



Seattle's Law Enforcement Assisted Diversion (LEAD): Program effects on recidivism outcomes



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ABSTRACT

Drug users and dealers frequently cycle through the criminal justice system in what is sometimes referred to as a “revolving door.” Arrest, incarceration and prosecution have not deterred this recidivism. Seattle's Law Enforcement Assisted Diversion (LEAD) program was established to divert these individuals to case management and supportive services instead of jail and prosecution. A nonrandomized controlled evaluation was conducted to examine LEAD effects on criminal recidivism (i.e., arrests, criminal charges). The sample included 318 people suspected of low-level drug and prostitution activity in downtown Seattle: 203 received LEAD, and 115 experienced the system-as-usual control condition. Analyses were conducted using logistic generalized estimating equation models over both the shorter term (i.e., six months prior and subsequent to evaluation entry) and longer term (i.e., two years prior to the LEAD start date through July 2014). Compared to controls, LEAD participants had 60% lower odds of arrest during the six months subsequent to evaluation entry; and both a 58% lower odds of arrest and 39% lower odds of being charged with a felony over the longer term. These statistically significant differences in arrests and felony charges for LEAD versus control participants indicated positive effects of the LEAD program on recidivism.

1. Introduction

Despite policing efforts, drug users and dealers frequently cycle through the criminal justice system in what is sometimes referred to as a “revolving door.”¹ The traditional approach of incarceration and prosecution has not helped to deter this recidivism (Wormith & Olver, 2002). On the contrary, this approach may contribute to the cycle by limiting opportunities for these individuals to reenter the workforce, which relegates repeat offenders to continue to work in illegal markets (Fletcher, 2013). This approach also creates obstacles to obtaining housing, benefits, and drug treatment. There have thus been calls for innovative programs to engage and rehabilitate these individuals in a more effective and socially just manner to stop the revolving door (Warner & Kramer, 2009).

This need for innovative programs to reduce recidivism inspired Seattle's collaborative, harm-reduction, community-based, prebooking (i.e., following arrest and prior to charges and incarceration) Law Enforcement Assisted Diversion (LEAD) program. The LEAD pilot program was established in 2011 as a means of diverting people suspected of low-level drug and prostitution offenses to social and legal services instead of prosecution and incarceration. LEAD comprises

three primary components: 1) an initial program entry process, which includes diversion from the criminal justice and legal systems; 2) harm-reduction case management (i.e., low-barrier counseling and connection to social and clinical services that is offered with neither requirement of nor pressure towards substance-use treatment or abstinence); and 3) higher-level coordination of legal system involvement.

People suspected of violations of the uniform controlled substances act (VUCSA) and/or prostitution offenses are arrested as usual and brought to the police precinct. There, they are screened for LEAD eligibility by an on-duty officer. Eligible individuals are offered the option of participating in LEAD instead of undergoing standard criminal booking and prosecution. Interested individuals are referred to an intake with a LEAD case manager.

LEAD case management is provided by Evergreen Treatment Service's REACH homeless outreach program, which provides outreach and harm-reduction-oriented case management to individuals experiencing housing instability and substance use disorders. A harm reduction approach entails meeting individuals ‘where they are at’ in their communities and in their own motivation to change substance use (Collins et al., 2011), even if they are not yet ready, willing or able to stop using substances. In this highly individualized, collaborative

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approach, the goals are to engage and retain individuals in services by listening attentively to clients' needs and connecting them with appropriate community resources, such as housing placement, medical care, legal advocacy, job training, mental health counseling, and chemical dependency treatment.

Legal advocacy, which features coordination with the legal system for cases that are not eligible for diversion, is a particularly important aspect of the LEAD program. Prosecutors make discretionary decisions about whether to file charges, recommend pretrial detention or release conditions, reduce charges, recommend incarceration after conviction, and/or dismiss charges for LEAD participants. This assistance allows LEAD participants to continue to make progress on prosocial program goals.

To date, various types of diversion programs have been implemented to address the needs of individuals with high levels of criminal recidivism co-occurring psychiatric disorders (Hayhurst, Leitner, & Davies, 2015; Lattimore, Broner, Sherman, Frisman, & Shafer, 2003; Sirotych, 2009). These programs typically include diversion from criminal prosecution and incarceration to social services and support. Recent systematic reviews of such programs have indicated mixed findings regarding their effectiveness and cost-effectiveness, indicating that evaluations of diversion programs to date have shown some evidence of improving drug outcomes for participants but no evidence that they reduce recidivism or reduce overall costs to the criminal justice and legal systems (Hayhurst et al., 2015; Sirotych, 2009).

LEAD differs from such existing programs because it is the first prebooking diversion program in the US specifically designed for substance users who are suspected of drug and prostitution offenses. Further, it maintains a harm-reduction orientation, which removes many barriers, such as legal coercion for and program requirements of abstinence achievement and treatment attendance, that block engagement with other programs. (Goetz & Mitchell, 2006) Because LEAD differs from other existing diversion programs, an evaluation is critically needed to inform key stakeholders, policy makers, and other interested parties of its impact. The primary aim of the LEAD program is to reduce criminal recidivism (i.e., arrests and charges) relative to standard booking and prosecution. To evaluate this aim, the present program evaluation tested the effects of LEAD compared to a system-as-usual (i.e., booking and incarceration) control group on shorter- and longer-term recidivism (i.e., arrests and criminal charges).

2. Method

2.1. Participants

This evaluation included 318 adults who were suspected of recent violations of the uniform controlled substances act (VUCSA) and/or prostitution offenses and were deemed eligible for LEAD by arresting officers. Individuals were ineligible for participation if any of the following exclusion criteria applied: a) the amount of drugs involved exceeded 3 g (all drug classes were eligible); b) the suspected drug activity involved delivery or possession with intent to deliver and there was reason to believe the suspect was dealing for profit above a subsistence income; c) the individual did not appear amenable to diversion; d) the individual appeared to exploit minors or others in a drug dealing enterprise; e) the individual was suspected of promoting prostitution; f) the individual had a disqualifying criminal history (i.e., conviction for murder 1 or 2, arson 1 or 2, robbery 1, assault 1, kidnapping, Violation of the Uniform Firearms Act 1, sex offense, or attempt of any of these crimes); g) within the past 10 years, the individual was convicted on a domestic violence offense, robbery 2, assault 2 or 3, burglary 1 or 2, or Violation of the Uniform Firearms Act 2; or h) the individual was already involved in King County Drug Diversion Court or Mental Health Court.

2.2. Group allocation

Participants were allocated to the LEAD or control group in a few different ways. First, police officer shifts were randomized to be either LEAD or control shifts, and eligible individuals were allocated to those conditions if they were arrested during the respective shifts. Second, a pathway for social contacts (i.e., individuals who were encountered within the original catchment area and who were suspected of recent drug or prostitution activity) to enter into the LEAD program was deemed necessary from a policy and policing standpoint. Because they were all subject to the same inclusion criteria (i.e., suspicion of drug or prostitution activity in the neighborhood), LEAD participants recruited as social contacts or via arrest were likely drawn from the same population, which was confirmed in analyses reported below. Finally, after the evaluation began, operational partners recognized that there was a limited number of potential participants in the originally planned catchment area. Over time, most of these individuals were approached for LEAD involvement, which left a dwindling number of individuals available for inclusion in the control group. Thus, to accommodate the need for an adequate and comparable control group, control areas (in addition to control shifts) were added to the evaluation. Additional control areas were neighborhoods patrolled by the same squads as in the original catchment area. Thus, control and LEAD participants were brought into the study using identical criteria, which reflected the uniform within-squad training, patterns of exercising discretion, and supervision. This step ensured adequate representation of qualifying participants in the control condition to make up for the initial catchment area's relatively small population.

Based on the location and timing of their arrest, 203 individuals were allocated to LEAD, and 115 individuals were allocated to the control condition. At the time of referral, 146 of the LEAD participants were under arrest, and 57 entered the evaluation as social contacts.

2.3. Evaluation design

Given the real-world constraints on group allocation, this evaluation has a two-group, longitudinal, nonrandomized controlled design. According to federal standards, nonrandomized controlled designs are consistent with the early intervention development and evaluation exemplified by the LEAD program (Rounsaville, Carroll, & Onken, 2001). Further, high-quality nonrandomized controlled evaluations that account for potential confounds show similar effect sizes and widely correspond to RCT outcomes (Benson & Hartz, 2000). Thus, this design approach was deemed most appropriate for the program as delivered.

2.4. Measures

Demographic and program data were obtained from the LEAD case management team and from the Seattle Police Department LEAD records. Case management contacts were defined as any phone or in-person communications between a REACH case manager and a LEAD participant lasting at least 5 min. Contact data were logged by case managers and stored in the REACH database (AGENCY Software, Seattle, WA). Nationwide arrest data were extracted by the King County Prosecuting Attorney's office from the FBI's National Crime Information Center (NCIC) and given to the evaluation team for analysis.

For the purpose of this evaluation, arrests refer to having been taken into police custody for a crime committed during the LEAD program evaluation time frame (i.e., 10/1/2009 through 7/31/2014). These were arrests related to new offenses, and did not include parole or probation violations or failure to comply offenses pursuant to prior violations, which were removed for these analyses (5.1%; $n = 188$). Warrant arrests related to offenses that occurred after the date of study entry were included in the data set, but analyzed as described below.

Charges were criminal charges—including felonies—that occurred during the LEAD evaluation time frame noted above.

2.5. Procedures

The implementation phase, including participant recruitment, occurred from October 2011 through July 2014. The evaluation team obtained all necessary IRB exemptions and data sharing agreements from the appropriate entities for the purposes of conducting this program evaluation.

2.5.1. LEAD condition

Individuals who were encountered during LEAD shifts were screened for project eligibility by officers on duty and, provided they met inclusion criteria and completed the intake process, they were diverted to the LEAD program at point of arrest instead of undergoing standard jail booking and criminal prosecution. A smaller number of individuals were referred by officers as social contacts. Social contacts were individuals who were eligible for the LEAD program due to known recent criminal activity, but were recruited by officers outside of a criminal incident during a LEAD shift within the original LEAD catchment area. Both arrest and social contact referrals to LEAD required that participants were suspected of narcotics or prostitution activity and met other program criteria.

Interested individuals were referred to a LEAD case manager. Case managers were social workers and chemical dependency counselors who were trained in a harm-reduction orientation and were responsible for connecting participants to appropriate services. Case managers obtained written, informed consent for participation in LEAD. Next, case managers conducted intake assessments evaluating participants' substance-use frequency, treatment episodes, time spent in housing, quality of life, psychological symptoms, interpersonal relationships, and health status. After completing the intake process, participants received case management through Evergreen Treatment Services' (ETS) REACH homeless outreach program, which connected participants with existing resources in the community (e.g., legal advocacy, job training or placement, housing assistance, counseling). Service provision was not time-limited and was available for as long as clients needed case management services in their own estimation; thus, there was no program graduation as such. During all interactions with LEAD participants, case managers used a client-centered, harm-reduction approach (i.e., a nonjudgmental, compassionate style; client-driven goal setting; no requirement of abstinence from substances). Additionally, case managers had access to funds to provide financial support for the fulfilment of participants' basic needs (e.g., motel stays, housing, food, clothing, treatment, and various additional items and services). Other key program features included some coordination of prosecution strategy in other pending criminal cases participants had in local courts as well as some assistance with miscellaneous civil legal problems.

2.5.2. System-as-usual control condition

Eligible individuals who were arrested during control shifts or within control areas were processed through the criminal justice system as usual (e.g., jail booking, criminal charges). These participants served as the control group in the current evaluation. All participants were recruited by the same officers from four squads who were assigned to the downtown Seattle area. These individuals were not prevented from seeking or receiving social services, including treatment; however, their service utilization in the community was not tracked in the context of this evaluation.

2.6. Data analysis plan

The goal of this evaluation was to test LEAD effects on recidivism (i.e., arrests, criminal charges) over both the shorter term (i.e., six

months prior and subsequent to program involvement) and the longer term (i.e., encompassing two years prior to the LEAD start date through 7/31/14). Given their relative statistical rarity, recidivism counts were converted to dichotomous (yes/no) outcomes, excluding any arrest that occurred the day participants entered the evaluation. Because longer-term analyses involved unequal windows of time for participants starting at different points during the program implementation, we statistically controlled for this factor in each of the longer-term models, using number of months prior and subsequent to evaluation entry as a time-varying covariate.

2.6.1. Propensity score weights

Propensity score weighting is a statistical means of accounting for systematic differences between treatment and control groups in observational studies and nonrandomized controlled trials (Austin, 2011). When appropriate, using propensity scores as sample weights can reduce or eliminate the effects of confounding when using observational data to estimate treatment effects (Guo & Fraser, 2015). Propensity score weighting is particularly appropriate when propensity score matching could result in match failures that would decrease the sample size and resulting statistical power below desirable levels and when analyses involve more complex data and modeling situations (e.g., covariates, nonnormally distributed data) (Guo & Fraser, 2015).

We used generalized boosted regression to estimate propensity scores for all eligible participants ($N = 318$). This type of regression employs an automated, data-adaptive algorithm that fits several models by way of a regression tree and then merges the predictions of these various models. The advantage of generalized boosted regression is that it is computationally fast to fit, handles various types of data distributions, and takes into account interaction terms. In addition, it is invariant to one-to-one transformations of the independent variables; thus, the raw, log, and exponentiated variants lead to the same propensity score adjustments (McCaffrey, Ridgeway, & Morral, 2004).

Next, we created two weighting variables: one for estimating the average treatment effect (ATE) and one for estimating the average treatment effect for treated participants (ATT) (Guo & Fraser, 2015). ATE may be considered to be a between-subjects' difference or the average effect of moving an untreated population to a treated population (Austin, 2011). Alternatively, treatment effects may be considered at the individual or within-subjects level. The ATT may be considered to be the average effect of treatment for those who receive the treatment—in this case LEAD (Austin, 2011). Both types of propensity scores are relevant for the current analysis because, if considered effective, LEAD a) would be applied widely to the larger population of drug and sex work offenders (reflected in ATE) and b) is a highly tailored, individual-level intervention whose effects on program participants, which are reflected in ATT effects, would be important to track as well. Both propensity score weights were thus used in analyses and reported on in the results section.

Propensity score analyses comprised three steps. First, we generated the propensity scores using generalized boosted regression. Where p is the propensity score, the ATE is $1/p$ for LEAD participants and $1/(1 - p)$ for control participants. ATT is equal to 1 for treated participants, and $p/(1 - p)$ for control participants. Second, we used ATE and ATT weights to conduct balance checks, which comprised a series of ordinary least squares, logistic and multinomial logistic regressions testing whether propensity scores improved the balance between the control and LEAD groups. Finally, we used the ATT and ATE as sampling weights in the primary analyses.

2.6.2. Primary analyses

Using SPSS 19, descriptive analyses were conducted to describe the sample. Population-averaged generalized estimating equation models (GEEs) (Zeger & Liang, 1986), conducted in STATA 13, were used in primary analyses. GEEs model marginal effects and may be used to accommodate alternative distributions (e.g., binomial) and correlated

data (e.g., data collected on the same participant over time). In this evaluation, GEEs were used to test the relative effects on recidivism outcomes of: a) *time* (0 = baseline, 1 = follow-up), which controlled for overall, longitudinal effects that could reflect regression to the mean; b) *intervention group* (0 = control, 1 = LEAD); and c) the two-way *time x intervention group* interaction. The interaction shows the effect of the LEAD intervention on longitudinal recidivism outcomes. Additionally, in longer-term analyses, we controlled for time in the evaluation as a time-varying covariate (i.e., years prior and subsequent to evaluation entry). Propensity score weights were included in the analyses as sampling weights.

Because recidivism outcomes were dichotomous, we specified Bernoulli distributions with the logit link. We assumed an exchangeable correlation structure to accommodate repeated measures on one individual, which served as the sole clustering variable (Hardin & Hilbe, 2003). To enhance model interpretability, resulting effect sizes were exponentiated and reported as odds ratios (ORs), where ORs < 1 indicate an inverse association, ORs = 1 indicate no association, and ORs > 1 indicate a positive association. Alphas were set to $p = 0.05$. Confidence intervals were set to 95%.

3. Results

3.1. Overall sample description

Participants in this evaluation ($N = 318$) had an average age of 40.17 ($SD = 11.85$) years and were predominantly male (34% female; $n = 109$). Of the overall sample, 60% were identified in police records as African American, 26% as European American, 4% as American Indian/Alaska Native, 4% as Multiracial, 3% as Hispanic/Latino/a, 2% as Asian American, and 1% as Other.

Agency records indicated that 84.24% of participants who were assigned to the LEAD condition had at least one case management session. Overall, participants had a mean of 19.36 ($SD = 18.84$) contacts with case managers.

In the six months prior to evaluation entry, participants had accrued a total of 206 arrests and 151 charges, of which 17% ($n = 26$) were felony charges. Expanding out to all incidents since the start of the evaluation time frame (10/1/09) through the current evaluation window (7/31/14), evaluation participants accrued 1415 arrests and 994 charges, of which 21% ($n = 213$) were felony charges.

3.2. Group differences at baseline

3.2.1. Arrest diversion versus social contact participants who received LEAD

Of the baseline demographic and recidivism (i.e., criminal history) variables, participant age was the only variable that evinced a statistically significant difference between the arrest diversion ($M = 40.35$, $SD = 11.09$) and social contact ($M = 45.24$, $SD = 10.65$) groups ($p = 0.006$; other $ps > 0.12$). Given the lack of observed differences and the fact the two groups were recruited using the same inclusion criteria by the same officers, it was concluded that these two groups were very likely drawn from the same population. The arrest diversion and social contact groups were therefore collapsed and analyzed as a single LEAD group.

3.2.2. LEAD versus control group

Wilcoxon rank-sum and Pearson chi-square tests indicated significant group differences on demographic and arrest variables at baseline between LEAD and control participants (see Table 1 for group comparisons). Further, 11 participants died during the 5-year evaluation, including 9 LEAD participants (4.43%) and 2 (1.74%) control participants. This group difference was not statistically significant, and it should be noted that LEAD participants' deaths were systematically documented, whereas control participants' deaths were not. These individuals were included in all analyses, and death was used in

Table 1
Baseline sample characteristics by group.

Demographic Variables	LEAD Group $n = 203$ Mean(SD)/ %(n)	Control Group $n = 115$ Mean(SD)/ %(n)	z/X^2	p -value
Age	41.72 (11.16)	37.44 (12.57)	-3.03	0.003
Gender	39% (79) female	26% (30) female	5.36	0.021
Race/ethnicity			19.43	0.003
American Indian/Alaska Native	6% (13)	0% (0)		
Pacific Islander				
Asian American	< 1% (1)	3% (4)		
Black/African American	55% (112)	68% (78)		
European American	27% (55)	25% (29)		
Hispanic/Latino/a	5% (10)	1% (1)		
More than one race	4% (9)	3% (3)		
Other	1% (3)	0% (0)		
Death	4% (9)	2% (2)	1.60	0.21
Years since evaluation entry	1.54 (.63)	1.78 (.52)	3.66	< 0.001
Total arrests prior to evaluation entry	1.42 (1.49)	1.39 (1.70)	-0.67	0.50

propensity scores and subsequent weighted analyses. There were no significant group differences on baseline arrest or criminal charges ($ps > 0.09$).

3.3. Propensity score balance check

We conducted a check of the group balance after the ATE and ATT weights were applied (see Table 2 for the balance check). Nonsignificant values indicate propensity scores successfully balanced the LEAD and control groups for these variables. Findings indicated that both ATE and ATT performed moderately well in balancing the groups; thus, we report findings for both ATE and ATT in this report.

3.4. Primary analyses

3.4.1. Shorter-term recidivism analyses

The average treatment effect (ATE) model for arrests, which tested overall group effects, was significant, Wald $X^2(3, N = 318) = 19.18$, $p < 0.001$. The ATE indicated that, compared to control participants, LEAD participants had 60% lower odds of having at least one arrest subsequent to program entry. Specifically, the time x intervention group interaction effect was significant indicating a LEAD effect over time ($OR = 0.49$, robust $SE = 0.16$, $p < 0.03$). The ATT model, which indicated the treatment effect for LEAD participants alone, was also significant, Wald $X^2(3, N = 318) = 16.10$, $p = 0.001$. The time x intervention group interaction was likewise significant ($OR = 0.50$,

Table 2
Group balance check following application of propensity score weights.

Covariates	Significance level (p)	
	ATE	ATT
Age	.03*	0.11
Gender	0.07	0.13
Race/ethnicity (dummy group: European American)		
African American	0.31	0.37
Other race/ethnicity	0.07	0.05
Died	0.21	0.20
Overall years in evaluation	0.002*	0.003*
Total arrests prior to evaluation entry	0.66	0.37

Note: * $p < 0.05$. See Table 1 for mean values for the imbalanced variables prior to propensity score generation.

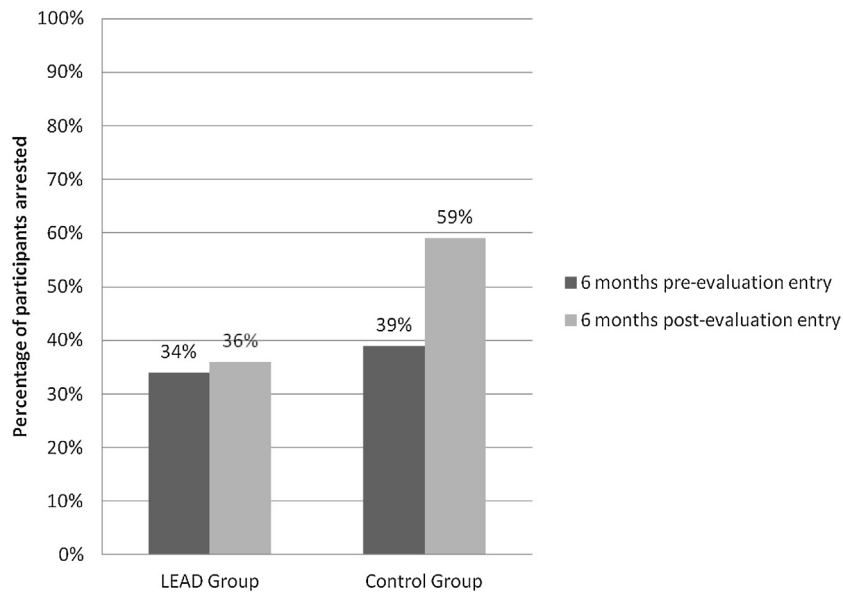


Fig. 1. Percentage of participants arrested 6 months prior and subsequent to evaluation entry (excluding index arrest).

robust $SE = 0.17, p = 0.04$), and indicated 57% lower odds of arrest subsequent to LEAD involvement. See Fig. 1 for the percentage of participants arrested in each group both six months prior and subsequent to evaluation entry.

There were no statistically significant differences between the LEAD and control groups on total charges or felony charges for the 6-month analyses (model $ps > 0.28$).

3.4.2. Longer-term recidivism analyses

We expanded the evaluation time frame to encompass two years prior to the initial LEAD program start date (10/1/2009) through our evaluation close date (7/31/2014). The average treatment effect (ATE) model for arrests was significant, Wald $\chi^2(4, N = 318) = 55.09, p < 0.001$. The time x intervention group interaction showed a significant LEAD effect over time ($OR = 0.30, robust\ SE = 0.11, p = 0.001$). This finding indicated that, compared to control participants, LEAD participants had 58% lower odds of being arrested at least once subsequent to program entry. The ATT model, which indicated the treatment effect for the LEAD participants alone, was also significant, Wald $\chi^2(4, N = 318) = 53.66, p < 0.001$. Results indicated 56% lower odds of being arrested at least once subsequent to LEAD involvement, which was reflected in the significant time x intervention group interaction effect ($OR = 0.29, robust\ SE = 0.11, p = 0.001$). See Fig. 2 for the percentage of participants arrested at least once in each

group prior and subsequent to evaluation entry.

Criminal charge models were statistically significant ($ps < 0.001$). The time x intervention group interactions, however, were not ($ps > 0.18$), which indicated a lack of a significant LEAD effect over time. That said, descriptive statistics indicated that these nonsignificant group differences were in the desired direction. Specifically, 73% of LEAD participants were criminally charged prior to evaluation entry compared to 45% subsequent to evaluation entry, whereas 70% and 57% of control participants were criminally charged prior and subsequent to evaluation entry, respectively.

When we considered group differences for felony charges, the ATE model was significant, Wald $\chi^2(4, N = 318) = 33.47, p < 0.001$. The time x intervention group interaction effect indicated a significant LEAD effect over time ($OR = 0.49, robust\ SE = 0.16, p = 0.03$). This finding indicated that, compared to control participants, LEAD participants had 39% lower odds of being charged with at least one felony subsequent to program entry. The ATT model, which indicated the treatment effect for the LEAD participants specifically, was significant, Wald $\chi^2(4, N = 318) = 34.85, p < 0.001$. Results indicated 36% lower odds of being charged with a felony subsequent to LEAD involvement, and this was reflected in a significant time x intervention group interaction ($OR = 0.47, robust\ SE = 0.16, p = 0.02$). See Fig. 3 for the percentage of participants charged with at least one felony in each group prior and subsequent to evaluation entry.

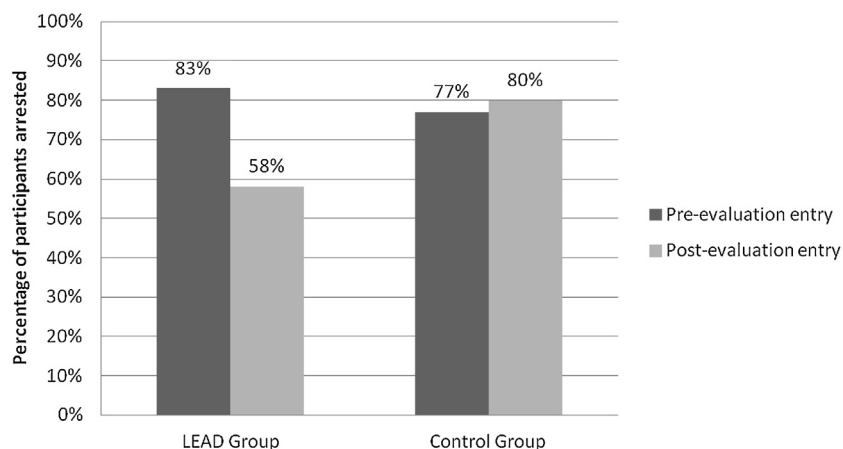


Fig. 2. Percentage of participants with ≥ 1 arrest over the entire LEAD evaluation (excluding index arrest).

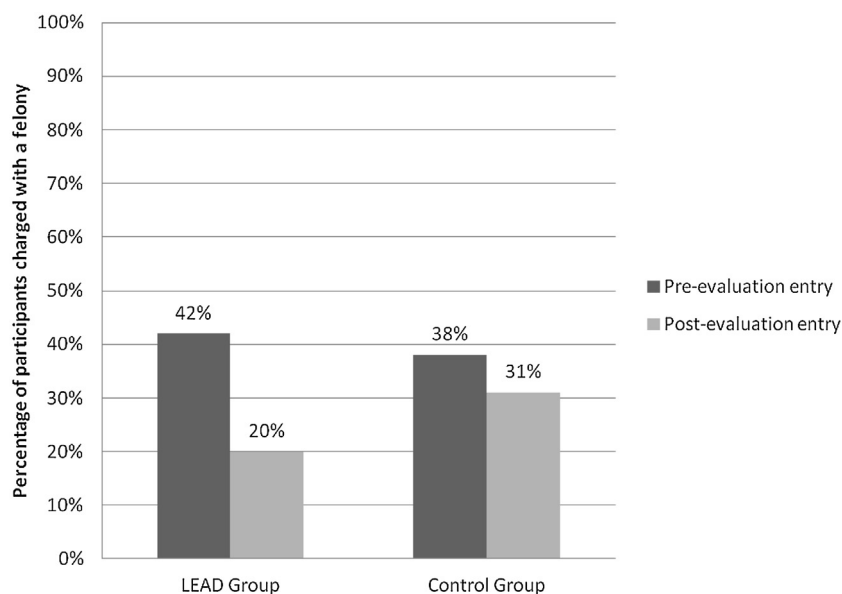


Fig. 3. Percentage of participants charged with ≥ 1 felony over the entire LEAD evaluation (excluding index charge).

4. Discussion

The LEAD program is reaching a recidivating population that has experienced the street-to-jail-to-street revolving door. Findings indicated that LEAD is associated with positive effects for some shorter- and longer-term recidivism outcomes.

4.1. Arrest outcomes

When looking at shorter-term, six-month arrest outcomes, there was a significant LEAD effect, which reflected the fact that the proportion of LEAD participants being arrested leveled off, whereas the proportion of control participants arrested increased. Over the longer term, however, these effects were more pronounced. When the time frame was expanded to include recidivism from the start of data collection (10/1/09) through the evaluation window (7/31/14), the proportion of LEAD participants arrested decreased, whereas the proportion of control participants arrested increased.

Taken together, arrest findings indicate positive LEAD effects on recidivism that are likely due to features of the LEAD program. LEAD case managers focus on building strong, trusting therapeutic alliances with LEAD participants. Case managers then provide LEAD participants with long-term, tailored case management that supports fulfilment of basic needs, including housing stability, job attainment, educational advancement, and enrollment in drug and alcohol treatment. Case managers also coordinate with prosecutors to ensure nondiverted cases are managed to support and not compromise LEAD intervention plans.

It is, however, important to consider other potential explanations for the patterns in the arrest findings. First, increases in the control group's odds of arrest following evaluation entry across all analyses are worth discussing. It is important to bear in mind that the Seattle West Precinct was subject to policy changes during the LEAD evaluation time period, which could have affected both the LEAD and control participants' rate of arrest. It is therefore possible that more focused enforcement—and not necessarily increased criminal activity—was responsible for increases in the prevalence of arrests in the control group. These larger, systemic changes, however, would not account for the LEAD group's drop in arrest prevalence, which would have been expected to reflect the same environmental conditions as the control group.

Another potential explanation for these findings is that officers could have made intentional decisions to avoid arresting LEAD

participants. Upon further consideration, however, this explanation is not highly probable. Only approximately 40 of 1300 Seattle Police Department officers were involved in the LEAD program. Further, few if any officers outside of the LEAD squads were aware of individuals' group assignment. There were neither department-wide communications/trainings about the program nor system flags visible to officers that would signal LEAD participation. Thus, we are confident the observed LEAD effect in reducing arrest is not primarily due to intentional differences in decision-making by officers.

4.2. Criminal charge outcomes

Over the 6-month follow-up, LEAD participants did not show statistically significant differences in odds of being charged with a crime or being charged with a felony crime. When considered over the longer term, however, LEAD participants had significantly lower odds of being charged with a felony compared to people experiencing the system as usual.

It should be noted that, in contrast to arrests, felony charges could have been affected by the decisions of LEAD stakeholders, particularly the Trial Unit Chief for the King County Prosecutor. As an unblinded operational partner, the prosecutor's office could take into account LEAD participation and progress in the program when deciding whether and when to file felony charges. Thus, the lower odds of felony charges among LEAD participants compared to control participants could have been precipitated by differential decision-making in the prosecutor's office.

4.3. Understanding these findings in the context of existing evaluations

The present findings are particularly meaningful when placed in the context of the existing literature on interventions targeting recidivism. For example, nationwide *meta*-analyses and systematic reviews have shown that some programs targeting recidivism, including mental health court, drug court and tailored psychosocial interventions, are superior to mainstream criminal justice processing across various outcomes. (Brown, 2010; Perry, Coulton, & Glanville, 2006; Scott, McGilloway, Dempster, Browne, & Donnelly, 2013) That said, systematic reviews of diversion programs more specifically have indicated mixed findings. Although the results of some studies conducted primarily with methamphetamine users in California, have indicated improved drug outcomes for participating individuals, there have been

null findings regarding recidivism and cost-effectiveness.

Closer to home, a recent Washington State Institute for Public Policy (WSIPP) evaluation found that existing evidence- and research-based approaches focusing on tailoring supervision to offender's relative risk level, motivation and needs had a small but significant collective effect ($d = -0.23$) and reduced recidivism by about 14 percentage points compared to traditional supervision. (Drake, 2013) It is notable that the current evaluation indicated LEAD had an even larger effect size ($d = -0.33$) and reduced recidivism by about 22 percentage points compared to the system as usual, which in King County, where this evaluation was conducted, includes various therapeutic courts. This evaluation therefore provides compelling support for LEAD—an innovative approach to reducing criminal recidivism—as a viable alternative to existing criminal justice system approaches.

4.4. Limitations

This evaluation's limitations should be noted. First, large administrative datasets often feature missing data and clerical errors. That being said, we have no reason to believe such errors asymmetrically affected LEAD participants versus control participants. Another limitation was the fact that we did not obtain HIPAA authorizations from participants, which made it impossible to analyze potential LEAD effects on health-care utilization.

Second, given real-world implementation realities, the originally planned randomization schema was relaxed, and a nonrandomized controlled design was employed in its place. To increase confidence in the causal impact of LEAD, both methodological and statistical approaches were used to balance the control and LEAD groups. For example, LEAD officers were trained on the application of the inclusion/exclusion criteria, and they made a systematic effort to identify qualifying LEAD and control participants using the same criteria. Further, there was no penalty to officers for excluding individuals from the evaluation based on the inclusion/exclusion criteria. LEAD squads were also consistent over the course of the evaluation for both control and LEAD groups; thus, the same officers were responsible for assessing all participants' inclusion/exclusion criteria over the course of the evaluation. Finally, we reduced the influence of potential selection bias using propensity score weighting, which is a statistical technique designed to ensure greater balance across groups and thereby decrease bias due to potentially confounding variables.

Third, descriptive sample analyses indicated some significant baseline differences between LEAD and control groups. Specifically, the LEAD group comprised more older and female participants. Because the groups were comparable in terms of recent criminal history, however, this difference does not seem likely to account for differences in post-entry recidivism. It is also worth noting that there was a higher proportion of African Americans in the control condition. Past arrest data suggest that drug arrests in the districts assigned to receive the control condition were more likely to involve African-Americans than those in the LEAD catchment area. Thus, the observed imbalance is more likely due to preexisting factors rather than officer behavior. Fortunately, with the exception of ATE for age, all baseline group demographic differences were successfully balanced by the propensity scores.

4.5. Conclusions and future directions

Findings indicated positive effects of the LEAD program on criminal recidivism over shorter six-month and longer evaluation-wide time frames. Specifically, the odds of arrests and felony charges were lower among LEAD versus control participants at the follow-up time points. The limitations of the current evaluation were ameliorated using both methodological and statistical approaches, which increased our confidence that the LEAD effects were due to the program itself and not other potentially confounding factors. Further analyses assessing the

effectiveness of the LEAD program compared to the system-as-usual control group on criminal and legal systems utilization and associated costs are planned. In the meantime, these initial findings indicate LEAD is a promising new approach that—compared to arrest, prosecution and incarceration—may better slow the street-to-jail-to-street revolving door.

Conflict of interest

The authors declare no conflicts of interest.

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