Reading for Life and Adolescent Re-Arrest: Evaluating a Unique Juvenile Diversion Program

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Abstract

We present results of an evaluation of Reading for Life (RFL), a diversion program for nonviolent juvenile offenders in a medium-sized Midwestern county. The unique program uses philosophical virtue theory, works of literature, and small mentoring groups to foster moral development in juvenile offenders. Participants were randomly assigned to RFL treatment or a comparison program of community service. The RFL program generated large and statistically significant drops in future arrests. The program was particularly successful at reducing the recidivism of more serious offenses and for those groups with the highest propensity for future offenses. © 2016 by the Association for Public Policy Analysis and Management.

INTRODUCTION

Although juvenile crime rates have fallen considerably over the past decade and a half (Butts, 2013), juvenile delinquency continues to be a pressing societal problem. In 2012, over one million juvenile arrests occurred throughout the country, with an overrepresentation of male and minority youth (Federal Bureau of Investigation, 2012). Moreover, at approximately 250 youths per 100,000 citizens, the United States leads all industrialized nations in juvenile incarcerations (Annie E. Casey Foundation, 2013). Nationwide, more than 25 percent of those arrested for property crimes and nearly 20 percent of those arrested for violent crimes are under the age of 18 (U.S. Department of Health and Human Services, 2008). Using a "willingness-to-pay" framework, Cohen, Piquero, and Jennings (2010) calculate that serious juvenile offenders cost society upward of $500,000 each during their adolescent years.

Contact with the justice system in adolescence carries lifelong consequences. Juvenile convictions have been shown to decrease job stability, lessen the likelihood of employment, and stunt pay growth (Grogger, 1995; Kling, 2006; Lott, 1990; Nagin & Waldfogel, 1993, 1995). Released felons have difficulty establishing solid career paths, and often find themselves mired in a series of temporary jobs without benefits (Nagin & Waldfogel, 1993). In a recent paper that uses variation in incarceration rates for juveniles generated by the random selection of judges, Aizer and Doyle (2015) found that incarceration reduced high school graduation rates and increased the chance of adult recidivism.

Juvenile delinquency is also a strong predictor of criminal activity as an adult (McCord & Esminger, 1997; Nagin & Paternoster, 2000), although not all youths embroiled in the justice system become adult offenders (Laub & Sampson, 1993;

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Sampson & Laub, 2003). Interventions and positive life events during adolescence may reduce the probability that a juvenile offender will become an adult criminal. Social relationships can create opportunities for turning points, or life transitions, which can either reinforce or counteract criminal behavior (Sampson & Laub, 2003). Recent longitudinal analysis demonstrates that the majority of juvenile offenders do not evolve into lifelong criminals, suggesting that positive turning points usually outweigh negative ones over time (Sampson & Laub, 2003).

One group of policy levers that may act as turning points are juvenile diversion programs that provide youth a way to bypass adjudication or punishment within the criminal justice system. Diversion programs are designed for a variety of purposes including reducing future involvement with the court system, lowering stigma associated with having a criminal record, increasing system efficiency, and lowering court costs (Cocozza et al., 2005; Cuellar, McReynolds, & Wasserman, 2006; Pogrebin, Poole, & Regoli, 1984). Historically, programs have consisted of a justice component (e.g., police decision, probation supervision, court process) and a service component (Cocozza et al., 2005); however, beyond these basic tenants, programs differ substantially from one another and few national standards have been established. Despite the diversity of interventions, there is relative uniformity on the criterion used for determining program success: the rate of recidivism. This is not surprising given that the outcome has implications for public safety, societal costs, and individual educational and employment outcomes. In addition, recidivism data can be easily obtained via administrative sources at a relatively low cost (Regoli, Wilderman, & Pogrebin, 1983). Unfortunately, evaluative similarities of juvenile diversion programs end in the definition of the key outcome variable. Results from the research are nearly as diverse as program characteristics themselves (McCord, Widom, & Crowell, 2001);[1] however, as we note in the next section, some program specifics have proven to be more successful than others, such as the use of mentors and those that include a rehabilitative or therapeutic orientation.

This paper evaluates the impact of a juvenile diversion program, Reading for Life (RFL), implemented in a medium-sized Midwestern town. A unique and innovative alternative to prosecution in the court system, RFL allows nonviolent, often first-time juvenile offenders to study works of literature in small groups led by trained volunteer mentors. Informed by philosophical virtue theory (MacIntyre, 1984), the program was designed to foster character development in at-risk adolescents through personal mentoring relationships and moral discussions. RFL strives to be a catalyst for transformative and enduring virtuous life changes by engaging, educating, and empowering its participants.

Given the overall lack of concrete evidence about the success of youth diversion programs, an evaluation of the RFL model is well situated within this broader literature. First, the intervention is a randomized control trial (RCT), providing the greatest possibility for internal validity. Second, the intervention attempts to reduce recidivism through character education and moral development, a new and untested method via mentoring, which has shown some promise in this area. Third, our key outcome is recidivism; therefore, results from this work are easily comparable to existing literature. Fourth, our samples are relatively large compared with other research. In their meta-analysis of 57 studies on this topic, Schwalbe and colleagues (Schwalbe et al., 2012) list 14 RCTs and only four have sample sizes larger than we use here.

[1] See Supporting Information Appendix B for a more comprehensive review of the literature on diversion programs. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Results presented below provide encouraging evidence that assignment to RFL generates large reductions in the likelihood of re-arrest. Those assigned to RFL treatment experienced a statistically significant 13.2 percentage point reduction in the probability of having another offense of any type within two years of original assignment, which is a 36 percent reduction over the control group mean. The program was particularly successful at reducing more serious offenses; the probability of being arrested for a prosecuted felony committed within two years of assignment fell by 51 percent over the control group mean (p-value = 0.015).

In the next section, we outline in detail the RFL program, the study protocols, and data collection. In the third section, we outline how key variables are measured and the basic statistical model. In the fourth section we present basic results and outline the heterogeneity in results across some various demographic groups. In the final section, we make some cost-effectiveness calculations, as well as provide some concluding remarks and suggestions for future research.

THE *READING FOR LIFE* DIVERSION PROGRAM

Participants

The project evaluates the impact of RFL, a juvenile diversion program run in a midsize, Midwestern county. Since 2010, eligible offenders have been referred by their probation officers to the diversion program, where they are randomly assigned to participate in either the RFL program (the treatment group) or to 25 hours of community service (the control group). Community service is a common method of diversion throughout the country. Youth are often handed a list of potential service sites and asked to report back when their hours are complete. Little or no direction is provided by the probation staff, and youth and their parents are responsible for ensuring the completion of service hours. The three hours that RFL intake staff spend with research participants represents three times the amount of time that most probation staff spend with youth who participate in community service diversion programs. In general, it takes about 16 weeks to complete both RFL and the community service component of the control treatment.

For the current study, participants were nonviolent offenders aged 11 to 18 who entered the juvenile justice system between June 2010 and December 31, 2013.[2] The first phase of pilot research suggested that RFL might be most effective at reducing recidivism with first-time offenders who had not been either labeled or jaded by extensive involvement in the system (Seroczynski et al., 2011). The selection of nonviolent offenders was beyond experimental control; violent offenders are not eligible for diversion programs and constitute the majority of youth who are adjudicated at other points in the system.

In Figure 1, we use a flow diagram to provide an indication of how arrestees in this age group made it into the RFL experiment over this time period. The numbers in parentheses represent the number of cases at each node in the decision tree. Over the period in question, a total of 9,368 youths were arrested in St. Joseph's county. A little more than half were dismissed or received a warning: for the remaining cases there was sufficient evidence to assign a probation officer to the case. Of these, 53.6 percent were eventually dismissed, 31.8 percent were adjudicated through traditional channels and 14.6 percent, or 672 cases, were recommended for diversion. In

[2] Juveniles arrested at 17 years of age might turn 18 before referral or completion of the diversion program.

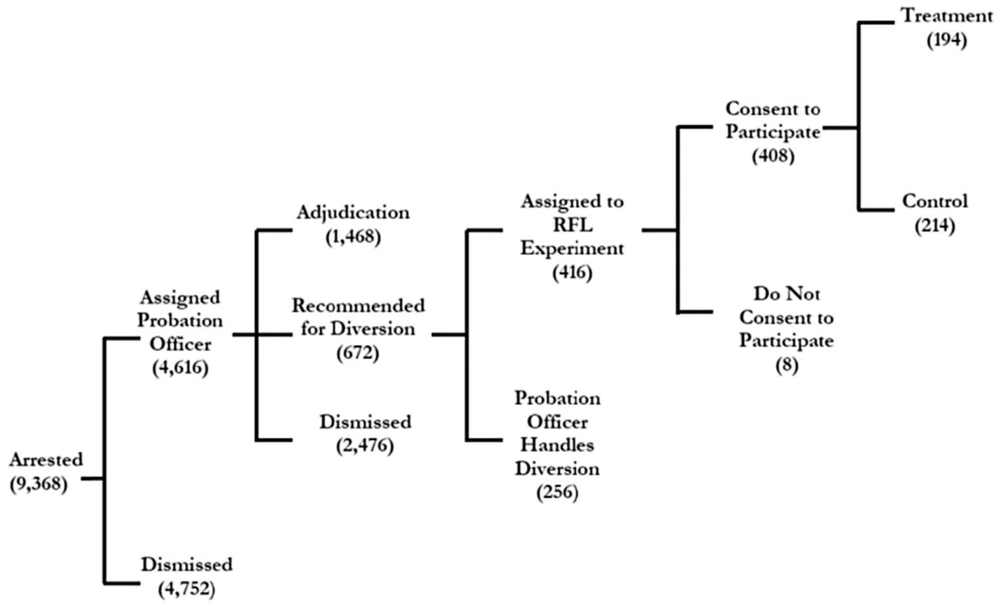

Figure 1. Flow Chart of Arrested Youth into Experiment.

this group, 256 cases were referred to probation officers who handled diversion,[3] while 416 were assigned to the RFL experiment. Eight arrestees did not consent to study participation, leaving 408 in the experiment. A total of 194 offenders were randomly assigned to the RFL treatment and 214 were assigned to the control group.[4] Randomization occurred immediately after referral to the program from the probation officer. Individuals were assigned a random number between 0 and 1 drawn from a uniform distribution and assignment to treatment was based on this number. In 2010, because volunteer mentor resources were scarcer, the probability of a candidate being assigned to treatment was 33 percent. In all other years, the probability of an arrestee being assigned to treatment was 50 percent.

In Table 1, we report the ages of those enrolled in the treatment and control groups by year. Note the low fraction in treatment in 2010, but in all other years the fraction in treatment is roughly 50 percent. There is also rough equivalence in the age distribution across the two groups. The peak age for enrollees in the experiment is 15 to 16, with 178 participants in this category. There are only 29 adolescents who entered the experiment aged 11 to 12.

The RFL program has a detailed intake assessment protocol; only the measures used in this analysis are discussed here. A demographic form is completed by a guardian of the juvenile offender upon referral to diversion services, which includes basic demographics and identifying information such as address and birth date,

[3] Probation officers decide to handle the diversion if there are some previous offenses or a more serious offense that would make the youth ineligible for the RFL program but maybe a good candidate for diversion. She or he might be diverted to officer care if there is some expectation that the family may need more services (e.g., counseling) than just diversion for the youth.

[4] Although only a small fraction of all arrested youth are eligible for the program, the experiment was assigned 62 percent of all eligible cases. Moreover, as we demonstrate below, this is a particularly interesting group because they have a high rate of recidivism. Among those in the control group, 25 percent are arrested for a prosecuted felony after two years.

Table 1. Age of participants by year and by program.

	Age on entry date				
	11–12	13–14	15–16	17–18	Total
Reading for Life treatment					
2010	1	7	6	7	21
2011	6	14	20	8	48
2012	4	14	29	18	65
2013	3	10	29	18	60
Total	14	45	84	51	194
Community service control					
2010	8	12	24	8	52
2011	3	13	20	11	47
2012	4	9	22	17	52
2013	0	13	28	22	63
Total	15	47	94	58	214
Total	29	92	178	109	408

family income, youth living situation, and parents' education. The RFL program also works with the Juvenile Justice Center to document arrest and prosecution rates of all participants.

Sample demographics are reported in Table 2. In the first column and for the purposes of comparison, we report characteristics of adolescents aged 11 to 18 from the county of the intervention. This data was collected from the 2008 to 2012 American Community Survey.[5] In column 2, we report means for the 408 people in the study. In column 3, we report the OLS-corrected difference in means between the treatment and control groups. This difference is generated by a regression of the descriptive variable on a treatment dummy and a set of dummy variables for the year the person was assigned to treatment. The year dummies are meant to control for the fact that the chance someone was assigned to treatment was lower in the first year of the experiment. The final column of the table contains the p-value for the test of the null hypothesis that the treatment dummy equals zero from this regression. In no case can we reject the null at a p-value of 0.15, and when we test the joint hypothesis that all 21 variables are zero we get a p-value on the F-test of 0.98. Almost 90 percent of youths in both the treatment and control samples completed their respective diversion programs. The similarity in completion rates in the treatment and control groups is not surprising since the time commitment is the same in both programs.

According to the American Community Survey, among county residents aged 11 to 18, roughly 10 percent are Hispanic, 17 percent are Black, and 66 percent are White; so Black respondents are overrepresented in our experiment while whites are underrepresented. The average age of those diverted is 15.3 years, which is slightly older than the average age of 11- to 18-year-olds in the county. As the program only takes nonviolent offenders, a majority of program participants are female.[6] A little more than one-quarter of program participants are living with both biological parents, which is well below the average for children in the county (56.7 percent).

[5] This data was downloaded from usa.ipums.org (Ruggles et al., 2010).
[6] Nationwide in 2011, among youths arrested, 82 percent of violent offenses were perpetrated by males and only 18 percent by females (U.S. Department of Justice, Office of Juvenile Justice and Delinquency Prevention, 2013).

Table 2. Sample characteristics for treatment and control groups.

Variable	2008-2012 ACS, 11- to 18-year-olds in county of intervention (29,895 obs.)	Mean outcome in total sample	Coefficient (standard error) on treatment dummy	p-Value on test that means are the same across samples
Completed program		0.898	−0.004 (0.041)	0.979
Race/ethnicity dummy variables				
White, non-Hispanic	0.664	0.451	0.037 (0.052)	0.365
Black, non-Hispanic	0.173	0.311	−0.025 (0.048)	0.673
Asian, non-Hispanic	0.012	0.016	−0.011 (0.012)	0.484
Hispanic	0.097	0.120	0.009 (0.032)	0.957
Multiracial/ethnic	0.080	0.091	−0.001 (0.029)	0.855
Other or unknown	0.017	0.089	−0.006 (0.008)	0.365
Age upon entry	14.7	15.3	−0.092 (0.200)	0.916
Male dummy variable	0.518	0.428	−0.049 (0.060)	0.363
Household type dummy variables				
Both biological parents	0.567	0.261	−0.016 (0.047)	0.588
Single parent	0.295	0.419	−0.023 (0.050)	0.669
One biological parent and partner		0.229	0.021 (0.041)	0.583
Other relatives	0.035	0.078	0.033 (0.025)	0.168
Adopted or foster parents	0.014	0.027	−0.002 (0.015)	0.845
Family Income				
Median if reported	$44,989	$38,995	−1,423 (3551)	0.648
Income not reported		0.200	−0.038 (0.041)	0.373
Mother's education dummy variables				
Less than high school	0.097	0.129	−0.009 (0.033)	0.609
High school diploma or GED	0.300	0.224	−0.025 (0.043)	0.458
Some college education	0.238	0.162	0.008 (0.039)	0.572
College degree or higher	0.365	0.156	0.048 (0.038)	0.268
Mother's education not reported		0.329	−0.022 (0.046)	0.802

Notes: There are 194 observations in the treatment group and 214 in the control group. The census data was not detailed enough to accurately provide this information. We only report if the child lives in a married, two-parent household versus a single parent or nonmarried household.

Parents were asked to provide annual family income and education levels for both the mother and father. Unfortunately, these two variables are missing in our sample 20 and 33 percent of the time, respectively. When reported, average family income for those in our sample is about 12.6 percent lower than the amount for the average family in the county with children aged 11 to 18 ($39,925 vs. $44,989). Likewise, maternal education in the control population appears well below the average education for mothers in the county with children 11 to 18. Income and education are more likely to be missing in more at-risk families.[7] In our regression

[7] Pooling the treatment and control samples, the average chance a participant came from a family with both biological parents is 30.1 percent if income is reported, but 14.1 percent if it is not. Likewise, among all participants, the fraction who lived with both natural parents is 33.7 for those who report maternal education, but only 13.4 percent for those who do not.

models, we set income and all education dummies to zero when these variables are not reported and include a dummy variable for whether the variable is not reported.[8]

Diversion Program

Treatment group members are given the 3-Minute Reading Assessment (Rasinski & Padak, 2005) to determine group placement. Groups consist of no more than five participants of comparable reading ability and two trained mentors; groups meet twice weekly for 10 weeks. RFL mentors are volunteers who have undergone extensive practical and theoretical training, including 12 weeks spent shadowing an experienced mentor. All mentors attend quarterly meetings for ongoing training and supervision. Mentors do not have access to or knowledge of their students' criminal records and delinquent past.

At the beginning of the program, each small group selects a novel to read from several options. Over the following weeks, the 60-minute sessions consist of oral readings, journaling questions developed by the mentors, and facilitated discussions on virtuous character implications found in the readings and writing exercises. Participants learn about seven classic virtues from Aristotle and Thomas Aquinas' virtue theory: justice, prudence, temperance, fortitude, fidelity, hope, and charity. There has been a recent revival in the use of stories to foster moral development (Bettelheim, 1976; Bruner, 2003, 2008; Coles, 1989; McGavock, 2007; Vitz, 1990), specifically that of a virtuous nature (Cain, 2005; Carr, 1991; Hoff-Sommers, 1993; MacIntyre, 1984; Nussbaum, 1990). Literature is uniquely suited to facilitate moral development because of the vicarious experiences and contextual relationships provided within (Cunningham, 2001; Vitz, 1990). Bruner (2003) notes that story may be particularly effective at fostering moral development because "the plights and the intentional states depicted in 'successful' fiction sensitize us to experience our own lives in ways to match" (p. 52). The journaling exercises in RFL groups frequently focus on personal life reflections that spring from the content of both the novel and group discussions.

All RFL groups are given the opportunity to practically apply these lessons, choosing a one-day community service project thematically consistent with the group readings and discussions. This component promotes reconciliation and engagement in the local community.[9] The RFL program culminates with a final presentation by the participants for their parents or guardians, group mentors, and RFL administrative staff. Participants in the treatment group spend 25 hours in formal program activities (not including individual reading time), an amount roughly equal to the time spent in community service in the control condition.

After successful completion of either diversion program, participants are not required to report that they were charged or convicted of a crime on any employment or academic application. In addition, when they become a legal adult and are offense-free for a minimum of three years, they may petition the State of Indiana to have their juvenile record expunged.

RFL is distinctive along a number of dimensions, including instruction in classic virtue theory, the inclusion of literature to facilitate moral development, and

[8] For maternal education, we generated five dummy variables: whether the mother has less than a high school degree, a high school diploma or a GED, some college, a college degree or higher, or maternal education not reported. For income, we used quartile groups for those who report income and included a dummy variable for income not reported.

[9] For example, groups that have read books with sick children as main characters have cooked meals and served families at the local Ronald McDonald house. Groups that have read books with an environmental theme have volunteered to clean up a local riverbank. Books about the Holocaust have led groups to the area's Jewish Federation for service projects.

the engagement of volunteer mentors. There is data suggesting that some of these elements have been used to reduce recidivism in other situations.

Although evidence about the effectiveness of juvenile diversion programs is murky at best, some research suggests that programs with a therapeutic or rehabilitative orientation such as RFL are more likely to be effective in mitigating recidivism. Cuellar, McReynolds, and Wasserman (2006) found that when appropriate, youth who were diverted to mental health treatment had significantly fewer arrests than a matched, wait-listed comparison group. A large meta-analysis by Landenberger and Lipsey (2005) found that programs that attempted to engender personal development by nurturing skills, relationships, and insight were more effective than programs seeking to deter violence or detect bad behavior. In particular, programs rooted in cognitive behavioral therapy have shown promising effects on recidivism (Heller et al., 2013; Landenberger & Lipsey, 2005; Lipsey et al., 2010; Lipsey, Landenberger, & Wilson, 2007; Pearson et al., 2002; Wilson, Bouffard, & Mackenzie, 2005), although models such as RFL based on other theoretical orientations have rarely been tested with a sound experimental design.

Finally, RFL employs volunteer mentors as small group leaders, and there is a sizeable body of literature supporting the use of mentoring to curb adolescent delinquent behavior.[10] In a meta-analysis of 46 programs, mentoring among high-risk populations—even when combined with other approaches—appeared to have positive effects on delinquency, aggressive behavior, drug use, and academic achievement (Tolan et al., 2014).[11] This is consistent with the prevailing view that mentoring programs are most beneficial for at-risk participants (DuBois et al., 2002; Hamilton & Hamilton, 1992). Programs that emphasize emotional development and include ongoing training for mentors, structured activities, expectations for frequent contact, and overall monitoring of program implementation seem particularly promising (Dubois et al., 2002; Tolan et al., 2014). Consistent with Sampson and Laub's (2003) life-course theory of criminal behavior, this suggests that mentoring may act as a turning point for youth who face a range of economic, family, educational, or interpersonal issues.

METHODS

As noted above, the primary outcome in most studies of juvenile diversion programs is whether adolescents recidivate. We used variants of this measure, as well as arrest counts over time, as our outcomes of interest. All data are obtained from two sources. First, data on juvenile arrests is obtained from the county Juvenile Justice Center (JJC). The JJC data comes from a relational database containing information about juveniles' demographics as well as their interactions with the criminal justice system. One portal within this database records the dates, descriptions, and outcomes of each arrest. To form our research dataset, we searched the JJC records for every person in the RFL study. Given the way the JJC data is structured, we had to download the data one record (e.g., person) at a time. We called records by name and birthdate and because all people in the RFL experiment had an arrest already, we can verify exactly that the record belonged to a particular RFL participant.

[10] Mentoring is defined here as a relationship in which two individuals interact over an extended period of time, the mentor passes along experience or knowledge to a mentee in position to benefit from it, and the mentor is a volunteer uninvolved with the youth in a professional capacity.

[11] The studies in this review included 27 with random assignment and 19 quasi-experimental designs. The random assignment studies include some famous mentoring programs such as the Big Brothers/Big Sisters programs (Grossman & Tierney, 1998; Herrera et al., 2007) and the Buddy system (O'Donnell, Lydgate, & Fo, 1979).

Therefore, there is little chance that we are understating arrests because of an inability to match individuals across datasets. We pulled arrest data in early May of 2014.[12]

The data at the JJC only includes arrests before age 18. Some of the participants age into the adult system a few years after completing their respective programs, so this data will not accurately measure re-arrests for this older group. In Indiana, all adult court records are public; therefore, we downloaded all offenses for study participants who turned 18 sometime during our follow-up.[13] This data will have any arrest that leads to a court appearance, so that prosecuted charges are defined similarly for juvenile and adult cases. However, in the juvenile data, arrests that are dismissed before a court appearance will appear in our data whereas no such arrest would appear in the adult data.[14] This data is in a searchable dataset that can only be accessed record by record. As this dataset does not contain juvenile data, there is a chance of a "false negative"—that we could not find a record for a person when in fact one existed. We searched this dataset based on the person's name and then the participant's birthdate to ensure that we were not missing any arrests.

The arrest records identify the class of the offense (including whether the incident was a misdemeanor, a felony, or "status offense" such as truancy or running away from home), and whether the arrest was prosecuted. Using this we construct six different indicators of recidivism. To construct the first three, we measure whether participants were arrested for any re-offense, then whether they were arrested for a misdemeanor or felony. These offenses may be prosecuted or not. To construct the final three indicators, we isolated the prosecuted offenses from the first three indicators. In Figure 1, determining whether to prosecute an offense is the second node in the decision tree. A prosecuted offense requires sufficient evidence to take the case before a magistrate and the crime must be at a level that precludes diversion by a probation officer. We should emphasize that whether a participant has a prosecuted offense is not a proper subset of all first offenses. If their first offense is not prosecuted and they have a second offense that is, the dummy for "Did you have any offense?" and "Did you have a prosecuted offense?" will both be one.

To complete our analysis, we examine program impact in three different samples. The first sample includes 408 people who were assigned a treatment status as of the end of 2013.

A limitation of this sample is that it includes participants followed for varying periods of time. For instance, those enrolled on December 1, 2013, have four months of follow-up, whereas those enrolled on June 1, 2010, have 46 months of follow-up. In practice, we would like to follow a group of participants over a fixed window of time and measure recidivism rates over that timeframe. Doing so necessarily reduces sample sizes. For example, if we were to limit our sample to participants with at least one-year recidivism rates, anyone who entered the program after April 31, 2013, would be dropped from the sample. Therefore, the second sample includes 357 participants who were followed for one full year after they were assigned to the treatment or control condition to examine one-year recidivism rates. The third is defined similarly for the 263 participants tracked for two full years after program assignment. The increased size of the first sample must therefore be weighed against

[12] Given that the data is administrative, we do not have the problem of sample attrition that may be present in some experiments. Once a person has completed the treatment or control program, every arrest is recorded in the administrative dataset. We will only "lose" data on participants if they commit a crime outside of the county of residence.

[13] See www.doxpop.com.

[14] The public adult data from doxpop.com also includes traffic violations that we did not include in our analysis.

the fact that, by examining recidivism rates of varying timeframes, we are not giving all participants an equal chance to re-offend.

For each sample and outcome, we initially report two estimated impacts. The first is a simple regression-adjusted difference in means that controls for the fact that the experiment had different probabilities of assignment to treatment in the first year. If y_i is the outcome of interest for person i and d_i is a dummy variable that equals 1 if the person was assigned to treatment, then the parameter of interest is obtained from a regression of the form

$$y_i = \alpha + d_i\delta + \sum_{t=2010}^{2012} \lambda_i(t)\pi_t + \varepsilon_i \tag{1}$$

where $\lambda_i(t)$ is a dummy variable that equals 1 if person i was enrolled in the experiment in year t, and ε_i is a random error. The estimates in Table 2 indicate that controlling for time affects the covariates that are uncorrelated with the intervention dummy d_i so adjusting for covariates should not alter the estimate for $\hat{\delta}$ much. However, covariates could reduce residual variance and possibly increase precision, so we consider a second model where we estimate the multivariate regression

$$y_i = \alpha + d_i\delta + \sum_{t=2010}^{2012} \lambda_i(t)\pi_t + x_i\beta + \varepsilon_i \tag{2}$$

where x_i is a vector of observed characteristics of program participants taken at the time of assignment. In our models, we add a dummy for sex, plus a complete set of dummies for a person's age, the year they entered treatment, race/ethnicity, family structure, mother's education, and family income. As youth in RFL are assigned to distinct reading groups, outcomes may be correlated for group members, decreasing the effective size of the treatment group. To deal with this possibility, we calculate standard errors allowing for arbitrary correlation in errors for members of each unique reading group. In this case, we treat all members of the control group as a unique group.

The measure of recidivism in the previous section only assesses the extensive margin of criminal activity. An alternative outcome would include some measure of the intensive margin as well. One such outcome is simply the counts of arrests for program participants. In Figure 2, we report counts of arrests within the first year for those in the treatment and comparison samples. In Figure 2a, we report these counts for all offenses (prosecuted and nonprosecuted felonies, misdemeanors, and status offenses); in Figure 2b we report the same numbers for all felony arrests. For all offenses, we see that the treatment group has a much higher fraction of no re-arrests and smaller counts of one, three, and four-plus arrests. These differences are much starker in Figure 2b. Comparing treatment to control sample, we also see much larger zero counts and dramatically smaller counts of one (2.9 vs. 12.3 percent), two (0.6 vs. 2.1 percent), three (0.6 vs. 1.1 percent), and four arrests (0.0 vs. 0.5 percent).

The low counts and high fraction of zero re-arrests in Figure 2b mean that OLS models may not provide an accurate way to estimate the impact of RFL for this outcome. Instead, we use a negative binomial model count data model with parameter values estimated via maximum likelihood. This model is a generalization of the Poisson that allows for overdispersion. We estimate the models using the same covariates as the models above. In this model, the coefficient on the treatment dummy variable (δ) is approximately the percentage change in expected re-arrests between the treatment and control group. The approximation to percentage changes is only accurate for small values of δ and the results in Table 3 suggest these estimates will

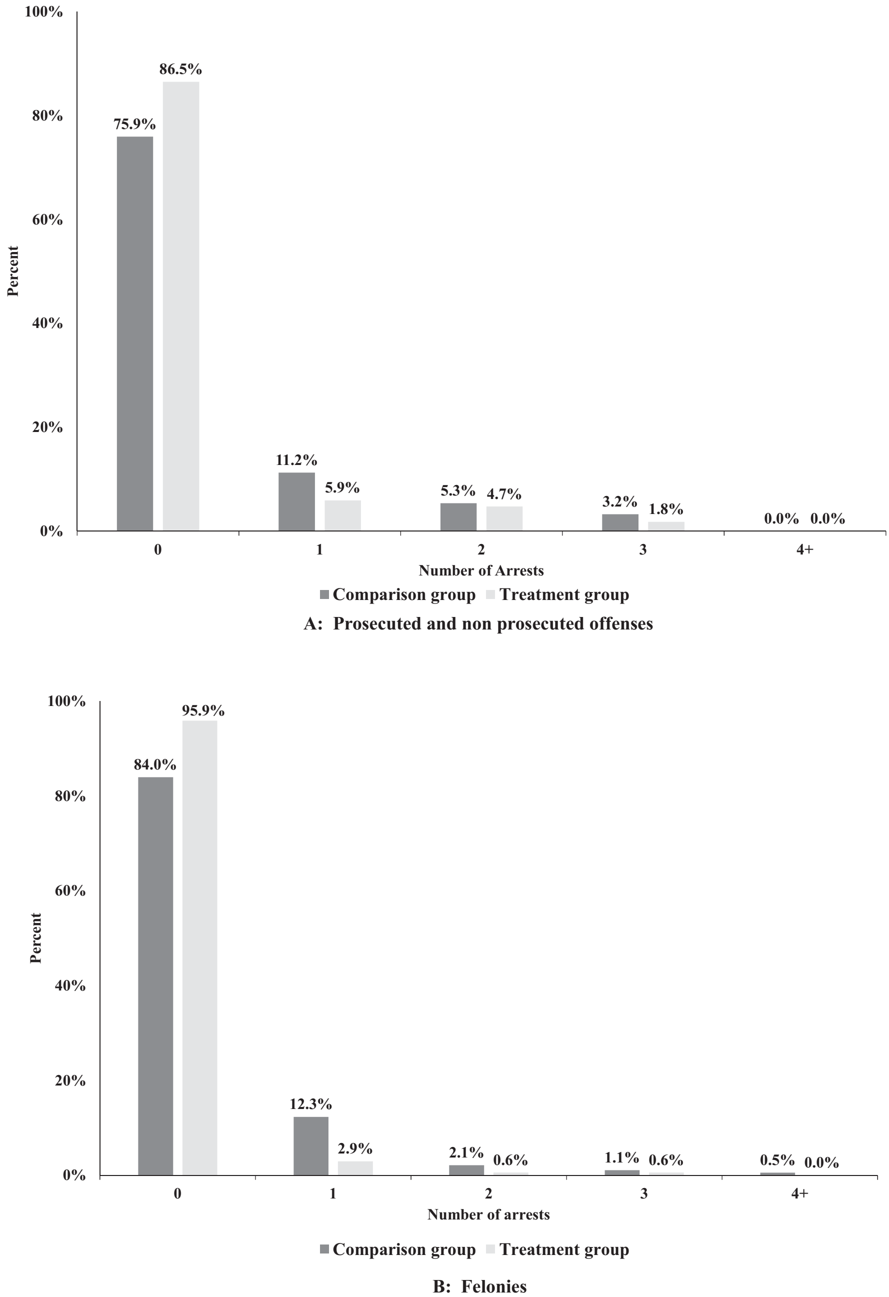

Figure 2. Histogram of Arrest Counts within the First Year.

be large, so for all models we will report the percentage change in expected counts as the more accurate value $e^{\delta} - 1$. In this case, we calculate the standard error on this percent using the "delta" method. Standard errors for the maximum likelihood estimates are calculated using a similar clustering procedure for the OLS regression outlined in the previous section.

Table 3. Estimates of the impacts of treatment on recidivism: Estimated impact (Standard error) {p-value on null that impact is zero}.

Seriousness of offense	Offense any time after assignment ($n = 408$): Mean of outcome in control group	Coefficient on treatment dummy: Model (1)	Model (2)	Offense in first year ($n = 357$): Mean of outcome in control group	Coefficient on treatment dummy: Model (1)	Model (2)	Offense in first two years ($n = 263$): Mean of outcome in control group	Coefficient on treatment dummy: Model (1)	Model (2)
Prosecuted and nonprosecuted offenses									
All offenses	0.379	−0.087	−0.081	0.241	−0.102	−0.106	0.367	−0.132	−0.133
		(0.047)	(0.044)		(0.041)	(0.039)		(0.059)	(0.058)
		{0.066}	{0.065}		{0.014}	{0.007}		{0.027}	{0.024}
Misdemeanors	0.248	−0.054	−0.049	0.123	−0.045	−0.051	0.230	−0.105	−0.109
		(0.041)	(0.040)		(0.033)	(0.031)		(0.050)	(0.049)
		{0.189}	{0.224}		{0.175}	{0.102}		{0.035}	{0.028}
Felonies	0.220	−0.081	−0.072	0.160	−0.104	−0.097	0.237	−0.082	−0.073
		(0.035)	(0.032)		(0.031)	(0.030)		(0.046)	(0.044)
		{0.023}	{0.027}		{0.001}	{0.002}		{0.076}	{0.096}
Prosecuted offenses									
All offenses	0.308	−0.096	−0.087	0.193	−0.111	−0.111	0.295	−0.185	−0.177
		(0.043)	(0.039)		(0.035)	(0.033)		(0.050)	(0.049)
		{0.025}	{0.026}		{0.002}	{0.001}		{0.000}	{0.000}
Misdemeanors	0.192	−0.057	−0.046	0.080	−0.056	−0.054	0.158	−0.154	−0.141
		(0.038)	(0.037)		(0.026)	(0.026)		(0.037)	(0.039)
		{0.131}	{0.216}		{0.032}	{0.038}		{0.000}	{0.000}
Felonies	0.187	−0.085	−0.080	0.134	−0.101	−0.092	0.194	−0.098	−0.095
		(0.032)	(0.029)		(0.027)	(0.027)		(0.040)	(0.038)
		{0.008}	{0.006}		{0.000}	{0.001}		{0.015}	{0.013}

Notes: Standard errors are clustered on the reading group for those in the treatment sample. Heteroskedastic-consistent standard errors are calculated for those in the control group. Other covariates in the regressions are a complete set of dummies for sex, age, race/ethnicity, year, family structure, income quartile, and mother's education.

RESULTS

Recidivism

Basic estimates for the six outcomes, three sample and two regression models are reported in Table 3. In the top half of the table we report arrest estimates for any offense, and in the bottom half of the table we generate estimates for the first prosecuted offense. Within each of these categories, we report separate estimates for all offenses, then misdemeanors and felonies separately. Reading from left to right in the table, we initially present estimates that consider recidivism at any time during follow-up for all participants that have had time to complete the program ($n = 408$). In the second column, we examine outcomes for all people that we can follow for at least one year ($n = 357$) and in the final column, we look at outcomes for those we can follow for two years ($n = 263$). For each sample/outcome combination, we report the mean of the outcome for the control sample, the coefficient on the treatment dummy from regression models (1) and (2) above. For each of these estimates, we report the parameter value, the standard error in parentheses, and in curly brackets, the p-value on the test of the null hypothesis that the coefficient equals zero. The addition of the demographic covariates did not significantly alter the estimated impacts and produced minor gains in precision and as a result, we primarily discuss the results from model (1). In these models, the coefficients on the demographic controls are in the expected direction. In Supporting Information Appendix Table A1, we report the coefficients and standard errors on most covariates for the six regressions outcomes associated with offenses that occur any time after assignment. Results suggest that low-income, younger, black males are more likely to recidivate.

In the first row of Table 3, we consider whether a participant was re-arrested for any offense. In the full sample, we find an 8.7 percentage point reduction in this probability ($p = 0.066$), which is a 23 percent reduction in control group mean of 0.379. We find smaller incidence rates in the comparison sample when we follow participants for one year (0.241), and treatment is estimated to reduce offenses by 10.2 percentage points ($p = 0.014$), which is a 42 percent reduction over the control group mean. Even when we follow participants for two years and the sample size falls considerably, we find a 13.2 percentage point reduction ($p = 0.027$). In the full sample, there is suggestive but imprecise evidence that the program reduces misdemeanor arrests ($p = 0.189$). By the two-year follow-up, misdemeanor arrests fell by a statistically significant 10.5 percentage points. In the one-year sample, we find a 10.4 percentage point reduction in felony offenses ($p = 0.001$), which is a 65 percent reduction over the sample mean in the comparison group.

The results in the top half of the table suggest that RFL is especially effective at reducing the chance of arrest for more serious offenses. This result is reinforced in the bottom half of the table where we consider whether participants are arrested and prosecuted for an offense. In the full sample, the chance of being arrested for a prosecuted offense falls by 9.6 percentage points ($p = 0.025$), which is a 31 percent reduction over the control group mean. The effect is most heavily concentrated in felony prosecutions. The reduction for this outcome is 8.5 percentage points ($p = 0.008$) and represents a 45 percent reduction in incidence rates. The results for one-year arrests are large and statistically significant at conventional levels for all prosecuted offenses and prosecuted felony offenses. For this later result, the estimated parameter (–0.101) represents a 75 percent reduction in the incidence of re-arrest for this type of offense. We also find statistically significant effects for prosecuted felony offenses in the two-year re-arrest rates models with a 9.8 percentage point reduction ($p = 0.015$), which is a 51 percent reduction in the offense rate compared to the sample means for the comparison group.

Randomization assigns individuals to either treatment or control conditions, but compliance may be incomplete, so the simple estimates outlined by equations (1) and (2) and reported in Table 3 are referred to as measures of "intention to treat" or ITT. In general, the experiment can only intend to treat a participant. It may be the case that the results are driven exclusively by those that actually complete the treatment program. If this is the case, then we would be interested in calculating the "treatment on the treated" (TOT), which is a measure of what completing the program does to recidivism rates. In this case, the TOT can be calculated via two-stage least-squares and is constructed by dividing the ITT estimates by the fraction completing the program. Since 89 percent of participants assigned to RFL completed the program, the TOT estimates are about 15 percent larger than the corresponding ITT values. The TOT is generated via a simple 2SLS model and the precision of this number is essentially the same as the precision of the ITT estimates.[15]

Counts of Arrests

In Table 4 we report the maximum-likelihood estimates for the negative binomial regressions.[16] The rows in the table are defined the same as in Table 3. For each model we report three sets of numbers. The first is the sample mean of arrests in the control group. The second is the maximum-likelihood estimate, standard error, and p-value on the treatment dummy variable, while the third is the percent reduction in arrest counts (and its standard error) implied by the parameter estimate.

The results in Table 4 are broadly consistent with the results in Table 3; RFL had a much larger impact on the more serious offenses compared to misdemeanors. In the full sample, we see a statistically significant coefficient on the treatment dummy variable and an implied reduction of 36.6 percent in arrest counts for felony offenses, but a statistically insignificant coefficient for misdemeanor offenses of around 20 percent. Looking at the most serious offenses—prosecuted felonies—we see that after one year, RFL participants experienced an 80 percent reduction in these counts and a 51 percent reduction after two years. Both of these estimates are statistically significant at conventional levels.

Heterogeneity in Program Response

In Table 5, we consider the heterogeneity in program response by estimating program effects for subsamples of the population. Although our sample sizes are large compared to most RCT interventions in juvenile diversion, cutting the sample across demographic groups does reduce power considerably. Therefore, we only consider heterogeneity in the treatment across two broad groups at a time (e.g., males and females). To obtain these values, we estimate a specification similar to model (2) above but allow the treatment effect and all other coefficients to vary across the two demographic groups.[17] In the table, we report estimates for prosecuted felonies for the one-year follow-up sample. There are four blocks of numbers and in the blocks we allow the treatment effect to vary across sex, age, family income, and race/ethnicity,

[15] For example, in the full sample the OLS-adjusted ITT estimate (standard error) [t-statistic] for all arrests is –0.081 (0.044) [–1.85]. The 2SLS model that generates the TOT is –0.093 (0.048) [–1.92]. Likewise, in the one-year follow-up samples, the ITT estimate for prosecuted felonies is –0.092 (0.027) [–3.36] while the TOT estimates generated by 2SLS are –0.106 (0.029) [–3.62].
[16] Although not reported in the table, we can easily reject the null that there is no over dispersion, suggesting the negative binomial is more appropriate than the Poisson in this context.
[17] This is equivalent to estimating separate models by subgroups but it allows us to directly test the hypothesis that the coefficients are equals across subgroups.

Table 4. Estimated impact of treatment on arrest counts from Negative Binomial Models: Estimated impact (standard error) {*p*-value}.

	Offense any time after assignment ($n = 408$)			Offense in first year ($n = 357$)			Offense in first two years ($n = 263$)		
Seriousness of offense	Mean of outcome in control group	Maximum-likelihood estimates	Percentage change in arrest counts	Mean of outcome in control group	Maximum-likelihood estimates	Percentage change in arrest counts	Mean of outcome in control group	Maximum-likelihood Estimates	Percentage change in arrest counts
Prosecuted and nonprosecuted									
All offenses	0.944	−0.255 (0.184) {0.166}	−0.225 (0.143)	0.519	−0.721 (0.269) {0.007}	−0.514 (0.131)	0.906	−0.525 (0.248) {0.035}	−0.408 (0.147)
Misdemeanors	0.360	−0.227 (0.225) {0.314}	−0.203 (0.180)	0.166	−0.506 (0.371) {0.173}	−0.360 (0.220)	0.324	−0.568 (0.311) {0.068}	−0.433 (0.176)
Felonies	0.341	−0.456 (0.259) {0.078}	−0.366 (0.164)	0.219	−1.113 (0.462) {0.016}	−0.671 (0.152)	0.338	−0.382 (0.309) {0.216}	−0.317 (0.211)
Prosecuted offenses									
All offenses	0.598	−0.390 (0.199) {0.050}	−0.323 (0.135)	0.294	−0.994 (0.347) {0.004}	−0.630 (0.129)	0.525	−0.989 (0.319) {0.002}	−0.628 (0.119)
Misdemeanors	0.234	−0.306 (0.283) {0.279}	−0.264 (0.208)	0.091	−1.039 (0.702) {0.139}	−0.646 (0.249)	0.187	−1.967 (0.793) {0.013}	−0.860 (0.111)
Felonies	0.262	−0.641 (0.314) {0.041}	−0.473 (0.165)	0.160	−1.599 (0.820) {0.051}	−0.798 (0.166)	0.252	−0.718 (0.430) {0.095}	−0.512 (0.210)

Notes: Standard errors are clustered on the reading group for those in the treatment sample. Heteroskedastic-consistent standard errors are calculated for those in the control group. Other covariates in the regressions are a complete set of dummies for sex, age, race/ethnicity, year, family structure, income quartile, and mother's education.

Table 5. OLS adjusted impact of treatment on prosecuted offenses in the first year, by subgroup: Estimates of impact (standard error) {p-value} [mean outcome in control group].

Treatment ×	Obs.	All offenses	Misdemeanors	Felonies
			By sex	
Male	141	−0.168	−0.053	−0.155
		(0.061)	(0.046)	(0.055)
		{0.007}	{0.249}	{0.005}
		[0.247]	[0.091]	[0.182]
Female	214	−0.081	−0.072	−0.066
		(0.045)	(0.034)	(0.032)
		{0.078}	{0.038}	{0.037}
		[0.155]	[0.073]	[0.100]
p-Value, coefficients are equal		0.277	0.748	0.149
			By age	
<16		−0.085	−0.002	−0.117
		(0.056)	(0.034)	(0.046)
		{0.130}	{0.943}	{0.012}
		[0.174]	[0.054]	[0.141]
≥16		−0.148	−0.091	−0.103
		(0.047)	(0.034)	(0.035)
		{0.002}	{0.009}	{0.004}
		[0.211]	[0.105]	[0.126]
p-Value, coefficients are equal		0.407	0.069	0.882
			By family income	
<Median or income missing	230	−0.164	−0.071	−0.105
		(0.050)	(0.037)	(0.040)
		{0.001}	{0.054}	{0.008}
		[0.233]	[0.108]	[0.150]
≥Median	126	-0.037	-0.019	-0.093
		(0.053)	(0.032)	(0.039)
		{0.486}	{0.552}	{0.019}
		[0.119]	[0.030]	[0.104]
p-Value, coefficients are equal		0.092	0.253	0.824
			By race/ethnicity	
White, non-Hispanic	164	−0.056	−0.025	−0.106
		(0.049)	(0.032)	(0.034)
		{0.252}	{0.429}	{0.002}
		[0.157]	[0.060]	[0.120]
Nonwhite	192	−0.185	−0.089	−0.097
		(0.056)	(0.040)	(0.047)
		{0.001}	{0.028}	{0.041}
		[0.221]	[0.096]	[0.144]
p-Value, coefficients are equal		0.101	0.218	0.881

Notes: Standard errors are clustered on the reading group for those in the treatment sample. Heteroskedastic-consistent standard errors are calculated for those in the control group. Other covariates in the regressions are a complete set of dummies for sex, age, race/ethnicity, year, family structure, income quartile, and mother's education.

respectively. We produce results for the three different outcome measures used in Table 3 (all offenses, misdemeanors, and felonies) and these are represented as different columns. For each regression, we report the OLS estimate of treatment effects for the subgroup, the standard error in parentheses, the p-value on the test of the null that the parameter is zero in curly brackets, and the mean outcome in the control group in square brackets. Below the two groups of estimates, we report the *p*-value for the null hypothesis that the effect is the same across demographic groups.[18]

We initially consider results for males and females. The coefficients in all models for females are negative but the *p*-value indicates statistically significant declines in the misdemeanor and felony offense models and marginal statistical significance ($p = 0.078$) in all offenses. The results for any prosecuted offenses and any prosecuted felonies are much larger for males, where we find statistically significant reductions of 68 and 85 percent, respectively. Only a handful of males in the RFL program were re-arrested for prosecuted felony offenses one year after program completion.

In the next group of results, we consider estimates by age of the participant at the time of randomization. We break the sample roughly in half and consider estimates for those less than 16 years of age and those 16 or older. Adolescents in the control group who enter diversion before the age of 16 have in general a higher prosecuted felony re-arrest rate than those who enter at 16 or older. For the younger groups, we find no evidence for a reduction in prosecuted misdemeanor offenses, but for the older group, there is a large and statistically significant reduction for this class of offense. We find large and statistically significant changes in the probability of being re-arrested for prosecuted felonies in both groups, with the effect marginally larger in absolute terms for the younger arrestees.

In the next block of results, we pool data from the lower half of reported income and those who do not report income and compare these results for those in the top half of reported income. The lower-income group has at least a 44 percent higher recidivism rate across all types of offenses than the higher income group. We find that program assignment reduces the prosecuted misdemeanor offense rate for the lower-income groups by a large amount and a *p*-value of 0.054 while the treatment effect for this outcome among the higher age groups is small and statistically insignificant. For prosecuted felonies, the estimated impact of the program is larger for the high-incidence/lower-income group. Both of these results are statistically significant.

In the final block of estimates, we consider outcomes for white, non-Hispanics versus nonwhite participants. Among all crimes, in the control sample, whites have about a 10.7 percentage point lower recidivism rate compared to nonwhites. For both groups, we find large reductions in prosecuted felonies after one year with a 10.6 ($p = 0.002$) and 9.7 percentage point ($p = 0.041$) reduction for whites and nonwhites, respectively. Among nonwhites, for all offenses, RFL reduces recidivism rates by 18.5 percentage points, or 84 percent of the control group mean ($p = 0.001$). These same numbers for whites are a 5.6 percentage point reduction, which is not statistically significant.

The pattern of results suggests that the program effects tend to be larger for those groups at higher risk for recidivism (males, lower income, nonwhite). Unfortunately, the standard errors are such that in all cases we cannot reject the null that the coefficients are the same across the two groups. Of the 12 regressions in Table 5, at a *p*-value of 0.05, we cannot reject the null that the estimated effects are the same across the two groups in any case.

CONCLUSION

These results suggest that participation in RFL greatly reduces the propensity to recidivate. The impact is especially large for more serious offenses and for participants

[18] When we reduced the sample to these subsamples and examined regression-adjusted difference in means for these subsamples, we maintain a balance in observed characteristics between those in the treatment and control groups.

with observed characteristics that would predict a greater likelihood to recidivate (e.g., males, nonwhites, participants from lower-income families). The effects are also large: participation in RFL reduces re-arrests for prosecuted felony offenses by 9.8 percentage points after two years, which is roughly 50 percent of the sample mean for the control group.

One key question then is whether the program was worth the expense. Since mentors are volunteers, the average cost for program participation is rather low. Program costs have totaled about $224,000 since 2010 or roughly $1,000/person in the treatment group. Our conversations with the county indicate that the average cost of managing a youth in the control program was roughly $300/person, so the marginal cost of RFL per participant was $700 and the additional costs associated with 170 people in the treatment group that we could follow for one year are ($700)(170) = $119,000.

Estimates from Table 4 indicate that RFL assignment reduces counts of prosecuted offenses by 39 percent. Within the control group, there were 66 offenses within this category including seven batteries, seven robberies, 19 thefts, two cases of receiving stolen property, and one case each of fraud and vandalism; the rest were more minor offenses including disorderly conduct, marijuana possession, and running away. McCollister, French, and Fang (2010) estimates the average societal costs for different felonies, ranging from $3532 (in 2008 $) for larceny to $4860 for vandalism, $6462 for burglary, $10,772 for a motor vehicle theft, and $107,020 for an aggravated assault. In the comparison sample, if we monetize the costs associated with the 66 crimes using the numbers in this paper and an estimate of $500/crime for the more minor offenses, the average cost per crime is $17,730, making the overall cost to society for these 66 crimes a total of $1,170,180. If we assume that offenses are reduced by the same amount across all categories, then total costs would fall by 39 percent, saving society $463,370, almost four times the marginal cost of the program. From a cost/benefit standpoint, RFL is a highly effective program.[19]

Despite the long-term secular declines in crime, the large numbers of adults incarcerated in the United States coupled with the fact that most adults start their criminal careers during adolescence make finding ways to reduce recidivism among youth offenders an important policy concern. The RFL program provides one promising avenue to consider. As with most successful RCTs, however, the research asks as many questions as it answers. For example, RFL has a number of unique features: the focus on virtue theory, the use of literature to highlight these virtues, and the use of trained volunteer mentors. Although this is a large RCT compared to others in the juvenile diversion nexus, it is not large enough to test which combination of features led to such dramatic reductions in recidivism. Likewise, it is not clear whether the results can be replicated in other environments. Time will obviously tell. The RFL program is currently being implemented in a second county; and in the county where this data was collected, the program has been expanded to more serious offenders who have been sentenced to detention and those returning to the community from long-term incarceration. Key future goals include testing

[19] These numbers suggest the net benefit for the cost per person treated is roughly $2,008. We can generate a similar number using a regression-based model. For all respondents in the one-year follow-up sample, we used the numbers in McCollister, French, and Fang (2010) and monetize all the crimes committed during the one-year follow-up. So if a participant had one larceny and one vandalism, we would use $3532 + $4860 = $8392. Call this measure the "social cost of crime" and we regress this on the same variables as in model (2) from Table 3. The coefficient (standard error) on this number is $4,624 ($1,713) and given that the marginal cost of treating a person in the experiment is $700, these estimates suggest that the marginal social benefit of treating another person is $3,924.

that the program can be replicated in these other situations and isolating the causal pathways that lead to the program's success.

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APPENDIX A:

Table A1. OLS estimates of recidivism equations, offenses any time after assignment.

	All offenses			Prosecuted offenses		
Covariate	All Offenses	Misds.	Felonies	All Offenses	Misds.	Felonies
Treatment	−0.081	−0.049	−0.072	−0.087	−0.046	−0.080
	(0.044)	(0.040)	(0.032)	(0.039)	(0.037)	(0.029)
Black, non-Hispanic	0.112	0.068	-0.017	0.063	0.030	0.009
	(0.058)	(0.053)	(0.043)	(0.052)	(0.045)	(0.039)
Hispanic	0.035	0.072	−0.095	−0.021	0.013	−0.062
	(0.070)	(0.074)	(0.053)	(0.068)	(0.063)	(0.046)
Male	0.115	0.076	0.136	0.104*	0.061	0.111
	(0.050)	(0.046)	(0.037)	(0.044)	(0.040)	(0.033)
Single parent	0.032	0.010	0.020	−0.013	−0.040	0.062
	(0.065)	(0.061)	(0.051)	(0.059)	(0.051)	(0.044)
1 biological parent partner	0.010	−0.029	−0.010	−0.028	−0.042	0.013
	(0.065)	(0.060)	(0.047)	(0.057)	(0.049)	(0.039)
Other relatives	−0.030	−0.020	−0.055	−0.042	−0.022	−0.022
	(0.102)	(0.094)	(0.073)	(0.090)	(0.079)	(0.065)
Adopted or foster parents	0.026	0.012	−0.170	0.096	0.074	−0.120
	(0.153)	(0.139)	(0.045)	(0.153)	(0.130)	(0.045)
First quartile income	0.225	0.161	0.166	0.274	0.164	0.158
	(0.073)	(0.068)	(0.067)	(0.070)	(0.058)	(0.059)
Second quartile income	0.125	0.115	0.037	0.096	0.058	0.050
	(0.075)	(0.068)	(0.063)	(0.072)	(0.061)	(0.049)
Third quartile income	0.084	0.057	0.043	0.074	0.055	0.037
	(0.064)	(0.055)	(0.056)	(0.061)	(0.050)	(0.045)
Income not reported	0.008	0.047	0.008	0.019	0.033	0.006
	(0.076)	(0.060)	(0.066)	(0.070)	(0.054)	(0.053)
Mom < high school	0.090	0.007	-0.036	0.042	0.034	-0.066
	(0.088)	(0.081)	(0.074)	(0.086)	(0.070)	(0.063)
Mom HS diploma/GED	0.051	0.045	0.050	0.018	0.040	0.034
	(0.075)	(0.067)	(0.063)	(0.070)	(0.060)	(0.055)
Mom some college	0.053	0.037	−0.070	−0.042	−0.043	−0.044
	(0.070)	(0.070)	(0.060)	(0.068)	(0.057)	(0.053)
Mom's educ. not reported	0.017	0.027	−0.001	−0.005	0.042	−0.007
	(0.071)	(0.064)	(0.059)	(0.065)	(0.052)	(0.050)
Age 11	−0.162	−0.247	0.153	−0.136	−0.239	0.142
	(0.176)	(0.167)	(0.169)	(0.184)	(0.171)	(0.172)
Age 12	−0.025	−0.166	0.051	−0.083	-0.233	0.044
	(0.148)	(0.136)	(0.118)	(0.140)	(0.126)	(0.117)
Age 13	0.117	−0.012	0.111	−0.014	−0.187	0.064
	(0.133)	(0.131)	(0.095)	(0.130)	(0.119)	(0.089)
Age 14	−0.098	−0.151	0.004	−0.158	−0.199	−0.068
	(0.122)	(0.119)	(0.090)	(0.121)	(0.114)	(0.084)
Age 15	−0.019	−0.092	0.071	−0.112	−0.140	0.001
	(0.113)	(0.112)	(0.082)	(0.113)	(0.111)	(0.079)
Age 16	−0.024	−0.126	0.086	−0.069	−0.170	0.036
	(0.107)	(0.110)	(0.081)	(0.110)	(0.108)	(0.079)
Age 17	−0.087	−0.132	−0.032	−0.143	−0.189	−0.059
	(0.114)	(0.112)	(0.078)	(0.115)	(0.111)	(0.076)
R^2	0.149	0.091	0.178	0.142	0.074	0.183

Notes: Standard errors are clustered on the reading group for those in the treatment sample. Heteroskedastic-consistent standard errors are calculated for those in the control group. Year dummy variables are not reported.

APPENDIX B:

Related Literature on Juvenile Diversion Programs

The distinctive needs of accused juvenile offenders have led to an increased interest in recent years in finding adjudication and punishment systems that better meet the needs of this group. This effort began in earnest in 1967 when recommendations made by the President's Commission on Law Enforcement and Administration of Justice encouraged the development of local community juvenile diversion programs.[20] These initial programs were rooted in the idea that even processing a juvenile in court may do more harm than good (Lundman, 1993). "Labeling theory," asserts that delinquency can alter one's life course either by negatively impacting self-image or by provoking society to treat the individual with apprehension, disdain, or a lack of trust (Becker, 1963; Link et al., 1989; Matsueda, 1992). Labeling is believed to elicit negative reactions from teachers, peers, family, and state institutions that can, over time, lead to resentment, closed doors, and fewer life opportunities, making subsequent crime more likely (Bernburg & Krohn, 2003; Finn & Fontaine, 1985; Sampson & Laub, 1997; Thornberry et al., 1994; Widom, 1989). Research by Hagan 1993 and Jessor (1991) suggests that low-income youth tend to be judged most severely.

Over time, the types of (and justification for) diversion programs have proliferated. Today, diversion programs are typically designed with one or more of the following goals: a reduction of recidivism and future involvement in the court system, the rehabilitation of juvenile offenders, an increase in system efficiency, and lower court costs (Cocozza et al., 2005; Pogrebin, Poole, & Regoli, 1984). Historically, programs have consisted of a justice component (i.e., police decision, probation supervision, court process) and a service component (Cocozza et al., 2005); however, beyond these basic tenets, programs differ substantially from one another, and few national standards have been established. Diversion programs have taken the form of boot camps, community service projects, individual, group, and family counseling, case management services, and structured in-home family interventions (Cocozza et al., 2005). Programs differ not only in the services they offer, but also in a number of other ways. The point of contact could be with the police, with probation officers, or in court; sometimes the offender is fully adjudicated and sentenced, other times charges may be held in abeyance or expunged; the target population ranges from "Persons in Need of Supervision" and status offenders to felons (Cocozza et al., 2005).

Juvenile diversion programs are widespread; in 2011, about 46 percent of all youth offenders referred to the juvenile justice system underwent some type of informal adjustment.[21] Despite the diversity of interventions, there is relative uniformity on the criterion used for determining program success: the rate of recidivism. This is not surprising given that the outcome has implications for public safety, societal costs, and individual educational and employment outcomes. In addition, recidivism data can be easily obtained via administrative sources at a relatively low cost (Regoli, Wilderman, & Pogrebin, 1983). Unfortunately, evaluative similarities of juvenile diversion programs end in the definition of the key outcome variable. Results from the research are nearly as diverse as program

[20] National Criminal Justice Reference Center (1999).
[21] Office of Juvenile Justice and Delinquency Prevention (2014).

characteristics themselves; therefore, only vague generalizations about diversion as a whole can be made (McCord, Widom, & Crowell, 2001).

Early reviews of the efficacy of juvenile diversion were discouraging: frequently cited in criminal rehabilitation literature is Martinson's (1974, p. 25) claim that ". . . with isolated exceptions, the rehabilitation efforts that have been reported so far have had no appreciable effect on recidivism." His finding was based on an examination of correctional interventions for both juveniles and adults; nonetheless, several literature reviews that focused exclusively on juvenile diversion treatments arrived at a similar conclusion. Several comprehensive reviews spanning five decades of research suggest that there is little consistent evidence that diversion programs reduce recidivism (Martinson, 1974; McGrath, 2008; Schwalbe et al., 2012; Whitehead & Lab, 1989). A 1985 National Academy of Sciences report suggests that one possible explanation for the poor performance of these programs may be the nature of the evidence rather than the programs themselves. In particular, the report noted the shortage of research with credible evaluation designs, such as random assignment experiments. In a meta-analysis of 51 different juvenile program evaluations that included control groups, Whitehead and Lab (1989) found that while a few programs were successful in reducing recidivism, no single intervention type consistently displayed overwhelmingly positive effects, and occasionally diversion program participants recidivated at a greater rate than associated control subjects. A recent meta-analysis limiting its scope to 57 studies with experimental or quasi-experimental design also concluded that diversion's effects were, on average, statistically insignificant, although a few interventions did manage to reduce recidivism (Schwalbe et al., 2012). Moreover, Schwalbe et al. 2012 found only 14 studies that used random assignment, and of this set, only five had more than 300 subjects combined in the treatment and comparison samples.

This might likely be the reason for the ambiguity in results; that is, only a small fraction of studies have taken advantage of experimental designs. As a result, the development of an evidence-base for interventions is still in progress (Patrick & Marsh, 2005; Schwalbe et al., 2012). The 1979 National Academy of Science's (NAS) Panel on Research on Rehabilitative Techniques (Sechrest, White, & Brown, 1979), in response to the disparaging reviews of juvenile diversion of the time, highlighted the possibility that the problem may be in the nature of the evidence from the research rather than in the concepts themselves. In particular, the NAS Panel drew attention to the absence of certain elements essential to credible evaluation research—controlled designs, sensitive measures, and well-implemented treatments (Sechrest, White, & Brown, 1979).

While this is an area that has progressed rapidly in the last 30 years (Schwalbe et al., 2012), not all randomized experiments are equal. The Office of Juvenile Justice and Delinquency Prevention (OJJDP) in the United States Department of Justice reviews programs for at-risk youth across the country and has developed a rating system to identify evidence-based "exemplary" programs. The OJJDP program screening criteria has been unable to identify many evidence-based "exemplary" (highest-rated) diversion programs for youth who have formally entered the juvenile justice system—especially for first-time and less serious offenders. This is, in part, due to ethical concerns that have hindered strong experimental research on such programs and legal issues involving access to juvenile records, but also to relatively few well-conducted impact evaluations.[22] Because of this, and the large number of at-risk adolescents who come into contact with these programs, researchers note

[22] New York State Division of Criminal Justice Services (2006).

that national, evidence-based studies need to be made a priority in order to identify how to redirect juveniles' offending trajectories (Schwalbe et al., 2012).

Although evidence about the effectiveness of juvenile diversion programs is murky at best, some research suggests that programs with a therapeutic or rehabilitative orientation are more likely to be effective in mitigating recidivism. A large meta-analysis by Landenberger and Lipsey 2005 found that programs that attempted to engender personal development by nurturing skills, relationships, and insight were more effective than programs seeking to deter violence or detect bad behavior, suggesting that program staff should see themselves as rehabilitators of wayward youth rather than punishers of juvenile predators. In particular, programs rooted in cognitive behavioral therapy have shown promising effects on recidivism (Heller et al., 2013; Landenberger & Lipsey, 2005; Lipsy, Landenbergerm, & Wilson, 2007; Lipsey et al., 2010; Pearson et al., 2002; Wilson, Bouffard, & Mackenzie, 2005), although models based on other theoretical orientations have rarely been tested with a sound experimental design.

A second class of interventions that has demonstrated some success in curbing delinquent behavior are mentoring programs. Tolan et al. 2014 conducted a meta-analysis of 46 mentoring programs, defined as those in which two individuals interact over an extended period of time, the mentor passes along experience or knowledge to a mentee in position to benefit from it, and the mentor is a volunteer uninvolved in a professional capacity. Among high-risk populations, mentoring—even when combined with other approaches—appears to have had positive effects on delinquency, aggressive behavior, drug use, and academic achievement (Tolan et al., 2014). This is consistent with the prevailing view that mentoring programs most benefit at-risk participants (Dubois et al., 2002; Hamilton & Hamilton, 1992). Programs that emphasize emotional development and include ongoing training for mentors, structured activities, expectations for frequent contact, and overall monitoring of program implementation seem particularly promising (Dubois et al., 2002; Tolan et al., 2014). In the context of Sampson and Laub's (1997) life-course perspective of criminal behavior, this suggests that mentoring may act as a turning point for youth who face a range of economic, family, educational, or interpersonal issues. The programs may prevent delinquents from dropping out of school, associating with high-risk friends or partners, or falling back to criminal behavior; although it is unclear whether these turning points result from mentors imparting practical skills or knowledge, or acting as role models who leave impressions on their mentees.

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