The Effectiveness of Public Safety Diversion Programs in Longmont, CO*

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Abstract

This paper evaluates the short-term effectiveness of the Law Enforcement Assisted Diversion Programs (LEAD) of the City of Longmont, Colorado, Division of Public Safety (LDPS). The LEAD program was implemented in July 2018 and, in the sixteen months between its inception and November 2019, it provided some form assistance to 133 individuals. Using a variety of estimates, I show that the number of all legal incidents (encompassing arrests, charges, court summons, use of force, driving under the influence and being a suspect or a victim) dropped by around 59 percent following first contact with LEAD and arrests declined by roughly 50 percent. I further document that all types of LEAD contact reduced legal incidents by 21 percent, LEAD case management contacts by around 17 percent and peer counseling contacts by 11 percent. Likewise, all LEAD contacts reduced arrests by 20 percent, case management contacts led to declines of about 19 percent and peer counseling contacts by 12 percent. The impact of LEAD programs on Emergency Medical Services use is more mixed, although there is some suggestive evidence that the frequency of hospitalizations might have declined by about 25 percent via peer counseling contacts.

Keywords: Law Enforcement Assisted Diversion, Recidivism, Arrests, Health, Policy Evaluation.

JEL Classifications: I0; I1; I3; K0.

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1 Introduction

The City of Longmont, in Boulder County, Colorado, has experienced a rapid increase in the number of residents who struggle with substance use and co-occurring disorders. There is a lack of accessibility to treatment providers and facilities causing individuals to frequent the emergency rooms, jail or to leave the community to receive necessary treatment. To address this need, Longmont Division of Public safety (LDPS), partnered with multiple agencies, including the Boulder County District Attorney (BCDA), Boulder County Public Health (BCPH), Public Defenders office, the Municipal Court, local hospitals and treatment providers to implement Crisis Outreach Response and Engagement (CORE), a Co-Responder crisis intervention program, and Law Enforcement Assisted Diversion Program (LEAD), a law enforcement assisted diversion program. These programs were launched on July 10, 2018 and, along with intensive case management services, they make up the Public Safety Diversion Programs which targets individuals within the City of Longmont who are struggling with substance use and/or co-occurring disorder.

CORE seeks to divert individuals with behavioral health conditions from the criminal justice system and the emergency room and into treatment and other supportive services. The CORE team is a primary response co-responder team that is dispatched to mental health related 911 calls. Once on scene, members apply their specialized skill sets to provide triage and assessment, crisis de-escalation, field clearances and direct transport to an appropriate destination. When not actively engaged on scene, personnel actively perform outreach and follow-up activities.

Likewise, LEAD also intends to reduce criminal justice involvement and behavior, and improve community safety by connecting low-level drug offenders to harm reduction based intervention and community-based supportive resources. Under LEAD, law enforcement officers identify people with substance use or behavioral health motivated crimes and offer them, in lieu of arrest, an opportunity to participate in a harm reduction and intensive case management program. CORE and LEAD are both backed by peer case management services which provide intensive ongoing engagement and follow up throughout the participant's program involvement in order to support progress and recovery. Peer Case Managers contact inpatient treatment facilities in the surrounding area to determine availability, arrange for admission for individuals, as appropriate, and provide transportation to and from out of county detox and treatment centers. They also help individuals connect to community-based resources, transition back into

the community, and into outpatient treatment.[1]

This paper evaluates the effectiveness of Longmont Division of Public Safety's Law Enforcement Assisted Diversion Program. The evaluation period spans from January 2015 to November 2019, covering nearly a five year timeframe around the launch of LEAD on July 10, 2018. From its inception to November 2019, Longmont's LEAD program provided assistance to 133 city residents in a host of different ways.

The objective of this paper is to assess the effectiveness of LEAD interventions and social services on a host of outcomes, such as behavioral change, disorder prevalence, duration and robustness of the former as well the socioeconomic and demographic conditions that might account for any potential effects LDPS's Public Safety Diversion Programs including LEAD services might be generating.

Based on the sample of individuals and period of coverage, I document that rates of "recidivism" in terms of adverse contacts with law enforcement and the legal system declined as a result of LEAD interventions and interactions. In particular, I show that the number of all legal incidents dropped by around 59 percent following first contact with LEAD and arrests declined by roughly 50 percent. I further document that all types of LEAD contact helped to reduce legal incidents by 21 percent, LEAD case management contacts account for roughly a 17 percent decline and peer counseling contacts for about 11 percent. Likewise, all LEAD contacts reduced arrests by 20 percent, case management contacts led to declines of about 19 percent and peer counseling contacts by 12 percent. The impact of LEAD programs on Emergency Medical Services use is more mixed although I document that the frequency of hospitalizations might have declined by about 25 percent via peer counseling contacts.

2 The Data & Summary Statistics

The City of Longmont launched its Law Enforcement Assisted Diversion program on July 10, 2018 and it has been collecting detailed individual-level data on subjects who have been in contact with a host of city social services a number of years prior to that date. All of the demographic data utilized in this study come from this database which is maintained by Civicore Data Platform.

In order to evaluate various outcomes and behaviors prior to contact with the LEAD pro-

[1] https://www.longmontcolorado.gov/departments/departments-n-z/public-safety-department/community-programs

gram, we chose to set the start date at January 1 2015. The data extend through the end of November 2019. For the 133 individuals who were provided LEAD assistance since July 2018, we then gathered data covering a host of outcomes and behaviors from the legal system and the City of Longmont Emergency Medical Services over the same time span (on which I elaborate further below).

The summary statistics of a host of descriptive and explanatory variables are listed in Table 1.A and some of the key raw correlations are shown in Table 1.B. As far as the key demographic profiles are involved, the 133 Longmont residents who were serviced through LEAD programs since July 2018 were, on average, 37.2 years old, more than 60 percent male, and they had slightly more than a GED (although educational attainment is recorded for only 59 respondents and is lacking for 74 others). As shown in Table 1.A, slightly above 66 percent of this sample were Caucasian, 13 percent Hispanic, 1.5 percent were African American, under 1 percent were Native American, while the remaining 19 percent did nor report their race. Around 36 percent of program participants were homeless. Married LEAD program participants comprised of 8.3 percent of the sample, while never married accounted for 24 percent, divorced another 14.3 percent, and separated and cohabiting individuals accounting for 3 percent each, respectively.

Turning to some of the main LEAD interactions, the total number of LEAD contacts with each subject was, on average, 14.6 annually, although with high variance such that the total number of contacts with LEAD participants ranged from a low of zero to a maximum of 144 per year over the sample period from the program inception in July 2018 to November 2019. Of the total number of 14.6 interactions per participant per year, 4.4 were case management contacts, .356 were counseling contacts, 2.8 were peer counseling contacts, and 1.8 were solely informational contacts.

In terms of some of the main outcomes variables we shall focus on below, we see in Table 1.A that the total number of *legal entanglements* among the 133 LEAD participants over the whole sample period of January 1, 2015 and November 30, 2019, broke down as follows: an average of 9.5 legal incidents per participant per annum before LEAD contact and 3.9 incidents after LEAD contact. Two arrests on average per year before LEAD contact and one after LEAD involvement. These 133 program participants were also involved in a total of roughly 4.6 Emergency Medical Services contacts per year before LEAD and 2.1 after LEAD. Of those, 4.3 culminated with hospitalization before LEAD and 2 after it.

[Table 1.A.]

Turning to Table 1.B, we see that there is a discernible positive correlation between being male or homeless with all types of LEAD contacts as well as EMS contacts and hospitalizations. Both arrests and hospitalizations before the LEAD program launch are correlated strongly positively with arrest and hospitalizations after LEAD launch, respectively. As well, hospitalizations and arrests are also positively and highly correlated.

[Table 1.B.]

3 Overall Program Impact & Baseline Means Tests

Starting with the overall impact of LEAD program participation on some of the key outcomes we already reviewed, Table 2 lists means comparison tests of the difference in outcomes before and after program participation and Figure 1 plot the means and 95-percent confidence intervals of differences in key outcomes.

The six main variables of interest are: numbers of total legal incidents, arrests, legal charges, court summons, contact with EMS and EMS that culminated with hospitalizations.[2] As shown, we can confidently reject that the means of total legal incidents and arrests remained unchanged after LEAD involvement. For both of these means, t-tests in fact find strong statistical support for sizable declines. The decline in total legal incidents is in excess of 59 percent and that in the number of arrests is more than 50 percent. By contrast, the means tests suggest that being charged as a suspect declined to a lesser extent (by around 24 percent) and did not attain a conventional level of statistical significance. And that of court summons stayed virtually flat. For emergency medical services contacts and those that ended with hospitalizations as well, the means tests once again suggest declines after LEAD program involvement. Both of these show declines on the order of around 52 percent, although the confidence intervals register only marginally within the thresholds of conventional statistical significance.

[Table 2 and Figure 1.]

In Table 3.A, I list the numbers of LEAD participants according to some distinct thresholds based on their behaviors before and after LEAD program contact. Specifically, for each of the key six outcome variables mentioned above, Table 3 lists the numbers of individuals who (a)

[2] All of these figures are annualized for ease of comparison.

engaged in the outcome of interest at least once before LEAD contact but stayed incident free after LEAD contact; (b) were free of an incident before LEAD contact but engaged in the outcome of interest at least once after LEAD contact; (c) stayed incident free both before and after LEAD contact; and (d) were engaged in the outcome of interest at least once both before and after LEAD involvement.

Here we see clear evidence that the fraction of individuals who had some record of involvement prior to LEAD contact and had none following it significantly outweighs the share of those who did not have any involvement prior to a LEAD affiliation and had some after it. This is the case across the board, although most marked differences can be seen for total legal incidents, arrests, summons and hospitalizations. To take two examples, we see that the share of LEAD participants who had an earlier arrest record but who remained free of an arrest after joining the program was 35 percent. By contrast, only 8 percent of the participants who did not have an earlier arrest record (over January 2015 and first contact with LEAD) ended up with one after LEAD involvement. Court summons also show such significant differences: the fraction of LEAD participants who had court summons earlier but who remained free of one after the program was 32 percent. Only 6 percent of the whole sample were subjects who did not have an earlier record but ended up with at least one court summons after LEAD involvement.

[Tables 3.A.]

The program participants have some record of a legal incident that predates their first LEAD contact, on average, by nearly 18 months and their tenure with LEAD after first contact is, on average, 8.6 months. Thus, the proportions listed in our previous table could be biased downward due to the relatively shorter period of time after LEAD involvement. In particular, if the incidence frequency among LEAD subjects who had a record before LEAD and had none after it is distinctly lower than those who had records both before and after LEAD, then the relatively higher proportion of individuals in the former category may well be driven by the shorter period of observations after LEAD contact.

Table 3.B addresses this concern and documents two salient facts: First, the average frequency of all legal incidents or arrests prior to LEAD involvement are remarkably similar across the two groups of populations. As shown, the annualized number of legal incidents amongst LEAD subjects who had a prior record and had none after is 10.7 whereas it is 11.1 for those who had incidents both before and after LEAD contact. Similarly, the annualized number of

arrests for those LEAD subjects with a clean sheet after is 3.41 whereas it is 3.34 for those who had incidents both before and after LEAD contact. This alleviates the concern that the data reported in Table 3.A are an artifact of the shorter average period of time of involvement after LEAD contact. Second, also note the distinctly lower frequency of all legal incidents amongst those who have records of legal entanglement both before and after LEAD (shown in the fifth and sixth rows of Table 3.B). This is some further suggestive evidence that LEAD led to lower legal incidents even amongst those who "relapsed" after LEAD.

[Tables 3.B.]

4 Accounting for LEAD Impact & Its Channels

The means tests summarized above suggest that recidivism as measured by legal incidents in general and arrests and court summons in particular declined markedly after first contact with LEAD. To a much weaker extent they offer some evidence that LEAD contacts may have reduced emergency medical services and fire department contacts that ended with hospitalizations. In what follows, I more systematically investigate and account for how LEAD contacts might have affected outcomes. As well, I explore the channels through which LEAD involvement could have lead to lower recidivism.

4.1 Empirical Methodology & Baseline Results

With the data at my disposal, I explore if the frequency and specific types of LEAD contact with clients after program inauguration helped to reduce likelihood of recidivism in various types of legal entanglements, such as arrests, charges or summons, as well as likelihood of EMS contacts that led to hospitalizations or other health interventions.

Our goal is to identify if rates of recidivism decline after individuals are referred to LEAD programs and they are subsequently counseled, monitored and consulted. The baseline empirical results below use the actual first contact dates by LEAD in estimating the differences in recidivism outcomes before and after LEAD interventions and based on various measures already discussed above.

On this basis, the baseline empirical specification I employ is as follows:

$$\text{Change in Behavior}_i \;=\; \text{LEAD Interactions}_i \;+\; X_i \;+\; \varepsilon_i \tag{1}$$

where the dependent variable, Change in Behavior$_i$, will alternatively be the (annualized) outcome variables of interest mentioned above differenced around the date of first contact between the individual and LEAD services. These include the changes in the number of total legal incidents, arrests, summons, as well as likelihood of EMS contacts that led to hospitalizations or other health interventions (and all differenced around the date of first contact with LEAD).[3]

The main explanatory variable of interest, LEAD Interactions$_i$, is the frequency of the various kinds of LEAD program interactions the participant had after his or her first contact with LEAD services. X_i encompasses various individual specific demographic controls, such as age, education level, gender, race, marital status and a homelessness indicator.

4.2 LEAD Impact on Legal Entanglements

4.2.1 Recidivism in All Legal Incidents

Table 4.A presents some baseline estimates based on equation (1) and where the left-hand-side variable is the number of arrests after first contact with LEAD. The right-hand-side variables include a host of demographic controls, such as age of the subject at the time of first LEAD contact, Age$_i$, a gender dummy, Male$_i$, an education indicator that takes on a value between zero and seven,[4] two marital status indictor dummies, Married$_i$ and Never Married$_i$, a homelessness indicator, Homeless$_i$, and two dummies for race, Caucasian$_i$, Hispanic$_i$.

In column (1) of Table 4.A, we see the estimates where the main explanatory variable, LEAD Interactions$_i$, is the annualized total number of contacts between the participant and LEAD after the individuals' first contact with LEAD programs.[5] As shown, being male exerts a positive and statistically significant effect on the difference in legal incidents before and after first contact with LEAD. Likewise, higher levels of education are also shown to have a positive and significant effect on the difference in legal entanglements, while being Hispanic leads to a lower and statistically significant impact. None of the other control variables show any statistically significant effect on the difference in all legal entanglements before and after contact with LEAD.

[3] For example, for a person whose first contact with LEAD services was on November 6, 2018, we look at the difference in the number of arrests after November 6, 2018 and before when the dependent variable is the change in arrests.

[4] The Education$_i$ indicator takes on the value of zero for missing data, one for some high school, two for a GED diploma, three for high school completion, four for some college enrollment, five for a two-year college degree, six for a college degree and seven for higher levels of education.

[5] Recall, as we reviewed in Section 3 above, that LEAD interactions with participants take various forms and methods. These include but are not confined to case management contacts, counseling contacts, peer contacts, contacts for informational updates, and outreach.

As far as the impact of our key explanatory variable in column (1), Total LEAD Contacts$_i$, we find that they had a negative and statistically significant effect on the difference in all legal entanglement. Specifically, Total LEAD Contacts$_i$ comes in with a coefficient of $-.139$ which is statistically significant at the 95 percent confidence level. Given that each program participant had on average 14.6 total LEAD contacts per year after they were referred to LEAD and the incident frequency among the 133 program participants was an average of 9.54 total number of legal entanglements per year before they were enrolled in LEAD programs, one can conclude that case management contacts helped to reduce legal issues about 11 percent on average (i.e. $-.139\ x\ 14.6 = -2.03$ which is about 21 percent of 9.54).[6]

The second column of Table 4.A repeats the exercise in column (1) replacing the *total number* of LEAD contacts with the frequency of LEAD *case management contacts* between the participant and LEAD between the individuals' first contact with LEAD services and the end of November 2019. As shown, all other control variables influence the dependent variable along similar the lines of those reported in column (1). That is, being male positively and statistically significant effects the difference in legal incidents before and after first contact with LEAD. As well, higher levels of education have a positive and significant effect on the difference in legal entanglements, while being Hispanic lowers it statistically significantly. And as in column (1), none of the other demographic controls statistically significantly predict the difference in all legal entanglements before and after first contact with LEAD.

We once again find that the frequency of LEAD case management contacts with program participants had a statistically significant negative influence on arrests subsequent to LEAD involvement. In particular, we see that Case Management Contact$_i$ comes in with a coefficient of $-.376$ which is statistically significant at the 95 percent confidence level. Given that each program participant had on average 4.37 LEAD case management contact per year after they were referred to LEAD and the incident frequency among the 133 program participants was an average of 9.54 total number of legal entanglements per year before they were enrolled in LEAD programs, one can conclude that case management contacts helped to reduce legal issues about 17 percent on average (i.e. $-.376\ x\ 4.37 = -1.64$ which is about 17 percent of 9.54). This amounts to a quantitatively sizable reduction in legal cases reported over a relatively short program implementation period of slightly over 16 months (between July 10, 2018 and November 30, 2019).

[6] Please see Table 1.A for the annual averages in legal incidents before and after LEAD contact and LEAD interactions such as the average case management contacts per year.

The three columns that follow on Table 4.A redo this exercise with three alternative LEAD contacts as the key explanatory variable: the frequency of LEAD counseling contacts, peer counseling contacts and informational contacts, respectively. As before, we see that the same demographic factors which had an influence on the left-hand-side variable in columns (1) and (2), such as being male or Hispanic, still exert the same kinds of effects in the final three columns of Table 4.A.

Most importantly, however, we see that all types of LEAD contact had negative effects on legal entanglements after enrollment in LEAD programs. In addition, the frequency of LEAD peer contacts, shown in column (4) had negative, quantitatively large and statistically significant effects on legal entanglements after LEAD involvement.

Specifically, note that *Peer Contacts*$_i$ carries a coefficient of $-.395$ in the fourth column and registers a 95 percent confidence significance level. Since LEAD program participants had 2.76 peer contacts annually after they were enrolled in LEAD, on average, and the record of legal incidents among the 133 program participants was an average of 9.54, we can deduce that LEAD peer counseling contacts helped to reduce number of arrests over our sample period by roughly 11 percent on average (i.e. $-.395\ x\ 2.76 = -1.09$ which is about 11 percent of 2.14).

In column (5), we repeat our estimates this time utilizing LEAD informational contacts with program participants as the main explanatory variable. Unlike the effects of LEAD case management, counseling or peer contacts on recidivism in all legal incidents, we find that the frequency of informational contacts with program participants did not exert any statistical significant impact on arrest recidivism (although we still see a negative point estimate).

[Table 4.A.]

4.2.2 Recidivism in Arrests

Next, we examine if and whether LEAD program contacts affected *arrests*. The empirical estimates of equation (1) using the difference in the number of arrests before and after the date of first contact with LEAD as the dependent variable are presented in Table 4.B.

The right-hand-side controls employed are identical to the ones utilized in Table 4.A and the five columns shown here examine the five different types of LEAD contact employed in the five columns of the previous table, respectively. Overall, the results with respect to recidivism in arrests are very much in line with those found with recidivism in all legal incidents in Table 4.A.

Specifically, we see in column (1) that the total number of LEAD contacts, once again, exhibits a negative and statistically significant effect here. Total LEAD Contacts$_i$ comes in with a coefficient of $-.028$ which is statistically significant at the 95 percent confidence level. Given that each program participant had on average 14.6 total LEAD contacts per year after they were referred to LEAD and the arrest frequency among the 133 program participants was an average of 2 per year before they were enrolled in LEAD programs, one can conclude that case management contacts helped to reduce arrests about 20 percent on average (i.e. $-.028\ x\ 14.6 = -.408$ which is about 20 percent of 2).

As in Table 4.A, the estimates in column (2) of Table 4.B attribute a fairly large negative and statistically significant impact of LEAD case management contacts on the number of arrests following LEAD program involvement. Based on the estimates in column (2), one can deduce that LEAD case management contacts after first contact with LEAD helped to reduce arrests by around 15 percent. Specifically, *Case Management Contacts*$_i$ comes in with a coefficient of $-.086$ in the second column of Table 4.B and registers a 95 percent confidence significance level. Since each LEAD program participant had, on average, 4.37 case management contacts annually after they were enrolled in LEAD, and the record of arrest among the 133 program participants was an average of 2 annually, we can deduce that LEAD case counseling contacts helped to reduce number of arrests over our sample period by roughly $-.086\ x\ 4.37 = -.376$ which is about 19 percent of 2.

Likewise, the estimates using LEAD peer contacts as the main explanatory variable (presented in column (4)) also assign a statistically significant and negative effect of LEAD peer counseling contacts on recidivism, as measured by the difference in the number of arrests following LEAD program involvement. Taking the estimates in column (4), for instance, one sees that an additional counseling contact reduced annual arrests by .089. Combined with the fact that a LEAD program participant was contacted via peer counseling 2.76 times annually, and the number of arrests was 2 per year, on average, we find that LEAD peer counseling reduced the number of arrests by roughly 12 percent annually.[7]

[Table 4.B.]

[7] That is based on $-.089\ x\ 2.76 = -.256$ which is about 12 percent of 2.

4.2.3 Recidivism in Court Summons

In the next table, we turn to an examination of how LEAD contacts might have affected recidivism in court summons. As shown, all five types of LEAD interactions carry negative coefficients, indicating negative effects on court summons after first contact with LEAD programs. And in line with the results in our previous two tables, the effects of total LEAD contacts (in column (1)), case management contacts (column (2)), and peer counseling contacts (Column (3)) all come in with statistically significant coefficients. Based on the estimates, one can derive as we have done with our previous results that total LEAD contacts, case management contacts and peer counselings led to declines in the average annualized number of court summons on the order of nearly 100 percent, 87 percent and 50 percent, respectively.

[Table 4.C.]

Similar regressions were ran using alternative legal data as dependent variables. These included number of vehicular incidents while driving under the influence, number of cases in which the individual was cited for use of force, incidents in which he or she was reported as a missing person. These estimates were inconclusive, most likely on account of the fact that the number of cases reported in these categories were very small.

4.3 LEAD Impact on EMS Contacts

In the next two tables, Tables 5.A and 5.B, I evaluate the potential impact of LEAD programs involvement on contact with Emergency Medical Services. Here we have a breakdown of how contact with EMS culminated. Namely, whether the individual was hospitalized, treated and released or refused treatment. However, the number of cases in which recipients were treated and released or refused treatment was fairly small, potentially leading to attenuation biases. As a result, I focus on two outcomes involving the Emergency Medical Services and the Longmont Fire Department: the total count of incidents involving EMS and hospitalizations.

4.3.1 Recidivism in Overall EMS Contacts

Table 5.A presents results utilizing the number of EMS contacts as the main dependent variable. The five columns shown control for the same independent variables employed in our previous tables and in that respective order.

In all five estimates shown, LEAD contacts generate a positive effect on differences in outcomes, with the impact of LEAD counseling (shown in column (3)) and informational contacts (listed in the final column) also attaining statistical significance. None of the other control variables produce statistically significant and consistent effects on total EMS contacts after LEAD involvement, although Hispanics had lower EMS involvements after first LEAD contact and homelessness is associated with a higher likelihood of contacts with emergency medical services and the fire department.

Based on the estimates in column (3), an additional LEAD counseling contact raises the number of EMS contacts by .278. Taking the fact that a LEAD program participant was contacted for LEAD counseling roughly .356 times annually, and the number of EMS contacts was before LEAD involvement was .664, on average annually, we find that LEAD counseling raised the number of total EMS contacts by around 15 percent.[8]

In similar fashion, the final column in Table 5.A also shows that informational contacts are associated positively and significantly with the number of EMS contacts. Based on the estimates shown, informational contacts between LEAD and participants seem to generate a 34 percent increase in EMS and fire department contacts after LEAD involvement.[9]

Based on the positive effects presented in Table 5.A overall, and those in columns (3) and (5) in particular, one could hypothesize that LEAD program involvements often follow a higher propensity of EMS contact (thereby leading to the positive associations shown in Table 5.A) and are mostly in attempts by LEAD staff and employees to monitor the health and progress of participants who required EMS support.

[Table 5.A.]

4.3.2 Recidivism in Hospitalizations

Table 5.B then turns to an empirical assessment of the number of hospitalizations as the main dependent variable. As in all our previous estimates, the five columns shown control for the same independent variables employed in our previous tables and in the same order.

The main take away here is that the results are pretty much in line with the findings using the total number of EMS contacts. In all five estimates shown, LEAD contacts generate a positive effect on differences in outcomes, with the impact of total LEAD contacts in column

[8] Based on .278 x .356 = .099 which is about 15 percent of .664.
[9] This is based on .127 x 1.77 = $-$.099 which is about 34 percent of .664.

(1), counseling interactions in column (3), and informational contacts in column (5) yielding statistical significance. As in Table 5.B, none of the other control variables produce statistically significant and consistent effects on total EMS contacts after LEAD involvement, although homelessness is associated with a higher likelihood of contacts with emergency medical services and the fire department and being Hispanic reduced the chances of such contacts.

To quantify the effects that are significant, we see in column (1) that an additional LEAD contact of any type raises the number of EMS contacts by .0143. Taking the fact that a LEAD program participant was contacted about 4.37 times annually on average, and the number of hospitalizations was before LEAD involvement was .612, on average annually, we find that total LEAD contacts raised the number of hospitalizations by about 10 percent.[10]

Turning to column (3), an additional LEAD counseling contact raises the number of EMS contacts by .308. Taking the fact that a LEAD program participant was contacted for LEAD counseling roughly .356 times annually, and the fact that each program participant was hospitalized .612 times per year, on average, we find that LEAD counseling raised the number of total EMS contacts by around 5.5 percent.[11]

The final column in Table 5.B also shows that informational contacts positively and significantly raised frequency of hospitalizations after LEAD involvement. Here we get about a 40 percent increase in hospitalizations after LEAD involvement.[12]

Based on the positive effects presented in Table 5.A overall, and those in the odd numbered columns in particular, one could once again surmise that LEAD contacts follow more hospitalizations due to attempts by LEAD staff and employees to improve the health and progress of participants who required EMS support.

[Table 5.B.]

5 Alternative IV Estimates

In deriving the above discussed empirical findings, I looked at the difference in outcomes before and after the date of first contact recorded between the individual and LEAD services staff. It is, however, clear that the actual dates of referral and, hence, first contact are clearly specific to individuals and their circumstances (i.e., they are endogenous) and participation in LEAD

[10] This is based on .0143 x 4.37 = .0625 which is about 10 percent of .612.
[11] Derived as .308 x .356 = .0338 which is 5.5 percent of .612.
[12] This is based on .137 x 1.77 = .243 which is about 40 percent of .612.

program is voluntary. As a result, the use of the actual dates of referral and LEAD program involvement could contaminate and bias the estimates of the impact of LEAD involvement on recidivism rates.

For example, LEAD services became available on July 10, 2018 in Longmont and they began to be offered to all individuals contacted by the Longmont police thereafter. Nevertheless, program participation is voluntary and referral to LEAD does not trigger an automatic LEAD involvement. Individuals have the right to refuse such services and their willingness to accept enrollment in LEAD services and its timing could be driven by unobservable and important factors, such as a stronger desire on the part of the subjects to make a lasting and positive change in their lives. Thus, given that such individual and unobservable factors might be important in if and when an individual enrolls in LEAD, they could also be important in leading to reduced rates of recidivism. In such a case, however, the statistics would be biased and misleading because it would attribute the positive outcomes to LEAD interventions, whereas the actual driver of the change is the change in unobservables that lead to acceptance and enrolment in LEAD services in the first place. Consequently, this could cast some doubt on the validity of the estimates and their results discussed in the preceding section.

One effective way to deal with this empirical issue is to randomize who is offered LEAD services. Randomization using a large sample ensures that both the treatment group (those who enroll in LEAD) and the control group (those who do not) are identical in their observable and unobservable traits. This was the approach taken—at least in the early stages of the program launch—when Seattle's LEAD program was being evaluated by Collins et al. (2015, 2017). There, police officer shifts were randomized so that LEAD was offered during some random shifts and they were not during others.

By contrast, LEAD services in Longmont were offered universally to all those who became eligible through police encounters or legal affairs after the launch of LEAD on July 10, 2019.[13] Thus, randomization of access to LEAD for evaluation purposes was not employed.

In order to deal with this issue, and as alternative estimates for the baseline analyses I presented above, I alternatively used the launch date of LEAD programs on July 10, 2018 as an instrument and examined differences in the same outcomes evaluated above based on individual behavior before and after the LEAD program launch on July 10, 2018.

[13] In effect, there were three cases who were referred to LEAD within a month prior to the official program launch.

5.1 Reestimating LEAD's Impact on Legal Incidents

The analogs of Tables 4.A through 5.B are presented in Tables 6.A through 7.B below, respectively. The upshot is that, while some results weaken compared with those already reported in the previous section, the qualitative nature of the findings remain intact.

Take, for instance, the impact of LEAD on the number of all legal entanglements as measured by the difference in all legal incidents before and after the LEAD launch date of July 10, 2018. Here, as in Table 4.A, the impact of all five types of LEAD contact come in with the expected negative sign, attaining statistical significance in column (4) where the explanatory variable is the annual number of LEAD peer contacts.[14]

Based on the estimates in this column, one can calculate that LEAD peer contacts, which were 2.76 annually on average, reduced the annual number of all legal entanglements by .0431 $*$ 2.76 = .119. Given that there was an average of 9.54 legal incidents annually prior to contact with LEAD across all of the 133 program participants, this comes out to a reduction in the annual number of all legal entanglements by around 1 percent due to LEAD peer contacts.

While this is vastly lower than the 11 percent reduction estimated using the actual first LEAD contact dates for program participants (shown in column (4) of Table 6.A), it should be considered as a lower bound on the impact of LEAD peer contacts on reductions in all legal incidents. The reason is that the estimates in this section use the LEAD launch date of July 10, 2018 as the instrument for program "treatment" for all individuals. However, in reality, individuals' dates of first LEAD involvement were typically much later than the program launch date. And with later actual program enrollment dates, the amount of time over which individuals were enrolled in LEAD programs was much shorter than these latter estimates take into account, thereby dampening any potential true effect.

[Table 6.A.]

Turning to an evaluation of the impact of LEAD interactions on the difference in the frequency of arrests before and after program launch on July 10, 2018, we not only see results that are consistent with those reported earlier in Table 4.B, but also much stronger statistically. In fact, all but one of the five types of LEAD contact with program participants are shown here to have statistically significantly lowered the frequency of arrests after LEAD program

[14] As far as the other control variables are concerned, here we see that total legal incidents declined statistically significantly with age in all specifications.

involvement. Doing an accounting of the impact of each of the four types of LEAD contact with statistically significant effects, one can verify that the annual number of arrests declined by 24 percent due to total LEAD contacts; 20 percent from case management contacts; 14.6 percent due to peer counseling; and 13 percent on account of informational contacts.

These are quantitatively very large effects, despite the fact the instrument used in these regressions for the date of LEAD program contact dampen the estimated program impact. In fact, comparing the three statistically significant effects of total LEAD contacts, case management and peer contacts in Table 4.B with the respective specifications here in Table 6.B, we see that the estimated reductions in the number of arrests annually due to these three types of contacts are fairly close: for overall contacts they are 20 and 24 percent, for case management contacts they are in the ballpark of about 19 or 20 percent and, for peer contacts, roughly 12 and 13 percent, respectively.[15]

[Table 6.B.]

Rerunning the regressions shown in Table 4.C but this time using the LEAD launch date as our instrument, we do not find any conclusive evidence that the five types of LEAD contact exerted any meaningful effect on the annual number of court summons amongst the LEAD program participants.

[Table 6.C.]

5.2 Revisiting LEAD's Impact on EMS Contacts & Hospitalizations

In the following two tables, we rerun the regressions presented in Tables 5.A and 5.B with the timing of first LEAD interactions instrumented by the launch date of the program on July 10, 2018. This yields results that are very similar for both overall EMS contacts and hospitalizations. Namely, results in both Tables 7.A and 7.B negative effects in four of the five regressions and produces a statistically significant and negative impact for peer contacts.

[Tables 7.A and 7.B.]

[15] According to Table 4.B, recall that we found total contacts lowered arrests by 20 percent, case management contacts reduced them by 19 percent and peer contacts by 12 percent. By contrast, we see in table 6.B that total LEAD contacts reduced arrests by 24 percent, case management contacts reduced them by 20 percent and peer contacts by 13 percent.

As a final line in inquiry, I also examined if the impact of the various kinds of LEAD contact discussed in Sections 4 and 5 are conditional on certain demographic factors, such as gender, age, marital status or housing status (i.e., being homeless). In particular, one can conjecture that LEAD interventions, such as counseling sessions and peer contacts, had variable effects based on demographics. However, I could not identify such variable effects based on any of the demographic factors that are accounted for in the above analysis.

6 Conclusion

This paper analyzed the effectiveness of the Law Enforcement Assisted Diversion Programs (LEAD) of the City of Longmont, Colorado, Division of Public Safety (LDPS) which was launched on July 10, 2018. In the sixteen months between its inception and the end of November 2019, LEAD provided some form assistance to 133 individuals.

Using a variety of estimates, I show that the number of all legal incidents (encompassing arrests, charges, court summons, use of force, driving under the influence and being a suspect or a victim) dropped by around 59 percent following first contact with LEAD and arrests declined by roughly 50 percent. I further document that all types of LEAD contact reduced legal incidents by 21 percent, LEAD case management contacts by around 17 percent and peer counseling contacts by 11 percent. Likewise, all LEAD contacts reduced arrests by 20 percent, case management contacts led to declines of about 19 percent and peer counseling contacts by 12 percent. The impact of LEAD programs on Emergency Medical Services use is more mixed, although there is some suggestive evidence that the frequency of hospitalizations might have declined by about 25 percent via peer counseling contacts.

The LEAD program continues to run in full force and the next goal of the research program is to revisit this analysis toward the end of 2020 when a more comprehensive assessment covering a longer-term time horizon can be carried out.

References

[1] **Collins, Susan, E., Heather S. Lonczak, and Seema L. Clifasefi.** (2015). "LEAD Program Evaluation: Recidivism Report," Harm Reduction Research and Treatment Lab, University of Washington–Habrorview Medical Center, Working Paper, March.

[2] **Collins, Susan, E., Heather S. Lonczak, and Seema L. Clifasefi.** (2017). Seattle's Law Enforcement Assisted Diversion (LEAD): Program effects on recidivism outcomes," *Evaluation and Program Planning,* 64, pp. 49-56.

[3] **Dole, Jenna and Linda Freeman.** (2018). "Evaluation of Santa Fe's LEAD Program: Criminal Justice Outcomes," New Mexico Sentencing Commission, Working Paper, August.

Table 1.A: Summary Statistics

Variable ($N = 133$)	Mean	St. Dev.	Min.	Max.
Age	37.2	11.6	19	70
Education	1.16	1.56	0	7
Male	.609	.490	0	1
Caucasian	.662	.475	0	1
Hispanic	.128	.335	0	1
African American	.015	.122	0	1
Married	.083	.276	0	1
Never Married	.241	.429	0	1
Divorced	.143	.351	0	1
Homeless	.361	.482	0	1
Pre-LEAD Legal Incidents	9.54	13.3	0	84
Post-LEAD Legal Incidents	3.94	13.1	0	144
Pre-LEAD Arrest Count	2.00	3.67	0	24
Post-LEAD Arrest Count	.997	2.69	0	20
Pre-LEAD Suspect Count	.871	2.25	0	12
Post-LEAD Suspect Count	.659	1.89	0	16.5
Pre-LEAD Summons Count	.993	2.31	0	12.7
Post-LEAD Summons Count	.1.04	8.39	0	96
Pre-LEAD EMS Contacts	4.59	19.6	0	180
Post-LEAD EMS Contacts	2.12	12.2	0	138
Pre-LEAD Hospitalizations	4.29	19.6	0	180
Post-LEAD Hospitalizations	2.04	12.2	0	138
Total LEAD Contacts	14.6	20.3	0	144
LEAD Case Mgmt. Contacts	4.37	8.14	0	50.1
LEAD Counseling Con.	.356	1.07	0	7.77
LEAD Peer Contacts.	2.76	7.91	0	59.3
LEAD Info Contacts.	1.77	2.86	0	16.2

All legal incidents and LEAD contacts are reported in annualized terms.

Table 1.B: Some Key Correlations

$N = 133$	Age	Edu.	Male	Cauc.	Homels.	Incidents	Arrests	EMS	Hospt.	Total Con.
Age	1									
Education	.135	1								
Male	.119	.061	1							
Caucasian	−.055	.172	.056	1						
Homeless	.178	.112	.228	.057	1					
Total Incidents	−.023	−.038	.004	−.015	.125	1				
Arrest Count	−.059	.007	.059	−.017	.172	.833	1			
EMS Contacts	.310	−.018	.101	.009	.170	.652	.385	1		
Hospitalizations	.298	−.011	.091	.018	.171	.633	.369	.997	1	
Total Contacts.	.194	.059	.177	.074	.127	.581	.368	.653	.640	1
Case Mgmt. Cont.	.219	−.028	.183	.032	.172	.569	.359	.648	.632	.935
Counseling Con.	.199	−.116	.124	.079	.206	.452	.205	.632	.633	.752
Peer Contacts	.177	.029	.226	.047	.085	.585	.394	.604	.592	.882

$N = 133$	Case Mgmt. Con.	Counseling Con.	Peer Con.
Case Mgmt. Con.	1		
Counseling Con.	.699	1	
Peer Contacts	.769	.631	1

Table 2: Mean Comparison t-tests

Variable ($N = 133$)	Mean	St. Dev.	95 % Confidence Intervals	
Pre-LEAD Legal #	9.54	13.3	7.26	11.8
Post-LEAD Legal #	3.93	13.1	1.70	6.18
Difference Tests		H_a :	$\text{mean}(\Delta) < 0$	$\text{mean}(\Delta) > 0$
			$\Pr(T < t) = .99$	$\Pr(T < t) = .00$
Pre-LEAD Arrest #	2.00	3.67	1.38	2.64
Post-LEAD Arrest #	.993	2.69	.531	1.45
Difference Tests		H_a :	$\text{mean}(\Delta) < 0$	$\text{mean}(\Delta) > 0$
			$\Pr(T < t) = .99$	$\Pr(T > t) = .00$
Pre-LEAD Suspect #	.871	2.25	.485	1.26
Post-LEAD Suspect #	.659	1.89	.334	.984
Difference Tests		H_a :	$\text{mean}(\Delta) < 0$	$\text{mean}(\Delta) > 0$
			$\Pr(T < t) = .81$	$\Pr(T > t) = .19$
Pre-LEAD Summons #	.993	2.31	.596	1.39
Post-LEAD Summ. #	1.04	8.39	$-.396$	2.48
Difference Tests			$\text{mean}(\Delta) < 0$	$\text{mean}(\Delta) > 0$
		H_a :	$\Pr(T < t) = .47$	$\Pr(T > t) = .53$
Pre-LEAD EMS #	4.59	19.6	1.23	7.95
Post-LEAD EMS #	2.12	12.2	.028	4.21
Difference Tests		H_a :	$\text{mean}(\Delta) < 0$	$\text{mean}(\Delta) > 0$
			$\Pr(T < t) = .91$	$\Pr(T > t) = .09$
Pre-LEAD Hospital #	4.29	19.6	.936	7.65
Post-LEAD Hospital #	2.04	12.2	$-.055$	4.13
Difference Tests		H_a :	$\text{mean}(\Delta) < 0$	$\text{mean}(\Delta) > 0$
			$\Pr(T < t) = .89$	$\Pr(T > t) = .11$

Table 3.A: Outcomes by Various Categorizations of Behavioral Change

$N = 133$	Totals	Percent
Pre-LEAD Incidents > 0 & Post-LEAD Incidents $= 0$	42	32
Pre-LEAD Incidents $= 0$ & Post-LEAD Incidents > 0	2	2
Pre-LEAD Incidents $= 0$ & Post-LEAD Incidents $= 0$	15	11
Pre-LEAD Incidents > 0 & Post-LEAD Incidents > 0	74	55
Pre-LEAD Arrests > 0 & Post-LEAD Arrests $= 0$	46	35
Pre-LEAD Arrests $= 0$ & Post-LEAD Arrests > 0	10	8
Pre-LEAD Arrests $= 0$ & Post-LEAD Arrests $= 0$	44	33
Pre-LEAD Arrests > 0 & Post-LEAD Arrests > 0	33	24
Pre-LEAD Suspect > 0 & Post-LEAD Suspect $= 0$	34	26
Pre-LEAD Suspect $= 0$ & Post-LEAD Suspect > 0	13	10
Pre-LEAD Suspect $= 0$ & Post-LEAD Suspect $= 0$	67	50
Pre-LEAD Suspect > 0 & Post-LEAD Suspect > 0	19	14
Pre-LEAD Summons > 0 & Post-LEAD Summons $= 0$	42	32
Pre-LEAD Summons > 0 & Post-LEAD Summons $= 0$	8	6
Pre-LEAD Summons > 0 & Post-LEAD Summons $= 0$	70	53
Pre-LEAD Summons > 0 & Post-LEAD Summons $= 0$	13	9
Pre-LEAD EMS > 0 & Post-LEAD EMS $= 0$	34	26
Pre-LEAD EMS $= 0$ & Post-LEAD EMS > 0	10	8
Pre-LEAD EMS $= 0$ & Post-LEAD EMS $= 0$	60	44
Pre-LEAD EMS > 0 & Post-LEAD EMS > 0	29	22
Pre-LEAD Hospital > 0 & Post-LEAD Hospital $= 0$	30	22
Pre-LEAD Hospital $= 0$ & Post-LEAD Hospital > 0	9	7
Pre-LEAD Hospital $= 0$ & Post-LEAD Hospital $= 0$	68	51
Pre-LEAD Hospital > 0 & Post-LEAD Hospital > 0	26	20

Table 3.B: Outcomes by Various Categorizations of Behavioral Change

	n	Mean	St. Dev.
Pre-LEAD Incidents > 0 & Post-LEAD Incidents $= 0$	42		
Pre-LEAD Incidents		10.7	12.1
Post-LEAD Incidents		0	0
Pre-LEAD Incidents > 0 & Post-LEAD Incidents > 0	74		
Pre-LEAD Incidents		11.1	14.6
Post-LEAD Incidents		7.03	16.9
Pre-LEAD Arrests > 0 & Post-LEAD Arrests $= 0$	46		
Pre_LEAD Arrests		3.41	4.67
Post-LEAD Arrests		0	0
Pre-LEAD Arrests > 0 & Post-LEAD Arrests > 0	33		
Pre_LEAD Arrests		3.34	3.66
Post-LEAD Arrests		3.37	4.52

Pre-LEAD period covers 17.8 months & Post-LEAD period covers 8.6 months on average.

Table 4.A: Baseline Results with All Legal Incidents

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Difference in Incidents (annual)				
EXP. VAR.:					
Total Contacts	-0.139**				
	(0.0621)				
Case Mgmt Con.		-0.376**			
		(0.162)			
Counseling Con.			-1.760		
			(1.354)		
Peer Contacts				-0.395**	
				(0.159)	
Info Contacts					-0.723
					(0.451)
Age	-0.0389	0.0208	-0.0649	-0.0429	-0.0198
	(0.0928)	(0.0790)	(0.113)	(0.0898)	(0.102)
Education	0.841*	0.372	0.678	0.859*	0.712
	(0.460)	(0.386)	(0.510)	(0.514)	(0.448)
Male	2.910**	3.127**	2.542**	3.810***	2.074
	(1.136)	(1.210)	(1.171)	(1.094)	(1.280)
Caucasian	-0.314	-0.122	-0.368	-0.383	-0.642
	(1.353)	(1.372)	(1.288)	(1.285)	(1.350)
Hispanic	-5.799**	-5.413*	-5.983*	-5.619**	-6.805**
	(2.877)	(2.763)	(3.339)	(2.834)	(3.327)
Married	-3.782	-2.698	-5.106*	-4.542	-4.060
	(3.096)	(3.177)	(2.912)	(3.049)	(3.247)
Never Married	-0.845	-0.758	-2.855	-1.869	-2.084
	(2.037)	(1.964)	(2.362)	(2.044)	(2.273)
Homeless	-1.603	-0.762	-1.337	-2.099	-0.878
	(1.493)	(1.525)	(1.529)	(1.453)	(1.413)
Observations	132	129	132	132	129
R-squared	0.160	0.173	0.131	0.187	0.134

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.B: Baseline Results with Arrests

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Difference in Arrests (annual)				
EXP. VAR.:					
Total Contacts	-0.0281*				
	(0.0153)				
Case Mgmt Con.		-0.0860**			
		(0.0381)			
Counseling Con.			-0.187		
			(0.351)		
Peer Contacts				-0.0886**	
				(0.0349)	
Info Contacts					-0.132
					(0.107)
Age	-0.00782	0.0134	-0.0168	-0.00725	0.00174
	(0.0282)	(0.0217)	(0.0319)	(0.0274)	(0.0276)
Education	0.222	0.0838	0.220	0.222	0.169
	(0.161)	(0.142)	(0.178)	(0.165)	(0.157)
Male	0.417	0.537	0.316	0.634	0.309
	(0.443)	(0.449)	(0.447)	(0.438)	(0.458)
Caucasian	-0.571	-0.475	-0.694*	-0.559	-0.619
	(0.427)	(0.412)	(0.417)	(0.405)	(0.390)
Hispanic	-1.931**	-1.816**	-2.098**	-1.856**	-2.151**
	(0.943)	(0.902)	(1.037)	(0.937)	(1.013)
Married	-1.843	-1.572	-2.047	-1.998	-1.900
	(1.279)	(1.296)	(1.285)	(1.281)	(1.324)
Never Married	-0.149	-0.101	-0.548	-0.334	-0.439
	(0.674)	(0.644)	(0.763)	(0.672)	(0.744)
Homeless	-0.569	-0.329	-0.605	-0.665	-0.409
	(0.509)	(0.512)	(0.517)	(0.484)	(0.501)
Observations	132	129	132	132	129
R-squared	0.079	0.107	0.055	0.098	0.063

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.C: Baseline Results with Summons

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Difference in Summons (annual)				
EXP. VAR.:					
Total Contacts	-0.0691**				
	(0.0312)				
Case Mgmt Con.		-0.183**			
		(0.0870)			
Counseling Con.			-0.905		
			(0.616)		
Peer Contacts				-0.192**	
				(0.0825)	
Info Contacts					-0.320
					(0.212)
Age	-0.0458	-0.0258	-0.0579	-0.0484	-0.0479
	(0.0501)	(0.0443)	(0.0652)	(0.0504)	(0.0630)
Education	0.376*	0.183	0.288	0.386	0.355
	(0.209)	(0.158)	(0.223)	(0.244)	(0.219)
Male	-0.0989	-0.0202	-0.276	0.330	-0.521
	(0.365)	(0.395)	(0.405)	(0.306)	(0.481)
Caucasian	-0.217	-0.235	-0.222	-0.264	-0.513
	(0.454)	(0.481)	(0.393)	(0.387)	(0.479)
Hispanic	-1.742	-1.624	-1.807	-1.672	-2.318
	(1.509)	(1.424)	(1.885)	(1.498)	(1.868)
Married	-0.820	-0.316	-1.489	-1.196	-0.994
	(0.998)	(1.057)	(0.982)	(1.023)	(1.120)
Never Married	-0.0112	0.0263	-1.008	-0.528	-0.653
	(1.018)	(0.944)	(1.206)	(1.095)	(1.207)
Homeless	-0.340	-0.00232	-0.191	-0.588	-0.110
	(0.753)	(0.803)	(0.711)	(0.713)	(0.704)
Observations	132	129	132	132	129
R-squared	0.151	0.159	0.119	0.161	0.109

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5.A: Baseline Results with Emergency Medical Services Contacts

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Diff. in EMS Contacts (annual)				
EXP. VAR.:					
Total Contacts	0.0131				
	(0.00858)				
Case Mgmt Con.		0.0182			
		(0.0223)			
Counseling Con.			0.278**		
			(0.139)		
Peer Contacts				0.0208	
				(0.0170)	
Info Contacts					0.127**
					(0.0495)
Age	-0.0211	0.00314	-0.0212	-0.0182	-0.00158
	(0.0276)	(0.0163)	(0.0269)	(0.0272)	(0.0151)
Education	0.143	0.0605	0.179	0.135	0.0656
	(0.120)	(0.0741)	(0.127)	(0.119)	(0.0738)
Male	-0.159	-0.0648	-0.143	-0.179	0.0220
	(0.255)	(0.245)	(0.249)	(0.268)	(0.186)
Caucasian	-0.413	-0.0957	-0.483	-0.358	-0.141
	(0.412)	(0.324)	(0.430)	(0.403)	(0.253)
Hispanic	-0.917*	-0.721	-0.987*	-0.862*	-0.700
	(0.518)	(0.493)	(0.535)	(0.506)	(0.455)
Married	-0.468	-0.443	-0.302	-0.400	-0.420
	(0.314)	(0.281)	(0.313)	(0.301)	(0.258)
Never Married	-0.133	-0.0864	0.0599	0.00184	-0.119
	(0.472)	(0.442)	(0.444)	(0.442)	(0.417)
Homeless	0.372	0.545*	0.287	0.425	0.403
	(0.342)	(0.289)	(0.340)	(0.345)	(0.245)
Observations	132	129	132	132	129
R-squared	0.085	0.108	0.095	0.060	0.216

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5.B: Baseline Results with Hospitalizations

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR: Diff. in Hospitalization (annual)				
EXP. VAR.:					
Total Contacts	0.0143*				
	(0.00860)				
Case Mgmt Con.		0.0204			
		(0.0230)			
Counseling Con.			0.308**		
			(0.139)		
Peer Contacts				0.0243	
				(0.0180)	
Info Contacts					0.137***
					(0.0480)
Age	-0.0205	0.00315	-0.0207	-0.0176	-0.00171
	(0.0269)	(0.0159)	(0.0262)	(0.0265)	(0.0149)
Education	0.142	0.0635	0.182	0.134	0.0678
	(0.118)	(0.0725)	(0.125)	(0.118)	(0.0733)
Male	-0.198	-0.104	-0.181	-0.226	-0.00883
	(0.250)	(0.239)	(0.244)	(0.264)	(0.179)
Caucasian	-0.436	-0.122	-0.516	-0.382	-0.168
	(0.402)	(0.317)	(0.419)	(0.395)	(0.242)
Hispanic	-0.946*	-0.752	-1.026*	-0.894*	-0.725
	(0.519)	(0.496)	(0.535)	(0.511)	(0.457)
Married	-0.466	-0.446	-0.283	-0.391	-0.417
	(0.314)	(0.280)	(0.313)	(0.301)	(0.258)
Never Married	-0.131	-0.0800	0.0791	0.0118	-0.111
	(0.455)	(0.424)	(0.430)	(0.427)	(0.402)
Homeless	0.322	0.489*	0.227	0.380	0.339
	(0.334)	(0.282)	(0.330)	(0.339)	(0.236)
Observations	132	129	132	132	129
R-squared	0.093	0.110	0.108	0.066	0.236

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6.A: Alternative Results using LEAD Start Date as Instrument

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Difference in Incidents (annual)				
EXP. VAR.:					
Total Contacts	-0.0130				
	(0.0100)				
Case Mgmt Con.		-0.0326			
		(0.0312)			
Counseling Con.			-0.0904		
			(0.120)		
Peer Contacts				-0.0431*	
				(0.0241)	
Info Contacts					-0.0226
					(0.0566)
Age	-0.0486*	-0.0536**	-0.0526*	-0.0480*	-0.0600*
	(0.0272)	(0.0260)	(0.0313)	(0.0275)	(0.0315)
Education	0.286	0.282	0.285	0.285*	0.321*
	(0.173)	(0.170)	(0.183)	(0.170)	(0.181)
Male	-0.0891	-0.0542	-0.135	0.0193	-0.130
	(0.549)	(0.562)	(0.559)	(0.544)	(0.571)
Caucasian	-0.0261	-0.194	-0.0805	-0.0144	-0.270
	(0.663)	(0.696)	(0.662)	(0.654)	(0.687)
Hispanic	-0.0296	-0.138	-0.104	0.0143	-0.279
	(1.131)	(1.142)	(1.203)	(1.127)	(1.220)
Married	-0.0946	-0.0358	-0.191	-0.167	-0.172
	(1.386)	(1.379)	(1.394)	(1.410)	(1.391)
Never Married	-0.849	-0.863	-1.035	-0.930	-1.020
	(0.628)	(0.611)	(0.711)	(0.653)	(0.700)
Homeless	0.833	0.807	0.819	0.789	0.732
	(0.639)	(0.667)	(0.642)	(0.617)	(0.672)
Observations	132	129	132	132	129
R-squared	0.083	0.090	0.072	0.092	0.078

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6.B: Alternative Results using LEAD Start Date as Instrument

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.:Difference in Arrests (annual)				
EXP. VAR.:					
Total Contacts	-0.0331***				
	(0.0125)				
Case Mgmt Con.		-0.0933***			
		(0.0336)			
Counseling Con.			-0.386		
			(0.269)		
Peer Contacts				-0.0967***	
				(0.0290)	
Info Contacts					-0.165*
					(0.0920)
Age	-0.00648	0.00747	-0.0133	-0.00699	-0.00366
	(0.0236)	(0.0206)	(0.0285)	(0.0231)	(0.0270)
Education	0.169	0.0561	0.136	0.172	0.144
	(0.140)	(0.135)	(0.152)	(0.142)	(0.146)
Male	0.219	0.285	0.127	0.444	0.0293
	(0.451)	(0.459)	(0.464)	(0.449)	(0.484)
Caucasian	-0.208	-0.176	-0.242	-0.216	-0.317
	(0.435)	(0.446)	(0.439)	(0.420)	(0.435)
Hispanic	0.320	0.411	0.252	0.376	0.0577
	(0.986)	(0.969)	(1.105)	(0.991)	(1.067)
Married	-1.101	-0.830	-1.404	-1.282	-1.175
	(1.537)	(1.568)	(1.551)	(1.540)	(1.617)
Never Married	-0.0587	-0.0176	-0.535	-0.295	-0.362
	(0.541)	(0.526)	(0.645)	(0.546)	(0.617)
Homeless	0.747	0.948	0.793	0.631	0.896
	(0.557)	(0.580)	(0.572)	(0.536)	(0.591)
Observations	132	129	132	132	129
R-squared	0.118	0.129	0.106	0.125	0.105

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6.C: Alternative Results using LEAD Start Date as Instrument

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Diff. in Summons (annual)				
EXP. VAR.:					
Total Contacts	7.15e-05				
	(0.00885)				
Case Mgmt Con.		-0.00129			
		(0.0242)			
Counseling Con.			0.108		
			(0.110)		
Peer Contacts				0.000283	
				(0.0235)	
Info Contacts					0.0335
					(0.0359)
Age	-0.0201	-0.0251*	-0.0225	-0.0201	-0.0278*
	(0.0149)	(0.0130)	(0.0168)	(0.0151)	(0.0160)
Education	0.102	0.120**	0.122*	0.102	0.130*
	(0.0662)	(0.0540)	(0.0730)	(0.0670)	(0.0680)
Male	-0.0121	-0.0116	-0.0293	-0.0128	-0.00114
	(0.127)	(0.127)	(0.137)	(0.132)	(0.143)
Caucasian	0.0236	-0.0505	-0.0479	0.0234	-0.0797
	(0.155)	(0.150)	(0.157)	(0.151)	(0.146)
Hispanic	-0.403	-0.449	-0.486	-0.404	-0.471
	(0.480)	(0.464)	(0.524)	(0.483)	(0.523)
Married	-0.230	-0.238	-0.190	-0.230	-0.259
	(0.388)	(0.387)	(0.385)	(0.389)	(0.389)
Never Married	-0.591**	-0.581**	-0.587*	-0.591*	-0.624*
	(0.298)	(0.276)	(0.319)	(0.307)	(0.320)
Homeless	0.440*	0.398*	0.383*	0.440**	0.340*
	(0.223)	(0.235)	(0.208)	(0.215)	(0.205)
Observations	132	129	132	132	129
R-squared	0.131	0.148	0.150	0.131	0.162

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7.A: Alternative Results using LEAD Start Date as Instrument

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Diff. in EMS Contacts (annual)				
EXP. VAR.:					
Total Contacts	-0.00436				
	(0.0101)				
Case Mgmt Con.		-0.00390			
		(0.0314)			
Counseling Con.			-0.0484		
			(0.163)		
Peer Contacts				-0.0306*	
				(0.0177)	
Info Contacts					0.0672
					(0.0745)
Age	0.0557**	0.0515*	0.0548**	0.0584**	0.0457*
	(0.0257)	(0.0282)	(0.0256)	(0.0252)	(0.0266)
Education	0.111	0.124	0.107	0.105	0.145
	(0.119)	(0.121)	(0.119)	(0.119)	(0.119)
Male	0.0514	0.0297	0.0388	0.151	0.0479
	(0.301)	(0.306)	(0.293)	(0.299)	(0.262)
Caucasian	0.228	0.164	0.222	0.279	0.102
	(0.428)	(0.445)	(0.428)	(0.401)	(0.396)
Hispanic	0.205	0.155	0.194	0.290	0.104
	(0.675)	(0.681)	(0.718)	(0.654)	(0.673)
Married	-0.0958	-0.105	-0.135	-0.124	-0.154
	(0.373)	(0.399)	(0.371)	(0.362)	(0.376)
Never Married	-0.0307	-0.0653	-0.0934	-0.0200	-0.158
	(0.731)	(0.752)	(0.687)	(0.685)	(0.702)
Homeless	0.405	0.373	0.410	0.397	0.252
	(0.421)	(0.442)	(0.400)	(0.412)	(0.388)
Observations	132	129	132	132	129
R-squared	0.132	0.114	0.130	0.154	0.129

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7.B: Alternative Results using LEAD Start Date as Instrument

	(1)	(2)	(3)	(4)	(5)
	DEPENDENT VAR.: Diff. in Hospitalizations (annual)				
EXP. VAR.:					
Total Contacts	-0.00466				
	(0.00927)				
Case Mgmt Con.		-0.00518			
		(0.0294)			
Counseling Con.			-0.0344		
			(0.147)		
Peer Contacts				-0.0303*	
				(0.0159)	
Info Contacts					0.0708
					(0.0702)
Age	0.0541**	0.0492*	0.0527**	0.0566**	0.0428*
	(0.0249)	(0.0271)	(0.0248)	(0.0244)	(0.0256)
Education	0.119	0.134	0.118	0.114	0.158
	(0.115)	(0.117)	(0.115)	(0.116)	(0.115)
Male	-0.0727	-0.0996	-0.0890	0.0245	-0.0826
	(0.297)	(0.302)	(0.287)	(0.294)	(0.253)
Caucasian	0.228	0.162	0.210	0.276	0.0932
	(0.409)	(0.424)	(0.405)	(0.379)	(0.367)
Hispanic	0.149	0.103	0.124	0.230	0.0436
	(0.667)	(0.673)	(0.710)	(0.643)	(0.668)
Married	-0.0574	-0.0635	-0.0927	-0.0866	-0.120
	(0.361)	(0.382)	(0.362)	(0.353)	(0.366)
Never Married	-0.0323	-0.0609	-0.0987	-0.0265	-0.164
	(0.707)	(0.726)	(0.670)	(0.666)	(0.684)
Homeless	0.358	0.324	0.354	0.348	0.193
	(0.408)	(0.428)	(0.384)	(0.399)	(0.373)
Observations	132	129	132	132	129
R-squared	0.131	0.110	0.127	0.153	0.129

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Before and After Means and Confidence Interval Comparisons