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RANDOMIZED EXPERIMENT IN NEW YORK CITY

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Can Gun Violence be Deterred at Low Cost? Evidence from a Randomized Experiment in New York City

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ABSTRACT

Can gun violence be deterred at low cost? We report the results of a randomized experiment of a messaging intervention which was designed to reduce gun violence among individuals under parole supervision with a prior violent felony conviction or firearm arrest. The intervention consisted of a group meeting in which high-risk paroled individuals were notified of the sanction they would face upon reoffending while being offered community resources to support reintegration into the community. The program did not lead to a reduction in gun violence or create community spillover effects but did reduce parole violations by 15%. Potential mechanisms and implications for similar programs are discussed.

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1. Introduction

In the United States, 71% of individuals leaving prison are re-arrested within five years of release ([Durose & Antenangeli, 2021](#)). Despite more than five decades of policy innovation and a large evaluation literature focused on this population – including some of the foundational workforce training field trials in labor economics ([Lalonde, 1986](#)) – today’s recidivism rate remains largely unchanged from that of the 1970s. The social science literature – including a large and growing literature in economics – broadly supports the deterrence value of investments in law enforcement ([Nagin, 2013](#); [Chalfin & McCrary, 2017](#); [Braga et al., 2019](#)) as well as the impact of social service supports such as mental health treatment ([Heller et al., 2017](#); [Jacome, 2021](#)), summer jobs ([Heller, 2016](#); [Davis & Heller, 2020](#); [Gelber et al., 2016](#)) and community nonprofits more generally ([Sharkey et al., 2014](#)). However, few approaches, including the provision of holistic “wraparound” services, have a demonstrated record of success in deterring the highest-risk offenders – especially those leaving prison – who drive an outsize share of the social costs of violence ([Doleac, 2021](#); [Doleac & LaForest, 2022](#)).

Efforts to control crime must accommodate the dual empirical regularities that crime is intensively concentrated among a small number of people ([MacDonald, 2023](#)) and places ([Weisburd, 2015](#)). The intensity of that concentration suggests that focused efforts to deter or incapacitate the highest-risk offenders have the potential to generate outsize and potentially scalable dividends. With respect to crime control, recent evidence has found that targeted enforcement efforts, which focus on particular high-risk individuals and groups, can be effective in reducing violent crime ([Groff et al., 2015](#); [Braga et al., 2019](#); [Chalfin et al., 2021](#); [Dominguez, 2021](#)). However, while launching long-term investigations to arrest the drivers of violence can lead to sizable impacts, the approach is difficult and expensive and requires a great deal of expertise and investigative know-how to pursue effectively. Many of the cities that have launched task forces or similar initiatives to investigate high-priority law enforcement targets have experienced few public safety benefits as political infighting and a lack of resources and expertise frequently cannibalize such efforts ([Kennedy, 2019](#)).

A different approach – one that is far less expensive and easier to implement – is to use the bully pulpit that law enforcement has in supervising individuals on parole to maximize the salience of law enforcement inputs (i.e., deterrence) among high-risk offenders. Relative to incapacitation, deterrence is cheap as crime victims avoid the costs of deterred crimes and social planners do not

have to finance the cost of court investigations and incarceration ([Chalfin and McCrary, 2017](#)). Even more importantly, inexpensive deterrence interventions offer the promise of delivering impacts at scale, a bar that a considerable majority of public safety interventions, even those which are effective among a subset of the population, are unable to meet due to high administrative costs and limitations inherent in the difficulty of scaling up complex and context-specific interventions ([Davis et al., 2017](#)).

This study tests the deterrence value of messaging – a relatively inexpensive approach which has been used to reduce costs and increase compliance with the criminal justice system in a variety of other contexts ([Cumberbatch & Barnes, 2018](#); [Fishbane et al., 2020](#); [Shah & LaForest, 2022](#); [Owens & Sloan, 2023](#)) – applied to a high-risk group of offenders in New York City. The intervention, which was carried out from 2013 to 2014, was inspired by the principles of “focused deterrence,” a violence reduction strategy implemented through the collaborative efforts of law enforcement along with community-based social service organizations ([McDevitt et al., 2006](#); [Decker et al., 2007](#); [Papachristos et al., 2007](#); [Braga et al., 2014](#); [Braga et al., 2019](#)). The approach begins with the engagement of law enforcement. High-risk individuals are identified through intelligence or proactive investigations and are notified that their activities have caused them to become a potential law enforcement target. Individuals are warned that law enforcement will be surveilling them, with an intensive focus on criminal activities that have a nexus to serious violence. Individuals are also made aware of community organizations that can offer them support in disengaging from a high-risk lifestyle.

Focused deterrence is inspired by several empirical regularities in crime and violence research. First, violence is concentrated to a considerable degree among a small number of individuals and groups – who are generally known to law enforcement – operating in a small number of communities within a city ([Weisburd, 2015](#); [MacDonald, 2023](#)). These individuals include both focal members of gangs – who have a sizable impact on gang violence – and recently released prisoners – who account for as many as 20% of all arrests for violent crimes ([Rosenfeld et al., 2005](#)). Second, most violence is interpersonal – and stems from disputes and arguments – which can easily spill over into community violence as community members step in to defend one another. Focused deterrence may thus have the ability to reduce the multiplier effect that gangs and community groups have on the social costs of an interpersonal dispute.

Finally, while most research suggests that the prospect of a long prison sentence generates only limited deterrence (Nagin, 2013; Chalfin & McCrory, 2017), a variety of evidence from economics, criminology and other social science disciplines suggests that crime – which is disproportionately driven by present-oriented offenders, does tend to be responsive to the certainty of apprehension (Nagin, 2013; Nagin et al., 2015). By engaging directly with the drivers of violence, successfully conveying the message that they are on the radar of law enforcement, and sending the message that law enforcement is knowledgeable about their activities, law enforcement can potentially double down on the deterrence impacts that they are able to generate with a fixed level of resources. In other words, messaging, if effective, can increase the amount of law enforcement capital, making each unit of labor more productive in the production of public safety.

Quasi-experimental evaluations of focused deterrence programs, largely leveraging matched comparison groups, have generally found positive impacts on the outcomes of individuals who take part in the programs as well as positive spillover effects for nearby individuals in the community who do not take part (Braga et al., 2019; Meares et al., 2009; McDevitt et al., 2006). However, there is still substantial uncertainty in the literature about the precise effects of these programs due to the quasi-experimental nature of prior evaluations and the prospect for there to be substantial selection with respect to which groups were designated to receive the intervention (Braga et al., 2019).

To date, there have been two randomized evaluations of focused deterrence interventions delivered to adult offenders: a study in Sacramento, CA by Ariel et al. (2019) and an evaluation of a program in St. Louis by Hamilton et al. (2018). The research by Ariel et al. (2019) found that an intensive effort in which police officers visited the homes of previously-arrested individuals to convey a message of future surveillance reduced re-arrests.² The other study – that of Hamilton et al. (2018) – was limited by an extremely small sample size and was thus highly underpowered to detect treatment effects at conventional levels of significance.³

This study reports the results of a randomized experiment of the efficacy of delivering low-

² A third randomized evaluation of messaging by Davis et al. (2023) studied a set of four forums delivered over a four-day period to incarcerated juveniles in Cook County, IL. This research also found sizable effects though there is accumulating evidence that it is easier to modify offending among youth who have not yet acquired a great deal of criminal capital, or de-valued their human capital, than adult offenders who have a lifetime of deficits to overcome (Doleac, 2021).

³ The sample size in Hamilton et al. (2018) was very small (112) and had a low take-up rate (63%), thus making it difficult to reach quantitative conclusions. The study found null effects, albeit with large standard errors.

cost and easy-to-implement deterrence-based messaging to high-risk adult offenders. Critically, our study investigates the effects of a particularly inexpensive and light touch intervention, which is relatively easy to replicate with fidelity and has the potential to scale well across jurisdictions, if effective. The intervention was intended to increase the salience of future penalties as well as emphasize the availability of social supports through a single meeting that parolees were required to attend. We study the direct effect of the messaging on individuals and, recognizing the potential salience of spillovers in place-based randomized experiments (Miguel and Kremer 2004), on community-based spillovers using a randomized experiment explicitly designed for this purpose. By identifying spillovers, we are able to estimate whether the intervention has an impact at scale for the community at large, the critical estimand for a social planner and an estimand that is ordinarily conspicuously absent from programmatic evaluations.

In contrast to much of the prior literature, we find that the intervention had no significant impact on future arrests among those receiving the intervention, no significant spillover effects to nearby parolees who were not required to attend a forum, and no significant impact on neighborhood crime. However, we do find evidence that the forums led to reductions in future parole violations among participants. These reductions were concentrated within the first six months after forum attendance, and represent a reduction of approximately 15% in total violations and 25% in absconding violations among program attendees. The findings thus suggest that while the intervention did not have the intended effects on gun violence or other types of serious offending, the messaging was successful in modifying at least some behaviors among high-risk offenders.

2. Background

Study

The New York State Gun Violence Reduction Project (GVRP) was implemented across five sites in New York State in 2013 and 2014. Each month, in each study site, recently-released parolees were required to attend a single “notification forum” as a condition of their supervision. Each forum was one hour long and included 15-20 paroled individuals. At each forum, attendees heard short presentations delivered by a series of speakers representing various arms of law enforcement, community members, and social service providers from within the attendees’

communities. Representatives of law enforcement explained that the goal of the program was to create safer communities by reducing firearm violence, shared recent incidents of violence in the community and the consequences of that violence, and reminded parolees of the harsh penalties they would face, personally, should they engage in gun crimes and gang violence in their community. This message of deterrence was complemented by motivational stories from formerly incarcerated individuals and encouragement from community members and social service providers to make on-the-spot connections to career counseling, drug treatment, and other community support services.

The New York State GVRP was implemented in five sites across the state of New York. Two sites were in Albany and Schenectady, mid-sized cities several hours north of New York City. The remaining three sites were in New York City, one each in the Bronx, Manhattan, and Brooklyn.⁴ Individuals living in these communities were eligible for the forums if they (1) were on parole, (2) had been convicted of a violent felony or had a prior firearm arrest, (3) did not have a mental health diagnosis or a prior sexual assault charge, (4) had lived within the targeted area for at least 30 consecutive days, and (5) had been released from prison within the prior two years. Forums took place between August 2012 and July 2014 in each of the five sites, with randomized assignment of attendees taking place between February 2013 and July 2014.

Treatment assignment involved two stages of randomization in each of the three NYC study sites. In a first stage of randomization, census tracts within each area were randomly assigned to a “neighborhood treatment group” and a “neighborhood control group.” Individuals who lived within the treated communities were eligible to be called in to the forums. Individuals in the control communities were not eligible to be called in. In the Albany and Schenectady sites, no neighborhood level randomization occurred due to the substantially smaller eligible populations in these cities. In these two sites, all eligible individuals were classified as belonging to the neighborhood treatment group.

In all five study sites, eligible individuals were then randomly assigned to an “individual treatment group” and an “individual control group.” Thus, in the NYC study sites, eligible individuals belonged to one of four study groups based on their combination of neighborhood treatment group and individual treatment group, as shown in Figure 1. Only individuals in both the neighborhood and individual treatment group (Group 1) were eligible to be called in to the

⁴ See Appendix A for further details on the five study sites.

forums. In the Albany and Schenectady sites, eligible individuals belonged to one of two study groups – an individual treatment group and an individual control group. In these sites, all individuals in the individual treatment group were eligible to be called in.

3. Data

In order to evaluate the effects of the notification forums, we received deidentified data on all study participants from the New York State Division of Criminal Justice Services (DCJS) and New York State Department of Corrections and Community Supervision (DOCCS) ([Division of Criminal Justice Services and Department of Corrections and Community Supervision, 2020](#)). DCJS provided criminal history for all individuals in the study from January 1990 through June 2019.⁵ Additionally, DCJS provided demographic information for all individuals in the study such as birth date, sex, race, and ethnicity. DOCCS provided parole violation data for the study population from August 2012 through June 2019, including type (e.g., a new arrest or a technical violation) and date of violation. DOCCS also provided information on whether, and when, each individual attended a parole forum.

We considered two categories of individual-level outcomes in the evaluation. The first was arrests, including both “all arrests” and “violent felony arrests.”⁶ The second set of outcomes was documented parole violations that lead to parole revocation and reincarceration. These measures include “all parole violations”, as well as individual subsets of violations pertaining to “violations due to a new arrest”, “violations due to absconding”, or “other technical parole violations.”⁷ In order to study the effects of the forums on neighborhood crime in the NYC sites, we used publicly-available New York City Police Department (NYPD) crime data from the NYC OpenData portal ([New York City Police Department, 2019](#)). These data included the date and location of all crime

⁵ The data contained event-level criminal histories of adult fingerprintable arrests (i.e., felony and misdemeanor charges) and included information on the arrest date, arrest charges, arraignment-related prosecutorial and judicial actions, and relevant sentencing decisions. The data included both sealed and unsealed arrest records.

⁶ Although the program was established to target firearm violence specifically, there were too few weapons-related offenses to use this measure as an outcome.

⁷ “Absconding parole violations” occur when parolees do not report to their parole officers, change their addresses without approval, or move such that their whereabouts are unknown to their parole officers. “Other technical violations” occur when parolees break other individual-specific conditions of parole such as curfew, restrictions on drug and alcohol consumption, or restrictions on social contacts, such that DOCCS revokes their parole.

complaints, arrests, and shootings reported to the NYPD between July 2010 and June 2019.⁸ Complaint and arrest data include all valid felony, misdemeanor, and violation crimes and arrests.

The two stages of randomization provide assurance that individuals in the treatment group and control group should be balanced on both observable and unobservable characteristics. Table 1 presents the mean characteristics of sample members for the full samples used for the analysis broken down by treatment group, as well as the results of balance tests. Across all characteristics, including age, race/ethnicity, gender, number of arrests prior to entrance into the sample, number of prior arrests with a weapons charge, and number of prior arrests for violent felonies, there is no evidence to suggest that randomization resulted in any significant differences between the individual-level treatment and control groups.⁹ With respect to sample demographics, the average age of parolees was 37 and nearly all were male. Two-thirds were Black, 20% were Hispanic, and 10% were non-Hispanic white. Despite previously spending time in prison, the average parolee in the sample had been previously arrested 11 times, of which on average two were violent felonies.

3. Research Design

Leveraging the two stages of randomization, we conduct four analyses. The first analysis is used to estimate the effects of participation in the program on individual-level outcomes. For this analysis, the outcomes of individuals in Group 1 (individuals living in treatment neighborhoods who were randomly assigned to the individual treatment group) are compared with individuals in Group 2 (individuals living in treatment neighborhoods who were randomly assigned to the individual control group) across all five study sites. Our estimate for the effect of forum attendance on individual-level outcome Y_i is given by γ in Equation 1:

$$Y_i = \alpha + \beta X_i + \gamma(Group_1)_i + \theta_b + \varepsilon_i \quad , \quad (1)$$

$$\forall i \in Group_1, Group_2 \quad .$$

⁸ The NYPD datasets included the XY coordinate of the incident, which was then mapped to census tract in order to determine whether the incident occurred in a treatment group or control group neighborhood. 0.3% of complaints are missing location data, while no documented arrests or shootings are missing location data.

⁹ While this implies that there is no need to control for these potential differences in our models, we ultimately included these demographic and criminal characteristics as controls in the regressions to maximize the precision of our estimates and adjust for slight imbalances that were present across the treatment and control groups.

In (1), Y_i is the outcome of interest for individual i , an indicator of whether the individual was arrested or committed a parole violation during the post-intervention observation period.¹⁰ X_i is a vector of participant characteristics, including race/ethnicity, age and squared age, gender, number of arrests prior to entrance into the sample (and squared number of prior arrests), and the month in which the individual entered the sample. θ_b is a set of fixed effects for the block in which each neighborhood was classified.¹¹ The primary variable of interest is the indicator for the individual's treatment status, labeled "*Group*₁," which takes a value of one if the individual is a member of Group 1 and zero otherwise. Control variables in the model adjust for any incidental differences in characteristics of the sample population that might be associated with outcomes and are included to improve the precision of the estimates.

Our primary results estimate the "intent-to-treat" (ITT) effect of being invited to attend a forum as opposed to the effect of actually attending a forum. This method provides the most realistic estimate of how the program affects those targeted for the program, in practice, as not all those who are invited to a forum will actually attend. For the New York GVRP, 81% of individuals assigned to the treatment group ultimately attended, while 14% of individuals assigned to the control group attended. In order to adjust for treatment non-compliance, we also compute "treatment on the treated" (TOT) estimates where the ITT estimate is scaled up by the compliance rate with the intervention. As shown in Appendix Table A2, TOT estimates lead to the same substantive conclusions about the effects of the program as the ITT estimates.

This initial analysis leverages the full sample population across all five program sites and provides the most basic assessment of whether being called in to attend a forum affects an individual's outcomes. However, the analysis relies on the assumption that the forums have no effect on individuals who live in treatment neighborhoods who are not called in to attend the forums—in other words, that there is no geographic or social spillover effect that spreads beyond those who personally attend. We thus report the results of a second analysis which estimates the effect of the forums that is less vulnerable to bias from potential spillover effects. For this analysis the outcomes of individuals in Group 1 (individuals living in treatment neighborhoods who were

¹⁰ For individuals in the treatment group, the outcome period starts at the date of expected forum attendance. For individuals in the control group, it starts at the date of the forum the individual would have been expected to attend had they been assigned to the treatment group.

¹¹ Blocks are groups of neighborhoods that were created within the five program sites, prior to randomization, and which had roughly similar numbers of eligible individuals.

randomly assigned to the individual treatment group) are compared with individuals in Groups 3 and 4 (individuals living in the control neighborhoods), among the NYC study sites. The equation for this analysis is the same as Equation 1 except that the control group consists of individuals in Groups 3 and 4 instead of Group 2:

$$Y_i = \alpha + \beta X_i + \gamma(\text{Group}_1)_i + \theta_b + \varepsilon_i \quad , \quad (2)$$

$$\forall i \in (\text{Group}_1, \text{Group}_3, \text{Group}_4) \cap (\text{Bronx}, \text{Brooklyn}, \text{Manhattan}) \quad .$$

Because individuals in treatment group neighborhoods who were not called in to the forums are excluded from this analysis, there is less of a threat that a diffusion of the forum's message across the community could contaminate the estimated program impact.

The third analysis explicitly tests for the existence of community spillovers caused by the forums. This analysis compares re-offending among individuals in Group 2 (individuals living in treated neighborhoods who were randomly assigned to the individual control group) with the outcomes of individuals in Groups 3 and 4 (individuals living in control neighborhoods), among the NYC study sites. The estimate, derived from Equation 3, thus provides an indication of whether there is an effect of living in a neighborhood where others were being called in to attend the forums, among individuals who were not called in to attend the forums:

$$Y_i = \alpha + \beta X_i + \gamma(\text{Group}_2)_i + \theta_b + \varepsilon_i \quad , \quad (3)$$

$$\forall i \in (\text{Group}_2, \text{Group}_3, \text{Group}_4) \cap (\text{Bronx}, \text{Brooklyn}, \text{Manhattan}) \quad .$$

Finally, we report the results of a fourth analysis which estimates the program's impact on neighborhood-level crime. Because neighborhoods in the NYC sites were randomly assigned to neighborhood treatment and control groups, we can estimate the effect of the program on violent crime at the neighborhood level and, as such, comment on whether the intervention led to a reduction in crime at scale. The effect of the forums on neighborhood-level outcome Y_j is given by γ in Equation 4:

$$Y_j = \alpha + \beta X_j + \gamma(\text{Treatment})_j + \theta_b + \varepsilon_j \quad , \quad (4)$$

$$\forall j \in \text{Bronx}, \text{Brooklyn}, \text{Manhattan}$$

In (4), Y_j is the outcome of interest (total crime complaints, arrests, or shootings during the study period) in neighborhood j . X_j is a vector of neighborhood controls, including the total population and the lagged outcome measure over the year prior to the study. θ_b is a set of fixed effects for the neighborhood “block,” and the primary variable of interest is the indicator for the neighborhood treatment status, labeled “*Treatment*,” which takes a value of one if the neighborhood is in the treatment group and zero otherwise.

4. Results

We begin by estimating the effect of the forums by comparing treatment and control individuals living in treatment group neighborhoods (Groups 1 and 2, respectively). The results of this analysis are shown in Table 2. There is little evidence that the forums reduced rearrest rates among individuals who were randomly assigned to attend a forum. The estimates show that a similar share of individuals (23%) who were and were not invited to the forums were re-arrested within six months. The point estimate was -0.6 percentage points and the corresponding 95% confidence interval ranged from -3.7 to +2.5 percentage points. Based on these estimates, we conclude that the forums did not have a sizable effect on rearrests. We observe a similar lack of effect on violent felony arrests over the first six months after forum attendance. However, the estimates suggest that the forums did reduce parole violations. Parole violations, within six months of forum invitation, were reduced by approximately three percentage points, a 15% decrease in parole violations. Disaggregating parole violations by type, the results appear to be driven by a notable reduction in absconding violations – an invitation to the forums appears to decrease absconding parole violations by approximately two percentage points, or 25% from a base rate of

10%, in the first six months after forum attendance. These results are similar across both short- and long-term outcome periods.^{12,13,14}

Community Effects

Prior research on focused deterrence has shown the potential for there to be community spillover effects from those who were treated to their untreated peers (Braga et al., 2014; Braga et al., 2019). Even though we do not observe a reduction in crime among treated individuals, it remains possible that the intervention could have generated spillover effects among potentially more deterrable peers. Table 3 investigates these effects. Analysis 2 estimates the effects of the intervention, net of spillover effects, by comparing outcomes between individuals who were invited to the forums (Group 1) and individuals living in neighborhoods where no one was invited to the forums (Groups 3 and 4), across NYC sites. Results from this analysis are appreciably similar to estimates from Analysis 1, albeit with a slightly lower level of statistical power, implying that any community spillover effects are small at most. Analysis 3 directly estimates spillover effects by comparing the impact of the forums on individuals living in treated neighborhoods who did not personally attend the forums (Group 2), to individuals who lived in control neighborhoods (Groups 3 and 4), across NYC sites. Here too, we observe no difference in outcome for these two sets of individuals, providing additional evidence that the forums did not appear to produce community spillovers.

Finally, we estimate the impact of the forums on neighborhood-level crime. Table 4 shows the estimated effect of the forums when comparing census tracts randomized to treatment

¹² As shown in Appendix Table A1, our results are broadly consistent when measured over the first 3, 6, 12, and 24 months after forum attendance, though there are some suggestive signs that the effects of the forums on parole violations dissipate in the 12 and 24 month outcome periods. Results are also broadly consistent, albeit with slightly stronger effect sizes, when we measure the treatment of the treated effect, as shown in Appendix Table A2.

¹³ Since prior research has shown that criminal activity typically decreases with age, we estimated the effect of the forums among parolees under the age of 30 at the time of arrest. Considering that the forums were explicitly aimed at reducing gun violence, we also estimated the effect of the forums among parolees with at least two prior weapons charges. As shown in Appendix Table A3, we found no difference in effect of the forums for these two populations compared to the effects we observed for the entire sample with one exception – we found suggestive evidence that the forums reduced parole violations related to new arrests among parolees with at least two prior weapons charges, in the first six months after forum attendance.

¹⁴ To account for differences in program sites and the resulting potential variation in the program's impact, we estimated the effects of the forums separately for each individual study site. As shown in Appendix Table A4, we found no notable difference in effect across study sites.

neighborhoods to those tracts randomized to control neighborhoods across NYC sites.¹⁵ Results from this analysis suggest that there was no notable effect on crime in treatment neighborhoods. Specifically, we estimate a 95% confidence interval for the difference in crime complaints between treatment neighborhoods and control neighborhoods of between -3% and 5%. Based on these results we conclude that the forums appear to have had little to no impact on neighborhood-level criminal activity.

5. Discussion

We estimate the effect of the New York State Gun Violence Reduction Project on individuals called in to attend notification forums, on individuals in targeted neighborhoods who were not called in to attend the forums, and on neighborhood-level crime in the communities where the intervention took place. The intervention is built upon the innovative and provocative idea that the deterrence value of law enforcement can be maximized through messaging. Since messaging is inexpensive relative to alternative interventions, such a program, if successful, holds extraordinary promise in an era of shrinking police departments and reduced tolerance for the potentially high social costs of policing. Since paroled individuals are also particularly easy to identify, and the forums are relatively straightforward to conduct, such a program is also simple to replicate with fidelity and scale across jurisdictions.

The evaluation highlights three findings. First, there is no detectable effect of the intervention on subsequent arrests. This is true both in the short-term and also over the longer-term. Second, individuals who were called in to attend the notification forums were substantially less likely to violate their parole, an effect driven primarily by reductions in violations due to absconding. Our main estimates show that being called in to attend a forum reduced the probability of a subsequent parole violation that led to parole revocation during the first six months of parole by three percentage points, a reduction of 15% in parole violations relative to the control group, and reduced the probability of an absconding parole violation by two percentage points, a reduction of 25% relative to the control group. Based on these results it appears that the forums increased the salience of punishment for violating parole conditions, even if they did not appear to have an impact on future arrests.

¹⁵ Only census tracts with at least five study participants are included in these models.

Finally, we observe no evidence of an effect of the program on the behavior or criminal activity of individuals who lived in the neighborhoods where the program was implemented but did not take part in the forums. Changes in behavior were found only among individuals who attended the forums, and, even then, only in terms of their propensity to commit parole violations. Similarly, we found no impact of the forums on neighborhood-level crime, a finding which was made possible by a research design that involved the randomization of neighborhoods as well as the randomization of individuals within those neighborhoods to determine forum invitees.

These findings contradict the quasi-experimental results from similar deterrence-based messaging programs as well as from the only other well-powered randomized experiment of deterrence-based messaging. Some potential mechanisms are worth discussing. First, at the time of the study, New York parolees attended a single forum. It is possible that this was not a sufficient dosage to produce a notable reduction in arrests, and that increasing the number of forums attended may have a larger impact. Second, we were unable to measure how these forums impacted social service take-up, or how the combined messages of deterrence and community support conveyed at the forums may have complimented each other. For example, later iterations of the notification forums that continued in Manhattan after the conclusion of this study placed a greater emphasis on social services and community support. It is possible that this updated messaging had a greater impact on parolees' abilities to successfully transition back to their communities after release, and may have impacted future arrests. Third, it is possible that the null effect we observed was due to selecting parolees as the population of interest. Paroled individuals interact with the criminal justice system on a day-to-day basis, have interacted with the system constantly while incarcerated over the past several years, and thus may already have a strong understanding of the swift, certain, and severe punishments they will receive if they are arrested for future violent crimes. As such, these notification forums may do relatively little to increase the already substantial awareness of parolees about potential future sanctions or change their perceptions of the legitimacy of the procedural justice system.

A final hypothesis worth discussing is that the null effect of the forums on observed rearrests could be due, mechanically, to the reductions in absconding among treated individuals. As a proportion of absconders flee New York State, and those that are identified are reincarcerated, individuals who commit parole violations are no longer present to commit new crimes in New York State. Likewise, as treated parolees commit fewer parole violations, a larger proportion of

them is living in their community at any given time and available to be arrested relative to individuals in the control group. While we cannot say what proportion of individuals who are reincarcerated for parole violations or abscond to other states would have been arrested for new crimes had they remained in the community, we can bound this percentage. For example, recall that we found point estimates on the effects of the forums on new crimes and parole violations of -0.6 percentage points (not significant) and -2.4 percentage points (significant), respectively. As a lower bound, assume that the additional absconders in the control group would have committed new crimes at the rate of other parolees. As an upper bound, assume that the extra absconders in the control group would have all committed new crimes had they not absconded. Using a back-of-the-envelope calculation, if parole violation rates had not changed between the treatment and control groups we would expect to find a point estimate for the effect of the forums on future arrests between -1.2 percentage points (still insignificant at the 95% level) and -3.0 percentage points (insignificant but close to significant at the 95% level). As such, even under the strongest assumptions about whether parole violators would have committed new crimes had they remained in the community, we conclude that the intervention did not have a sizable impact on future arrests.

Although the New York GVRP did not generate a measurable impact on arrests, it did generate a substantial impact on parole violations. While this was an unanticipated outcome from the program, the effect on absconding violations may imply that the forums caused attendees to develop a stronger line of communication with parole officers, or made them more aware of the severity of punishment for not attending one's scheduled meeting with a parole officer. It also bears mentioning that while an individual often makes an explicit choice to violate a parole condition, the decision to commit a crime may be more impulsive. Likewise, the antecedent conditions for violence like gun carrying may be driven, to an extent, by the neighborhood conditions to which a parolee was returning. As the saying goes, some individuals may have figured that, even with a higher chance of being caught, it is better to be caught with a gun than without one.

Even without a reduction in arrests, there is value in a reduction in parole violations attributed to the program, particularly if it can be accomplished through a low-cost intervention like notification forums. For example, while New York State's incarcerated population has decreased in recent years, the number of New Yorkers that are detained for parole violations has remained relatively steady ([Division of Criminal Justice Services, 2019](#)). In New York City in

particular, those reincarcerated for technical parole violations increased by 18% in recent years, even while the total jail population has decreased by 23% (O'Brien & Gallea, 2019). The racial disparities in the corrections and community supervision system (and the criminal legal system generally) means that the reincarceration of parolees for violations falls disproportionately on Black and Hispanic communities. Black and Hispanic individuals are more likely than white individuals to be on parole in New York State, and together account for over two-thirds of the returns to custody for parole violations (49.6% and 19.2%, respectively) (New York State Department of Corrections and Community Supervision, 2014). Even during the COVID-19 pandemic, when New York City aimed to vastly decrease the jail population and jail admissions for health and safety reasons, over 7.5 times as many Black individuals were detained for technical parole violations than white individuals (Vera Institute of Justice, 2021). While efforts to reduce the reincarceration of parolees for violations likely also requires broader policy shifts, it is worthwhile to note that the notification forums may have provided parolees with the relevant support, incentives, or encouragement to abide by the conditions of their parole. Reductions in parole violations as a result of the notification forums seen in this analysis could ultimately lead to significantly fewer individuals admitted to prison each year, even though we did not see direct effects of this program on rearrests.

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Tables and Figures

Figure 1 – Classification of eligible individuals by neighborhood and individual treatment status

		Neighborhood-Level Randomization	
		Treatment	Control
Individual-Level Randomization	Treatment	Group 1	Group 3
	Control	Group 2	Group 4

Table 1 – Summary Statistics

Covariate	All Sites			NYC Sites			
	Group 1	Group 2	<i>p-value</i>	Group 1	Group 2	Group 3	Group 4
Age	36.57	37.01	0.34	36.50	36.76	36.92	37.16
Male	0.95	0.96	0.24	0.95	0.96	0.96	0.97
Race/Ethnicity							
African - American	0.68	0.68	0.97	0.69	0.68	0.66	0.65
Hispanic	0.21	0.22	0.71	0.27	0.28	0.30	0.31
Other	0.1	0.1	0.94	0.03	0.03	0.04	0.03
Prior Arrests							
All	11.39	11.53	0.76	12.08	12.11	12.32	12.59
Violent Felonies	2.31	2.34	0.72	2.60	2.59	2.59	2.82
Weapon - Related	1.16	1.24	0.20	1.33	1.40	1.27	1.35
N	1101	1054	-	751	738	710	692
F-test	-	-	0.32	-	-	-	-

Notes:

This table reports covariate means for various demographic characteristics by treatment group at the time of sample entry. It also reports p-values for t-tests on the difference between covariate means for treatment and control individuals in treatment group neighborhoods (groups 1 and 2) across all five sites, and the p-value on a joint test of the significance of the full set of these covariates in predicting treatment.

Table 2 – Effect of Parole Forums

	Control mean	Treatment Effect (%)	(pp)	Std Error
All Arrests	0.23	-3%	-0.01	0.02
Violent Felony Arrests	0.04	-8%	0.00	0.01
All Violations	0.21	-15%	-0.03 **	0.02
New Arrest Violations	0.08	-12%	-0.01	0.01
Absconding Violations	0.10	-25%	-0.02 **	0.01
Technical Violations	0.04	5%	0.00	0.01

Notes:

This table reports estimates from Analysis 1 on the impact of the notification forums. Regressions control for the number of eligible individuals in the neighborhood, city block fixed effects, race / ethnicity, gender, age (and squared age), the total number of prior arrests (and squared prior arrests), prior violent felony arrests, prior weapon arrests, and sample entrance month fixed effects. Standard errors are clustered at the census tract level. Outcomes are over the first six months after projected forum attendance date. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table 3 – Spillover Effects

	Control mean	Treatment Effect (%)	(pp)	Std Error
Analysis 2 - Individual Treatment & Neighborhood Treatment				
Relative to Neighborhood Control				
All Arrests	0.24	7%	0.02	0.02
Violent Felony Arrests	0.03	8%	0.00	0.01
All Violations	0.18	-7%	-0.01	0.02
New Arrest Violations	0.06	18%	0.01	0.01
Absconding Violations	0.10	-20%	-0.02	0.02
Technical Violations	0.02	-33%	-0.01	0.01
Analysis 3 - Individual Control & Neighborhood Treatment				
Relative to Neighborhood Control				
All Arrests	0.24	1%	0.00	0.02
Violent Felony Arrests	0.03	72%	0.02 *	0.01
All Violations	0.18	1%	0.00	0.02
New Arrest Violations	0.06	23%	0.01	0.01
Absconding Violations	0.10	-7%	-0.01	0.02
Technical Violations	0.02	-48%	-0.01	0.01

Notes:

Panel 1 reports results from Analysis 2 and Panel 2 reports results from Analysis 3. Regressions control for the number of eligible individuals in the neighborhood, city block fixed effects, race / ethnicity, gender, age (and squared age), the total number of prior arrests (and squared prior arrests), prior violent felony arrests, prior weapon arrests, and sample entrance month fixed effects. Standard errors are clustered at the census tract level. Outcomes are over the first six months after projected forum attendance date. N = 2152 for Analysis 2 and N = 2140 for Analysis 3. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table 4 – Neighborhood-Level Crime

	Control mean	Treatment Effect (%) (count)		Std Error
All Arrests	640.4	-1%	-3.9	24.4
Weapon-Related Arrests	25.6	-3%	-0.7	1.5
All Crime Complaints	648.1	1%	4.6	13.8
Weapon-Related Crime Complaints	21.7	-5%	-1.2	1.3
All Shootings	3.1	5%	0.2	0.5

Notes:

This table reports estimates from Analysis 4 on the impact of the notification forums on community-level crime. Observations are at the census-tract level. Regressions control for the lagged outcome in the year prior to the intervention, the total population, and city block fixed effects. The sample consists of all census tracts with at least five sample members. Outcomes are over a two-year period between February 2013, when the randomized assignment began, and January 2015, seven months after the last forum was conducted. N = 171. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Appendix

Appendix A – Study Sites

Maps of the five program sites are shown in Figure A1. In Manhattan, the 23rd and 25th police precincts were selected originally for the intervention. However, the program target area was expanded to include the 28th and 32nd precincts in order to generate a larger population of eligible individuals. In the Bronx site, the 40th precinct was selected as the target area for the intervention, but the area was expanded to include the 41st, 42nd, and 44th precincts. In the Brooklyn site, the 73rd precinct was selected as the original target area, but this area was expanded to include the neighborhoods immediately surrounding the 73rd precinct that are located in the 67th, 71st, 77th, 81st, 83rd, and 75th precincts (eligible neighborhoods are shaded in the map for the Brooklyn site).

In the maps for all of the New York City sites, census tracts that are shaded in green represent the set of neighborhoods in the neighborhood control group, and tracts that are shaded in red represent the set of neighborhoods in the neighborhood treatment group. Tracts that have a darker shade of red or green are those containing the largest number of eligible individuals (21 or more individuals), tracts with slightly lighter shades of green and red contain between 2 and 20 individuals, and tracts with the lightest shades of red and green contain only 1 individual. There are a few census tracts in each map that are within the target area for the intervention but are not shaded. These are census tracts that either had no eligible individuals living within them, or were not included in the study because the particular tract had no available matched tract with a similar number of eligible individuals. In either case, no individuals were selected for the working sample from these census tracts.

In the maps for the Albany and Schenectady sites, the eligible areas included the entire counties (shaded gray). In these sites, individuals were randomly assigned to the treatment or control groups but there was no randomization of neighborhoods. For this reason, all tracts are shaded in the same color, blue, rather than into red and green tracts. The lightest shaded tracts in the Albany and Schenectady sites have only 1 individual in the sample, and the darkest shaded tracts have at least 21 or more.

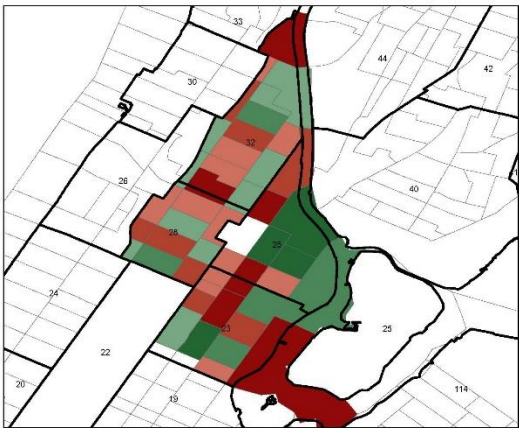
Figure A1. Locations of the five program sites and volume of participants by census tract.



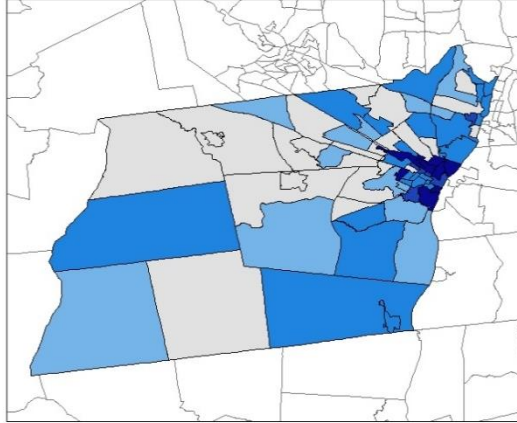
The Bronx site



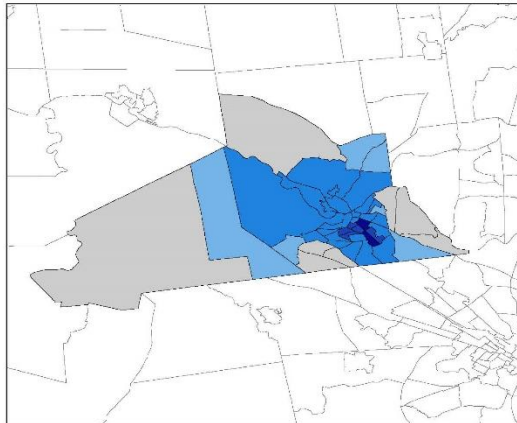
The Brooklyn site



The Manhattan site



The Albany site



The Schenectady site

Appendix B – Robustness Checks and Subgroup Effects

Table A1: Results by Outcome Period

	Control mean	Treatment Effect (%) (pp)		Std Error
All Arrests	0.23	-3%	-0.01	0.02
Within 3 months	0.12	-1%	0.00	0.01
Within 6 months	0.23	-3%	-0.01	0.02
Within 1 year	0.35	4%	0.01	0.02
Within 2 years	0.52	-1%	-0.01	0.02
Violent Felony Arrests				
Within 3 months	0.02	10%	0.00	0.01
Within 6 months	0.04	-8%	0.00	0.01
Within 1 year	0.06	11%	0.01	0.01
Within 2 years	0.11	1%	0.00	0.02
All Violations				
Within 3 months	0.14	-13%	-0.02	0.01
Within 6 months	0.21	-15%	-0.03 **	0.02
Within 1 year	0.31	-8%	-0.03	0.02
Within 2 years	0.40	-9%	-0.04 *	0.02
New Arrest Violations				
Within 3 months	0.05	-13%	-0.01	0.01
Within 6 months	0.08	-12%	-0.01	0.01
Within 1 year	0.12	-2%	0.00	0.01
Within 2 years	0.18	-10%	-0.02	0.02
Absconding Violations				
Within 3 months	0.06	-30%	-0.02 **	0.01
Within 6 months	0.10	-25%	-0.02 **	0.01
Within 1 year	0.13	-16%	-0.02	0.01
Within 2 years	0.17	-17%	-0.03 *	0.02
Technical Violations				
Within 3 months	0.03	19%	0.01	0.01
Within 6 months	0.04	5%	0.00	0.01
Within 1 year	0.07	-3%	0.00	0.01
Within 2 years	0.09	12%	0.01	0.01

Notes:

This table reports estimates from Analysis 1 on the impact of the notification forums. Regressions control for the number of eligible individuals in the neighborhood, city block fixed effects, race / ethnicity, gender, age (and squared age), the total number of prior arrests (and squared prior arrests), prior violent felony arrests, prior weapon arrests, and sample entrance month fixed effects. Standard errors are clustered at the census tract level. N = 2155. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table A2: Treatment on the Treated Effects

	Control mean	Treatment Effect		Std Error
		(%)	(pp)	
All Arrests	0.23	-4%	-0.01	0.02
Violent Felony Arrests	0.04	-11%	0.00	0.01
All Violations	0.21	-23%	-0.05 **	0.03
New Arrest Violations	0.08	-19%	-0.02	0.02
Absconding Violations	0.10	-38%	-0.04 **	0.02
Technical Violations	0.04	8%	0.00	0.01

Notes:

This table reports estimates from Analysis 1 on the impact of the notification forums, using two-stage least squares analysis where forum invitation is used as an instrument for forum attendance. Regressions control for the number of eligible individuals in the neighborhood, city block fixed effects, race / ethnicity, gender, age (and squared age), the total number of prior arrests (and squared prior arrests), prior violent felony arrests, prior weapon arrests, and sample entrance month fixed effects. Standard errors are clustered at the census tract level. Outcomes are over the first six months after projected forum attendance date. N = 2155. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

While we were not able to directly measure weapons arrests in the CCH file provided by DCJS, we constructed a proxy measure using a flag indicating firearms arrests, supplemented with top charges that correspond to NYS Penal Laws 265 (criminal possession of firearms and dangerous weapons), 120.05 subsection 2 (second degree assault by means of a deadly weapon or dangerous instrument), and 120.14 subsection 1 (menacing in the second degree by displaying a deadly weapon or dangerous instrument) and the NIBRS code for weapon law violations (520).

Table A3: Effects of Parole Forums on Specific Subgroups

	Control mean	Treatment Effect (%)	(pp)	Std Error
Under Age 30				
All Arrests	0.27	4%	0.01	0.03
Violent Felony Arrests	0.05	48%	0.03	0.02
All Violations	0.24	5%	0.01	0.03
New Arrest Violations	0.11	5%	0.01	0.03
Absconding Violations	0.09	-5%	-0.01	0.02
Technical Violations	0.03	56%	0.02	0.02
At Least Two Prior Weapons Charges				
All Arrests	0.25	9%	0.02	0.03
Violent Felony Arrests	0.05	-2%	0.00	0.02
All Violations	0.22	-20%	-0.04	0.03
New Arrest Violations	0.10	-44%	-0.04 **	0.02
Absconding Violations	0.10	-19%	-0.02	0.02
Technical Violations	0.03	38%	0.01	0.01

Notes:

This table reports estimates from Analysis 1 on the impact of the notification forums for two specific subgroups - individuals under the age of 30, and individuals with at least two prior weapons charges. Regressions control for the number of eligible individuals in the neighborhood, city block fixed effects, race / ethnicity, gender, age (and squared age), the total number of prior arrests (and squared prior arrests), prior violent felony arrests, prior weapon arrests, and sample entrance month fixed effects. Standard errors are clustered at the census tract level. Outcomes are over the first six months after projected forum attendance date. N = 694 for the "under age 30" regressions, and N = 686 for the "at least two prior weapons charges" regressions. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Site Specific effects are reported in Table A4. Due to the small sample sizes of individual program sites, the estimated effects here are imprecisely estimated and results should be interpreted cautiously. Still, despite variation in the direction of effect, likely stemming from these smaller sample sizes, most of the estimates for the various outcomes are statistically insignificant and relatively similar in size to the full-site estimates.¹⁶

¹⁶ Note that while the effect of the forums on future arrests at the Manhattan site is statistically significant, none of the other outcomes—in either Manhattan or the other four sites—are statistically significant at this level. As such, we do not consider this result to be robust and caution against drawing conclusions from it.

Table A4: Effects of Parole Forums by Site

	Control mean	Treatment Effect (%) (pp)		Std Error
Albany				
All Arrests	0.16	1%	0.00	0.04
Violent Felony Arrests	0.05	-45%	-0.02	0.02
All Violations	0.30	-13%	-0.04	0.06
New Arrest Violations	0.11	-22%	-0.03	0.03
Absconding Violations	0.10	-10%	-0.01	0.03
Technical Violations	0.11	-6%	-0.01	0.03
Bronx				
All Arrests	0.31	-22%	-0.07 **	0.03
Violent Felony Arrests	0.03	0%	0.00	0.01
All Violations	0.19	-6%	-0.01	0.03
New Arrest Violations	0.08	-17%	-0.01	0.02
Absconding Violations	0.09	14%	0.01	0.02
Technical Violations	0.02	-45%	-0.01	0.01
Brooklyn				
All Arrests	0.22	7%	0.02	0.03
Violent Felony Arrests	0.06	-32%	-0.02	0.02
All Violations	0.17	-13%	-0.02	0.04
New Arrest Violations	0.07	12%	0.01	0.02
Absconding Violations	0.09	-45%	-0.04	0.03
Technical Violations	0.01	67%	0.01	0.01
Manhattan				
All Arrests	0.22	29%	0.06 *	0.03
Violent Felony Arrests	0.03	84%	0.03	0.02
All Violations	0.17	-18%	-0.03	0.02
New Arrest Violations	0.05	16%	0.01	0.02
Absconding Violations	0.11	-36%	-0.04	0.02
Technical Violations	0.02	39%	0.01	0.01
Schenectady				
All Arrests	0.21	-26%	-0.05 *	0.03
Violent Felony Arrests	0.01	0%	0.00	0.01
All Violations	0.30	-20%	-0.06	0.05
New Arrest Violations	0.14	-24%	-0.03	0.03
Absconding Violations	0.09	-57%	-0.05	0.03
Technical Violations	0.07	38%	0.03	0.04

Notes:

This table reports estimates from Analysis 1 on the impact of the notification forums by study site. Regressions control for the number of eligible individuals in the neighborhood, city block fixed effects, race / ethnicity, gender, age (and squared age), the total number of prior arrests (and squared prior arrests), prior violent felony arrests, prior weapon arrests, and sample entrance month fixed effects. Standard errors are clustered at the census tract level. Outcomes are over the first six months after projected forum attendance date. N = 418 for Albany, N = 534 for the Bronx, N = 508 for Brooklyn, N = 447 for Manhattan, and N = 248 for Schenectady. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.