al. 2008 for a description). The results from these tests are reported in Table 4. These tests follow a Chi-squared distribution with one degree of freedom. Differences in the overall survival function for treatment and control groups were not statistically

Table 4. Non-Parametric Tests of Differences between Survival Functions

	Log-rank	Wilcoxon	Tarone-Ware	Peto-Peto-Prentice
Total	2.68	2.53	2.60	3.15
Cohort 1	0.72	0.82	0.77	1.39
Cohort 2	6.07	6.20	6.14	6.96
Cohort 3	3.48	3.48	3.48	3.54

Bold indicates $p < .05$

significant, suggesting the absence of a treatment effect. Results for the comparison between treatment and control groups by cohort are also reported; however, these should be interpreted with caution as the statistical power of these tests are low. The differences between treatment and control were not statistically significant for Cohort 1 and 3, but were statistically significant for Cohort 2. Again, this confirms that in Cohort 2, the treatment group performed worse than the control group.

To estimate the size of a possible treatment effect, a semi-parametric Cox regression was conducted. This model is called “semi-parametric” because the exact survival distribution

Table 5. Cox Regression of New Arraignment on Treatment

Variable	Haz. Ratio	SE
Treatment	1.405	0.382
Agency = Cambridge	0.980	0.426
Agency = Somersville	0.886	0.493
Risk Score	1.020	0.010

Bold indicates $p < .05$

remains unspecified, but it is assumed that covariates shift the baseline hazard multiplicatively (see Cleves et al. 2008).³ For this model, variables for the total risk score and lead agency were included along with the treatment indicator due to the detected differences between treatment and control groups. The results from the Cox regression are presented in Table 5 and confirm that there was no observed treatment effect. While no treatment effect was observed, there was a

relationship between the risk score and the hazard rate. Specifically, for a single unit increase in the risk score, the hazard rate (i.e., the rate of events per time unit) is expected to increase by 2%.

To better examine the above analyses, the Case Management Team also provided information about the amount of time spent by officers for each individual in the treatment group. This information consisted of specific actions (phone calls, emails, case reviews, meeting with clients) and the time spent on that action for each individual in the dataset. This information was aggregated into a total count of minutes per individual into three categories: administrative (attempts to contact, phone calls, emails), case management (meeting with clients, reviewing cases, courtroom advising), and law enforcement (investigations).⁴ The average time per client was calculated and is presented graphically in Figure 2.

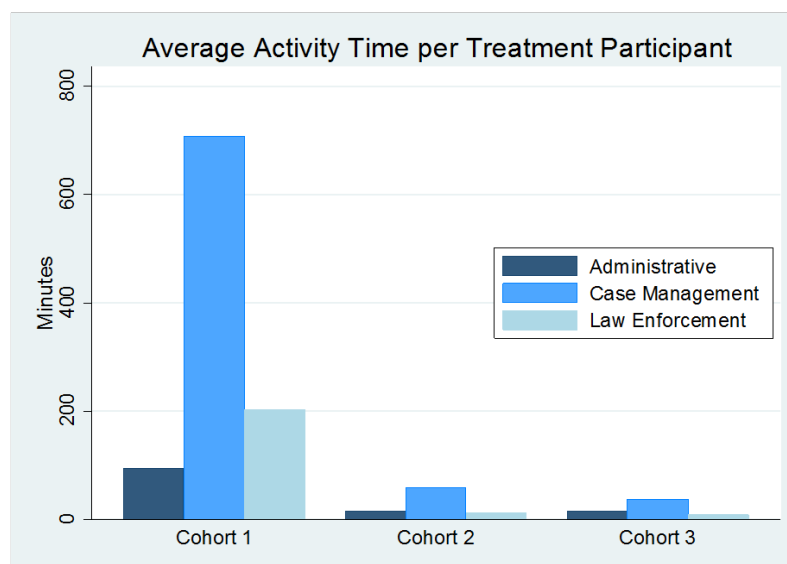


Figure 2. Average Time per Activity per Participant

³ Stata provides a formal test of the proportional hazards assumption based on the Schoenfeld residuals (see Cleves et al. 2008). The results from this analysis suggested no significant violations of the proportional hazards assumption for any of the variables as well as for the model as a whole.

⁴ A number of time estimates were missing from administrative actions. These actions corresponding to these missing time records were predominately time spent on phone calls or emails. The median time spent on administrative tasks was estimated (15 minutes) and substituted so as to avoid underreporting administrative time.

These results provide a very compelling explanation for the discrepant findings between Cohort 1 and Cohorts 2 and 3. Although Cohort 1 was advantaged by a very long observation window, the stark differences observed in the time spent in administrative, case management, and law enforcement tasks cannot be explained by this alone. In Cohort 1, an average of 93.52 minutes per client was spent on administrative tasks compared to 14.80 for Cohort 2 and 15.0 for Cohort 3. On average, 706.8 minutes were spent on case management activities per client in Cohort 1 compared to 58.4 minutes in Cohort 2 and 37.6 minutes in Cohort 3. Finally, in Cohort 1, 202.44 minutes per client were spent on law enforcement activities compared to 12.0 for Cohort 2.0 and 9 for Cohort 3. While ANOVA results indicate that these differences were statistically significant, there appears to be substantial evidence to suggest that the intensity of treatment was substantially lower after Cohort 1. This may have contributed to the absence of a detectable treatment effect.

In conclusion, research staff used arraignment data to conduct a survival analysis on the 150 individuals participating in RASOR. In Cohort 1, the treatment group was found to have a longer survival time across all measures meaning they did not reoffend as quickly as the control group. Research staff also found that the treatment group for Cohort 1 had significantly higher amounts of contact time (including administrative, case management, and law enforcement activities), more than any of the other cohorts.

While the treatment groups in Cohort 2 and 3 reoffended more quickly, the amount of contact time is also significantly less than the treatment group in Cohort 1. This could potentially be explained by the longer engagement period for these cohorts but also could be a consequence of the enhanced supervision and punitive sanctions of a focused deterrence program. It is important to remember that while one goal of focused deterrence is reduce recidivism and crime among participants, another important goal is to swiftly administer punitive sanctions for those who do not comply and ensure that other offenders see the seriousness which law enforcement takes the programs.

Individual Assessment

Although assessing the results of the performed analyses is critical to understanding the impact of the program, it is also important to highlight the complex nature of the cases and individuals participating in RASOR. This next section not only examines the stories of select participants but also demonstrates the intense efforts made by the Case Management Team and their impact on these individuals to improve their lives and decrease the social harm caused to their communities.

Participant A

Participant A is an African-American female who entered the program at the age of 23. The daughter of an alcoholic single mother who worked intermittently as a prostitute, A's childhood was marked by instability and an escalating involvement in the criminal justice system beginning at the age of 13. Consistently accumulating charges, A's criminal activity was seemingly related to her association with known gang members, and included multiple incidents of assault and battery, both as an adult and a juvenile. By her own admission, A had poor role models as a child, surrounded by criminal activity and excessive drug and alcohol abuse. After the assassination of a gang member boyfriend, which caused continuing mental trauma, A began

dating another known gang member and subsequently became pregnant. At the beginning of the treatment period, A was approximately six months along in her pregnancy and had multiple warrants out for her arrest. The officers and the social worker for RASOR were deeply involved with A and she was highly receptive to their assistance. Aiding her at court appearances, coordinating medical and legal assistance, and providing continuous support and advisement, hundreds upon hundreds of hours were spent on A. RASOR staff even visited in the maternity ward, bringing baby clothes and gifts, and coordinating housing arrangements for her and her daughter after discharge. Despite the stressful environment and continuing ties to offenders in the community, A received no new charges during the treatment period and remained receptive to assistance throughout the program. The persistent and personal nature of the RASOR staff's involvement with A, including emotional support to disengage from an unhealthy relationship, perpetual contact, and a caring rapport, established a trusting relationship that aided in A's development during the time period. Her cooperation waned slightly toward the end, likely tied to personal issues, some resulting in the eventual removal of her daughter by the state, but A remained grateful and continued on a much less destructive path than before the treatment period.

Participant B

Participant B is an African-American male who began the effort at the age of 20. One of the youngest participants in focused deterrence, B was particularly difficult to engage. Collecting 13 charges during the treatment period (and two charges less than a week before the advent of the initiative), he proved a problematic case despite strong parental involvement in focused deterrence. Cognitively impaired, possibly related to ongoing marijuana use starting as a pre-teen, B was generally charged with property and drug crimes. He was arraigned twice on assault and battery charges before the treatment period, one for an assault on his father, but these remained his only violent adult offenses. Many of the charges stemmed from domestic conflict initiated by B, including punching his father and habitually stealing from his mother and sister. The officers and social worker involved with RASOR dedicated extensive time to B, attempting contacts, attending court hearings, and working in conjunction with B's dedicated parents. Notwithstanding the multitude of attempts at engagement, B refused to attend meetings and seemingly escalated his criminal activity. Originally at a loss for the correlation between the program and B's rising criminal behavior, a hospitalization in the later months of 2015 resulted in a diagnosis of Bipolar Disorder. After the initiation of treatment for this issue, B has had no further interactions with the legal system.

Participant C

Participant C is a white male, age of 54 with 46 adult appearances covering 37 years. C was one of the oldest participants with one of the most extensive criminal histories. Despite very serious charges in the early 1980's stemming from driving under the influence, resulting in manslaughter convictions, the majority of his subsequent criminal activity presented as trespassing, larceny, and resisting arrest charges. Suffering from mental health issues and homelessness, C was awaiting trial during the month before the treatment period began. RASOR officers successfully worked during this time to have C released into detox and subsequently into a residential treatment program to address his substance abuse issues. The efforts of the officers produced obvious results. Seeing C in Harvard Square two months into the treatment period, he appeared

clean and sober, profusely thanking the officers and stating that he had no desire to ‘give the police a hard time anymore.’ C has not reoffended and is now voluntarily living in a group home.

Participant D

Participant D is an African American male who entered the program at the age of 21. Adopted alongside his biological brother at an early age, D benefited from a supportive family unit. Described as a ‘very happy person,’ RASOR officers noted that D strove to be accepted and to be part of a group, but that this ultimately allowed ‘low-lives’ to prey upon him. With a multitude of juvenile and adult charges, including armed robbery, assault, and multiple larcenies, D’s affiliation with the ‘wrong crowd’ only encouraged the negative behavior. This was compounded by habitual marijuana use as a form of self-medication for Attention Deficit Disorder after suffering adverse effects from prescribed medication such as Ritalin. D was very receptive to the program and after coordination by RASOR officers, D happily (and successfully) participated in ROCA, an intensive intervention program with strong outcomes of low recidivism rates. Supported by this program and consistent efforts by the RASOR team to combat some early non-responsive behaviors, he received no new charges during the treatment period and expressed gratitude for the assistance of the officers.

These are only a select few of the participants in the program, but many of these participants were able to take advantage of the opportunity and resources made available to them to improve their lives. While reoffending was one of the measures examined, other measures to examine could include basic needs sustainability such as housing and employment. It is important to note that many of these participants, who had the highest social harm indexes in the communities, are chronic offenders and to change these types of pattern requires time, resources, and devoted personnel to continue community engagement.

Conclusion

After Cambridge PD led efforts to implement the regional data driven focused deterrence strategy, RASOR proved to have several impacts on its participants and the community. RASOR was a very unique approach to focused deterrence programs that included innovative roles for law enforcement, involved a collaboration of several jurisdictions, and relied on data-driven approaches and strategies to identify and target offenders.

The dedicated staff focused on the implementation design, role of case management, engagement practices, intensive monitoring, and establishing partnerships among various agencies. Coordinating rehabilitative services, attending court appearances, and providing basic needs assistance are only a few examples of the services provided throughout the treatment period.

Understanding the criminogenic nature of the participants, the engagement period to have offenders work with and trust staff has its challenges. RASOR staff emphasized the necessity of a ‘soft’ initial approach which was a non-threatening interaction to garner trust not only towards the engaging officer, but the program in general. As seen in the data results, engaging participants in case management and services requires not only a lot of effort but a great deal of time. Furthermore, that close working relationship helps explain why the treatment group in Cohort 1, with the most amount of time, showed significantly better results than those in the control group 1.

After evaluating RASOR, it is evident that focused deterrence strategies require support from criminal justice agencies, social service agencies, friends and family members of offenders, and other important actors in the community to assist in interrupting the path to chronic offending. Department “buy-in” and communication among all of the criminal justice partners becomes an integral part of any focused deterrence program’s success. It is nearly impossible to provide immediate post-release incarceration services to a participant when officers are not notified about an early release. A police case manager cannot assist in obtaining substance abuse help for a participant without the help of mental health workers. All agencies must work collectively to achieve the outcomes of focused deterrence strategies.

Although RASOR staff acknowledge the challenges of engaging this population, especially those individuals living with mental health problems, they remain committed to the success of the program. With collaboration and engagement from all levels of the criminal justice system and strong support among the agencies, a regional data driven approach to focused deterrence shows strong potential for successful implementation.

Summary of Methods and Findings

Methods

We conducted a block experiment design where 150 offenders were ranked on a “social harm” index generated from a database that combined information from three agencies, Cambridge, Everett, and Somerville Police Departments. Subjects were placed into three strata (cohorts) based on this social harm score and then randomly assigned to RASOR and control conditions. These cohorts entered the study in staggered intervals (4/9/2014, 8/12/2014, and 11/13/2014 respectively) and were followed until a fixed date (5/31/2015). After this, arrest data were used to determine the time to arraignment for a new offense for RASOR and control subjects in each cohort. We hypothesized that RASOR subjects would have a longer time to arraignment than control subjects. We used non-parametric and semi-parametric survival analyses to examine this hypothesis.

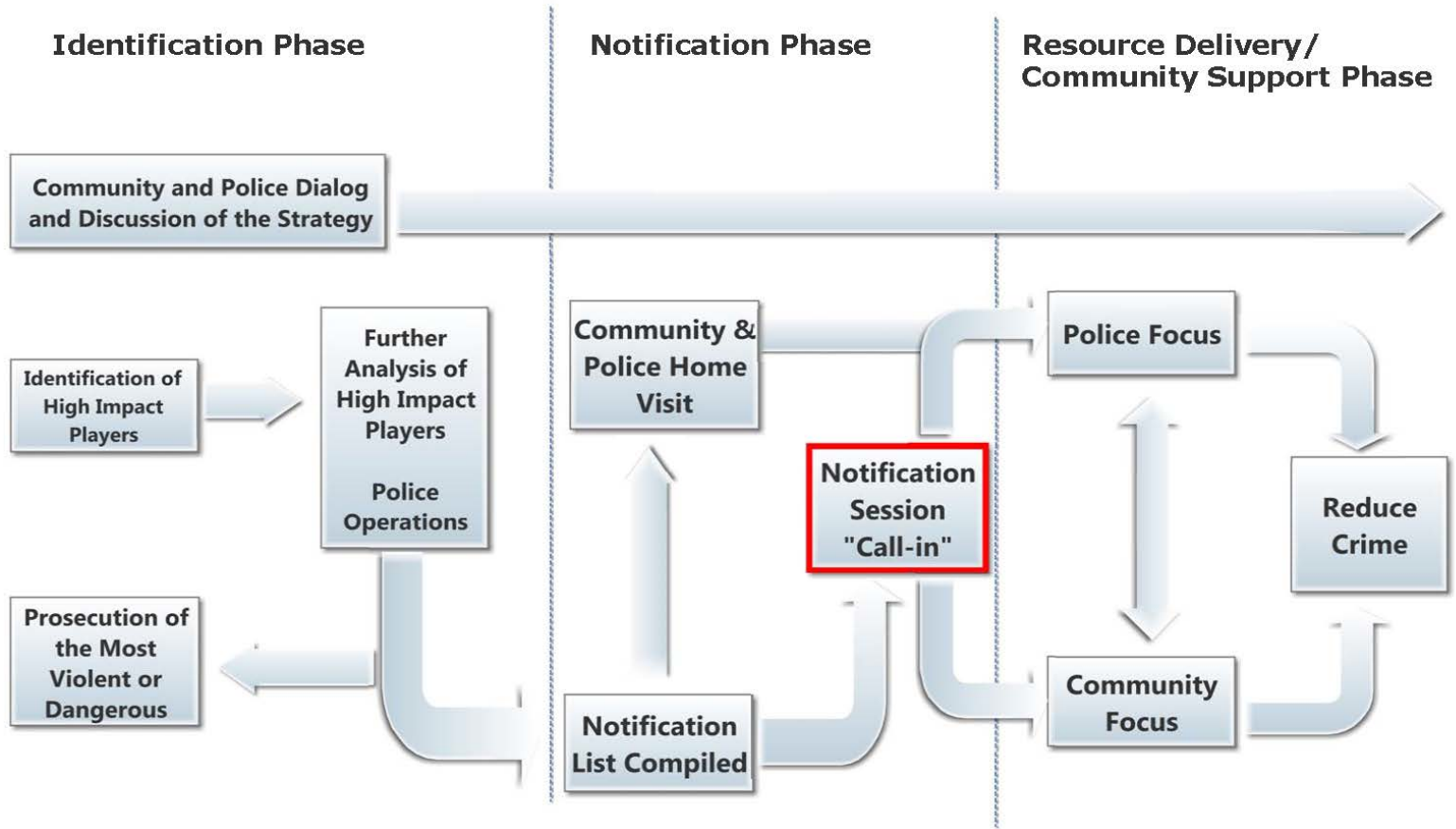
Findings

- There were no statistically significant differences in time to arraignment between RASOR and control subjects for the full sample.
- For cohort 1 the RASOR subjects had a longer time to arraignment compared to control subjects, but this difference was not statistically significant
- For cohort 2, the control subjects had a longer time to arraignment compared to RASOR subjects and this difference was statistically significant.
- For cohort 3, the control subjects had a longer time to arraignment compared to RASOR subjects, but this difference was not statistically significant.
- Supplementary analyses indicated that there were considerable differences in the amount of time spent per case between the cohorts. This may explain why the results from cohort 1 were more consistent with the hypothesized relationships.
 - Cohort 1 received on average 706.8 officer-minutes of case management time
 - Cohort 2 received on average 58.4 officer-minutes of case management time
 - Cohort 3 received on average 37.6 officer-minutes of case management time
- Future studies need to examine the relationship between dosage (case management time) and impact as this study suggests that the effectiveness of focused deterrence may be related to the amount of time spent by officers in case management activities.

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Appendix A



Adopted from Frabutt et. al. 2004