A Randomized Controlled Trial Evaluating the Effect of the New York State Gun Violence Reduction Program

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Abstract

In this paper we study the impact of the Gun Violence Reduction Project (GVRP), a targeted intervention designed to reduce involvement with gun violence among parolees with a violent felony conviction or firearm arrest. Based on a unique partnership between New York State, multiple law enforcement agencies, and a variety of social service providers, the GVRP led monthly group meetings (also known as "notification forums") where parolees were notified of the sanctions for reoffending on parole and offered community resources to support their integration into their community. Our evaluation of a randomized controlled trial of this program found that it did not impact gun violence or create community spillover effects, but did reduce parole violations - overall violations fell by 15 percent while absconding fell by 25 percent. We discuss potential mechanisms and challenges for other similar programs.

1. Introduction

In the United States, 71% of all individuals released from prison are re-arrested within five years of release (Durose & Antenangeli 2021). Further, individuals previously convicted of a violent felony have the highest rate of rearrest for new violent crimes – 32% are re-arrested for new violent crimes within five years. Many approaches and programs have been considered to decrease violent crime and recidivism among high-risk populations, including those previously convicted of a violent felony, but few have strong records of success. Recent evidence finds that targeted enforcement which focuses on particular, high risk individuals can be effective in reducing violent crime (Chalfin et al. 2021; Groff et al., 2015; Sherman & Weisburd, 1995) and certain social service provision programs, such as those that provide educational and employment opportunities, can be moderately effective in reducing crime (Cook et al., 2015; Heller et al., 2017; Davis & Heller, 2020; Gelber, et al., 2016). In addition some types of post-release community supervision programs can reduce recidivism as long as the conditions of supervision are not too demanding (Doleac & LaForest, 2022), but the combined effects of these programs is still relatively minor and no panacea yet exists (Doleac, 2021).

This study reports the first experimental evidence on an alternative approach often referred to as focused deterrence or "pulling levers," a strategy that has substantial encouraging observational evidence behind it (Braga and Weisburd 2012). Focused deterrence programs are designed to deliver a message making clear that the consequences of future violations will be swift, severe, and certain, combining this message with enhanced enforcement for any violations committed by participants (especially crimes committed with a gun) (Papachristos et al. 2007). This message of deterrence is supplemented with a message of support from the community, and offers of assistance and social services to help participants succeed regarding housing, the labor market, family and community life (Braga & Kennedy, 2012). Conducted in an atmosphere designed to convey mutual respect and dignity, the model is intended to increase the legitimacy of the message delivered by representatives of law enforcement, social service providers, and community members (Tyler 2004).

Over the last two decades, a growing number of focused deterrence programs have been implemented across the United States, many under the larger umbrella of the federal Project Safe Neighborhoods (PSN) program (Braga 2008; Braga and Weisburd 2012; Decker et al. 2007; McGarrell et al. 2010; Meares, Papachristos and Fagan 2009). A large number of these programs

utilize notification forums as a central means of engagement with potential future offenders. At these forums, members of law enforcement and the community connect directly with selected individuals identified as being at high risk of future offending. At each forum, these law enforcement and community members express a strong message of deterrence and community support to invitees in a setting selected to convey mutual respect and legitimacy.

Quasi-experimental evaluations of focused deterrence programs, largely leveraging matched comparison groups, have 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, Papachristos and Fagan 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 (Braga et al., 2019).

Our work contributes to the literature on targeted programs to decrease violent crime, and the effect of focused deterrence programs in particular, by presenting results from the first randomized controlled trial of a focused deterrence notification forum initiative. Specifically, we evaluate the effects of the New York State Gun Violence Reduction Program (GVRP), which consisted of notification forums, held between 2013 and 2014 in five sites across New York State, for recently-released paroles who had previously committed a violent felony or gun crime.[2]

In contrast to the prior literature, we find that the GVRP notification forums had no significant impact on future arrests, 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 6 months after forum attendance, and represent a reduction of approximately 15% in total violations and 25% in absconding violations among program attendees.

The central findings of the study are very clear, but interpreting the findings is more difficult. Strong effects on parole violations due to absconding may be driven by attendees'

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[2] In 2012, Patrick Sharkey was contracted by New York State to design an evaluation of the New York State Gun Violence Reduction Program. Sharkey developed the research design for the evaluation and carried out a preliminary analysis of the effect of the forums, which took place between February 2013 and July 2014, on outcomes through August, 2014, one month after the final participants entered the sample (for more information, see the initial policy report, Sharkey 2014). This study builds upon that initial policy report, evaluates the effects of the forum for the full set of participants over both short and long term outcome periods, and concludes analysis of the GVRP.

engagement with the program or simply by closer communication between attendees and parole officers. The absence of any impact on arrests could be due to the low dosage of the intervention, by the unique characteristics of the target population, or by the fact that the message focused on gun violence was less salient during this period of 2013-2014, one of the safest in New York's history. It is important to note, however, that these are ex post facto explanations for the absence of an impact on the main outcome, firearm arrests. The program model aligned with previous models implemented in other cities, and conversations with staff during the implementation of the program suggested that everyone involved expected the forums to be effective. The overall finding of no effect on any types of arrests is an important finding to be considered alongside the large literature from focused deterrence evaluations.

Section 2 provides additional information about prior focused deterrence programs and details on the New York GVRP randomized controlled trial. Section 3 discusses the data used for the analysis, and Section 4 presents the research design. Section 5 provides the results. Finally, in Section 6 we discuss how we interpret the results, potential mechanisms driving them, and their implications for similar programs.

2. Background

Focused Deterrence

Violence reduction programs based on focused deterrence have emerged as one of the more promising and innovative strategies to deter violent crime and reduce re-arrests of violent offenders over the last twenty years (McDevitt et al. 2006). Many of these programs utilize notification forums as their core component, in order to reach potential violent offenders directly with a message of deterrence, support, and mutual respect and legitimacy. Such programs have been implemented in dozens of sites across the country, many as part of the larger federal Project Safe Neighborhoods (PSN) program (Braga 2008; Braga and Weisburd 2012; Decker et al. 2007; McGarrell et al. 2010; Meares, Papachristos and Fagan 2009). These programs, and focused deterrence programs more generally, usually fall into one of three categories based on the target population – individuals involved in gang violence, individuals involved in drug markets, and individuals with prior violent crime convictions (Braga et al., 2019).

For all three target populations, evaluations of prior focused deterrence programs have concluded that they not only decrease future crime, but create strong positive spillover effects in

the wider community (Braga et al., 2019, Berman 2022). However, there remains substantial uncertainty in the literature regarding these results, as the vast majority of prior evaluations have relied on propensity score matching and none have used a randomized controlled trial design.

While a number of prior focused deterrence programs have focused on individuals involved in gang violence and drug markets, relatively few prior programs have focused on individuals with prior violent crime convictions. The most well-known program focusing on this population was the Chicago PSN program conducted in the early- to mid-2000s. PSN Chicago held notification forums for recently paroled individuals with a history of gun violence and gang participation, that were similar in scope and scale to the New York GVRP forums. Similar to evaluations of other focused deterrence programs, evaluations of the Chicago forums concluded that they were associated with reductions in community-level violent crime and individual arrests (Meares et al. 2009; Papachristos et al., 2007). Papachristos et al. (2007) also found that neighborhoods where PSN was implemented experienced a 37% drop in quarterly gun homicide rates relative to a comparable set of neighborhoods in Chicago that did not take part. Finally, they found that the effect on community crime rates was strongest in police districts where the most offenders attended a notification forum, and the largest impacts were found on outcomes measuring gun violence.

Studying the effect of participation in the Chicago forums on individual outcomes, Meares et al. (2009) found that participants were close to 30 percent less likely to return to prison compared with a control group of similar individuals who did not attend the forums. Long-term estimated effects were also large, as only about a quarter of participants had been arrested three years later, compared to half of the matched control group. Lastly, they found that the estimated effects were stronger for first time offenders, meaning those who had been convicted of only one prior offense. However, it is difficult to make strong causal claims based on these results of either of these two analyses because the implementation sites, and parolees chosen to attend the forums, were not randomly selected for the Chicago PSN program, requiring comparison groups to be selected by matching on observables.

Although prior results from focused deterrence programs, including those from the Chicago PSN, provide suggestive evidence that these types of programs can be effective, little is known about the mechanisms by which the program works or the specific features of the program that are most important for program success. As discussed by Braga et al. (2019), the main theoretical effect of the forums is one of deterrence, by increasing the "certainty, swiftness, and

severity" of punishment if an individual commits a future violent crime, increasing the salience of these punishments, and indirectly disseminating this information throughout the community. However, several additional mechanisms have been proposed, including redirecting individuals away from crime by providing access to social services and community supports (Braga et al., 2008), increasing the collective efficacy of a community (Sampson et al., 1997), and improving perceptions of the legitimacy of the procedural justice system (Nagan and Telep, 2017).[3]

Further, McDevitt et al. (2006) argued that the notification forums alone may be insufficient to generate a meaningful change in behavior, and argued that repeated follow-ups with the targeted population may be necessary to generate a true impact. In order for the meetings to be effective, several commentators, researchers, and observers have noted that the elements of the program designed to deter future criminal activity and the elements designed to foster legitimacy must be perceived as real (McDevitt et al. 2006). For instance, the forums might only serve as a deterrent if the threat of intensive oversight and severe punishment is carried out in the target communities.[4].

Other research has focused attention on the setting and content of the notification forums. Kennedy (2010) argued that the content of the messages delivered in the forums is essential to their success. To foster a sense of legitimacy, Kennedy (2010) placed emphasis on the importance of specific messages indicating clearly that law enforcement is eager to help the participants succeed, that the threat of violence in the community is the central concern for everyone in the room, that law enforcement is justified in their efforts to prosecute violent crime aggressively, that the participants have the capacity to succeed and integrate into their communities, and that law enforcement is willing to alter its own tactics as well. Meares and Papachristos (2009) focused their attention on the importance of the physical setting of the forums, arguing that the location of the forums and the setup of the room are crucial to establishing an environment of mutual respect and the importance of community.

The point of this discussion is to make clear that the estimated impact of focused deterrence programs must be viewed within the larger context of the details of each program's

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[3] Additionally, Braga et al. (2008) argue that the offer of support services was effective in most PSN sites, and reflected a real commitment to providing the types of services that can allow participants to integrate into their communities. For an example of an exception to this claim see Tita et al. (2003) and Tita et al. (2010).

[4] The increase in federal prosecution of firearms offenses in prior PSN sites suggests that this element of the project had been implemented and the threat of aggressive prosecution had been enforced (Gardner 2007).

implementation, the specific messages delivered at notification forums, and the various settings in which the forums take place. The main difference between the New York GVRP and Chicago PSN programs, relative to other focused deterrence programs, is the target population of high-risk parolees as opposed to individuals suspected of involvement in gang violence or drug markets. The only notable difference between the GVRP and the Chicago PSN program was that the Chicago PSN forums were conducted alongside a coordinated change in law enforcement strategy and prosecution policy (Papachristos et al., 2007). Otherwise, the New York GVRP forums match the implementation, messaging, and settings of the Chicago PSN and other prior focused deterrence programs.[5]

Study

The New York State Gun Violence Reduction Project was conducted across five sites in New York State in 2013 and 2014. Each month, in each study site, selected 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. For example, representatives of law enforcement clearly 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 Albany, Schenectady, and three sites in

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[5] Although our evaluation does not assess variation in the implementation of the GVRP across program sites, members of the research team did observe numerous meetings conducted in the three New York City sites. The notes taken at the various meetings confirmed that the same basic features of the intervention were found in all three of the New York City sites, largely matching the features of prior focused deterrence programs. Although there were slight differences in the settings for the forums across these sites and in the effectiveness of particular speakers, the content of the presentations that were delivered was very similar across sites.

New York City representing areas of the Bronx, Manhattan, and Brooklyn.[6] Individual living in these sites was eligible for the forums if they (1) were on parole, (2) had been convicted of a violent felony or firearm arrest, (3) did not have a mental health diagnosis or a prior sexual assault charge, (4) 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, neighborhoods (specifically, census tracts) within each area were randomly assigned to a "neighborhood treatment group" and a "neighborhood control group." Individuals who lived within the treatment group neighborhoods were eligible to be called in to the forums. Individuals in the control group neighborhoods 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 at these sites. 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 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). DCJS

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[6] See Appendix A for further details on the five study sites.

provided criminal history for all individuals in the study from January 1990 through June 2019.[7] 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."[8] 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."[9]

In order to study the effects of the forums on neighborhood crime in the NYC sites, we pulled publicly-available New York City Police Department (NYPD) crime data from the NYC OpenData portal. This data included the date and location of all crime complaints, arrests, and shootings reported to the NYPD between July 2010 and June 2019.[10] Complaint and arrest data include all valid felony, misdemeanor, and violation crimes and arrests.

The two stages of randomization ensure that individuals in the treatment group and control group should be balanced in expectation on both observable and unobservable characteristics, but there is always the possibility that they are unbalanced in the working sample. To assess how well the treatment and control groups were balanced on observable characteristics, Table 1 displays 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. We observed several characteristics about each individual 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

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[7] 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.

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

[9] "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.

[10] The NYPD datasets included the XY coordinate of the incident, which was then mapped to the 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.

felonies. As shown in Table 1, there is no evidence to suggest that randomization resulted in any significant differences between the individual-level treatment and control groups.[11] In terms of sample demographics, the average age of parolees was 37 and nearly all were male. Two-thirds were Black, 20% were Hispanic, and only 10% were non-Hispanic white. Finally, the average parolee in the sample had been previously arrested 11 times, of which on average two were related to prior violent felonies.

3. Research Design

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

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

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

In this equation Y_i is the outcome of interest for individual *i*, related to whether the individual was arrested or committed a parole violation during a given period of time after his projected forum attendance date. Specifically, for treatment individuals, the outcome period starts at the date of expected forum attendance. For control individuals, it starts at the date of the forum the individual would have been expected to attend had they been assigned to the treatment group. 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 (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). 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

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[11] 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.

otherwise. Control variables in the model adjust for any slight differences in important characteristics of the sample population that might be associated with outcomes, in order to provides more precise estimates.

Note that our primary results estimate the effect of being invited to attend a forum as opposed to the effect of actually attending – the "intent-to-treat" (ITT) effect. This method likely 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. Additionally, of those in the treatment group who attended a forum, 83% attended on the date of expected forum attendance.[12] As shown in Appendix Table A2, estimates of the "treatment on the treated" (TOT) effect lead to the same 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. It 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 spillover effect that spreads beyond those who personally attend.

The second analysis estimates the effect of the forums in a way that is less vulnerable to bias from potential spillover effects. For this analysis the outcomes of individuals in Group 1 (neighborhood treatment group and individual treatment group) are compared with individuals in Groups 3 and 4 (neighborhood control groups), 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:

$$Y_i = \alpha + \beta X_i + \gamma(Group_1)_i + \theta_b + \varepsilon_i \quad , \tag{2}$$

$$\forall i \in (Group_1, Group_3, Group_4) \cap (Bronx, Brooklyn, Manhattan) \quad .$$

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.

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[12] The remaining 17% of attendees had a median length of time between expected attendance date and attendance date of 35 days.

The third analysis is the converse of the second analysis – it explicitly tests for the existence of community spillover effects caused by the forums. We directly estimate these spillover effects by comparing the outcomes of individuals in Group 2 (neighborhood treatment group and individual control group) with the outcomes of individuals in Groups 3 and 4 (neighborhood control groups), among the NYC study sites. The estimate derived from Equation 3 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:

$$Y_i = \alpha + \beta X_i + \gamma (Group_2)_i + \theta_b + \varepsilon_i \quad , \tag{3}$$

$$\forall i \in (Group_2, Group_3, Group_4) \cap (Bronx, Brooklyn, Manhattan) \quad .$$

Finally, the fourth analysis 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. It is important to note that estimating the effects on neighborhood crime rates was not the central goal of the evaluation, and that the randomization procedure and sample sizes were designed to precisely estimate individual-level effects as opposed to neighborhood-level effects. However, given the strong findings from Chicago suggesting neighborhood-level effects of the Chicago PSN (Papachristos et al. 2007), we wanted to explore the impacts of the program on community-level crime. The effect of the forums on neighborhood-level outcome Y_j is given by γ in Equation 4:

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

$$\forall j \in Bronx, Brooklyn, Manhattan$$

In this equation 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 first estimate 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. First, there does not appear to be evidence that the forums reduced rearrest rates among the treatment group. The estimates show that individuals who were not invited to the forums had similar arrest rates in the first six months after forum attendance (23%) as individuals who were invited to the forums, with a point estimate on the effect of treatment of -0.6 percentage points and a 95% confidence interval of -3.7 to 2.5 percentage points. Based on these results there did not appear to be an effect of the forums on rearrests, and we can reject any hypothesis that the forums had a substantial effect on rearrests. We observe a similar lack of effect on violent felony arrests over the first six months after forum attendance.

Second, the estimates suggest that the forums did reduce parole violations in the short-term. Specifically, parole violation rates within six months of forum invitation, which started at a base rate of 21%, were reduced by around three percentage points - a 15% decrease in parole violations. Breaking down parole violations by type, the results appear to be driven by a notable reduction in absconding violations – invitation to the forums appears to decrease absconding parole violations by around two percentage points, or 25% from a base rate of 10%, in the first six months after forum attendance.

Given that the forum is a one-time event for each attendee, it is possible that the forums could deter criminal activity in the months immediately following the forum's messaging but not in the years after. However, as shown in Appendix Table A1, our main 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. If anything, results actually appear to be strongest over the six month outcome period. 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.

Next, since prior research has shown that criminal activity typically decreases with age, and there is reason to believe that GVRP in particular may have been more effective for younger parolees, 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, in the first six months after forum attendance, among parolees with at least two prior weapons charges. Finally, 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.

Community Effects

Prior research on focused deterrence initiatives have shown the potential for spillover effects (Papachristos et al., 2007; Braga et al., 2019). That is, the existence of the program in a given neighborhood may reduce rearrests and violations not only among treated parolees, but also among other individuals living in the same neighborhood. Analyses 2 and 3, presented in Table 3, investigate these effects. First, Analysis 2 estimates the effects of the forum 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 there is likely little bias in the results from Analysis 1 due to spillover effects. Second, 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 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 neighborhoods to those tracts randomized to control neighborhoods across NYC sites.[13] Results from this analysis suggest that there was no notable difference in crime between treatment and control neighborhoods. Specifically, we estimate a 95% confidence interval for the difference in crime complaints between treatment neighborhoods and control neighborhoods of -3% to 5%.

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[13] Only census tracts with at least five study participants are included in these models.

Based on these results we conclude that the forums appear to have had little to no impact on neighborhood-level criminal activity.

5. Discussion

This evaluation estimated the New York State Gun Violence Reduction Project's impact 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 design involved the randomization of neighborhoods as well as the randomization of individuals within those neighborhoods to determine forum invitees. As the randomization appears to have worked as expected, the research design provides great confidence that the estimated impacts represent causal effects of the New York GVRP program.

The evaluation highlights three findings. The first is that there was no detectable effect of the program on subsequent arrests. This does not mean that one should conclude definitively that the GVRP had no impact on arrests, but rather that the available evidence provides no hint of such an impact. Across different outcome periods, some specifications show negative effects on arrests, most estimates are close to zero, and a few are positive. For example, six months after forum attendance we estimate no impact of the forums with a 95% confidence interval falling between a 3.7 percentage point decrease in arrests and a 2.5 percentage point increase in arrests, from a baseline rearrest rate of 23%. Considering all of the evidence, our results motivate a conclusion that there was no strong effect of the program on individual-level future arrests.

The second finding is that individuals who were called in to attend the notification forums were substantially less likely to violate their parole, and that this effect was driven primarily by reductions in violations due to absconding. Specifically, our main estimates indicates that being called in to attend a forum reduced the probability of a subsequent parole violation that lead 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 likely that the forums increased the salience of punishment for violating parole conditions, even if they did not appear to have an impact of future arrests.

The third finding is that we observed 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. As such, it appears that the messaging of the forums did not disseminate through the community, or, to the extent it did, did not disseminate in such a way as to impact community criminal behavior.

These finding contradict the quasi-experimental results from similar, prior focused deterrence programs. While prior evaluations found that the message from the notification forums not only impacted program participants (by decreasing future violent offending) but also filtered out into the community, we found no such effects in New York. This result gives us pause about the effects of focused deterrence programs in other settings, given the quasi-experimental nature of the prior focused deterrence evaluations in other settings. Further, to our knowledge this study is the first to incorporate a research design that allows for an explicit test of community spillovers, and finds no evidence to support the presence of a "contagion" effect.

The main results from this evaluation are based on a carefully executed research design that allows for strong causal inferences, yet the findings are somewhat surprising and difficult to interpret. The evaluation was not set up to provide evidence on why the program did or did not generate strong impacts on different outcomes, and developing a full explanation of the results is not possible with the available data.

However, some potential mechanisms are worth discussing. At the time of the study, parolees attended a single forum. It is possible that this is not a sufficient dosage to see significant reductions in arrests, and that increasing the number of forums attended may have a larger impact. Additionally, 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.

Another hypothesis is that the null effect we observed was due to selecting parolees as the population of interest. Thought the Chicago PSN found positive effects of the program for this population, it is possible that focused deterrence notification forums are less effective for

individuals recently released from prison than for other high-risk individuals who live in the community. Paroled individuals interact with the procedural 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 third hypothesis worth discussing is whether the null effect of the forums on observed rearrests is due, mechanically, to the reductions in absconding among treatment group 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. As treatment group parolees commit less 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 can't say what proportion of individuals who are re-incarcerated 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 extra 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.

While the reduction in parole violations represents a positive impact of the program, the broad goals of the New York GVRP were to integrate prisoners back into their communities and to reduce the potential for them to become involved in violent crime, particularly crime with firearms. Thought the effects on parole violations suggest that the process of integration into the community may have been improved by participation in the notification forums, the evidence suggests that the goal of reducing violent crime was not achieved.

However, there is still value in this reduction in parole violations attributed to the program. 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 and Gallear, 2019).

Furthermore, 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) (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	*p-value*	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 (%)	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 track 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.

Tabe 3 – Spillover Effects

	Control mean	Treatment Effect (%)	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 track 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 (%)	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-track 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 randonized 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.
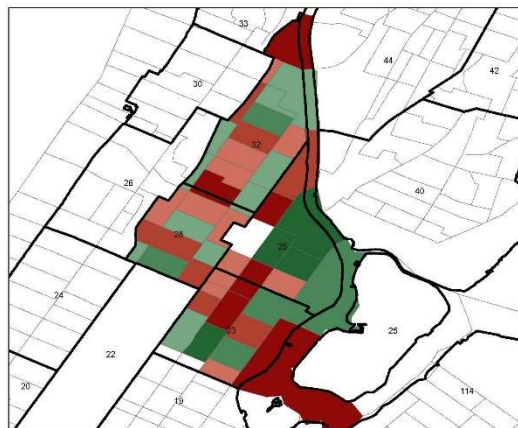
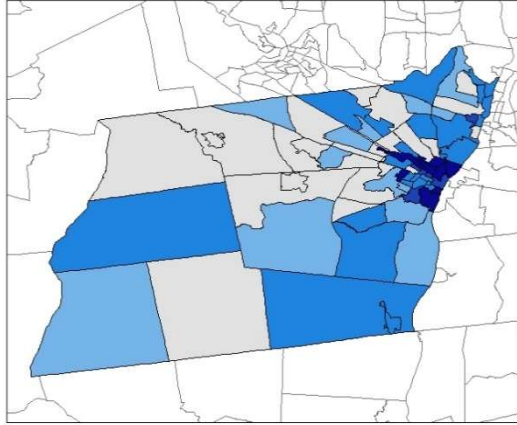**Figure A1. Locations of the five program sites and volume of participants by census tract.**
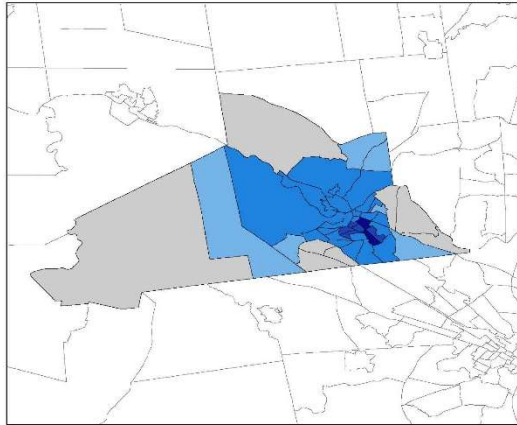

The Bronx site

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 (%)	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 track 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 (%)	Treatment Effect (pp)	Std Error
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 track 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 (%)	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 track 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.[14]

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[14] 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 (%)	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 track 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.