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 non-violent juvenile offenders in a medium-sized Midwestern county. The unique program uses virtue theory, works of literature, and small mentoring groups in an attempt 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 recidivism of more serious offenses for those groups with the highest propensity for future offenses.

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Abstract

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I. 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/or minority youth.[1] Moreover, at approximately 250 youth per 100,000 citizens, the United States leads all industrialized nations in juvenile incarcerations (Annie E. Casey, 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.[2] Using a "willingness to pay" framework, Cohen et al. (2010) calculate that serious juvenile offenders cost society upwards 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; Nagin and Waldfogel, 1995; and Lott, 1990). Released felons have difficulty establishing solid career paths, and often find themselves mired in a series of temporary jobs without benefits (Nagin and Waldfogel, 1993). In a recent working paper that uses variation in incarceration rates for juveniles generated by the random selection of judges, Aizer and Doyle (2013) 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 and Esminger, 1997; Nagin and Paternoster, 2000), although not all youth embroiled in the justice system become adult offenders (Laub and Sampson, 1993; Sampson and Laub, 2003). Even among youth with a high probability of continuing criminal behavior, positive life events may intervene.

[1] FBI, 2012.
[2] U.S. Department of Health and Human Services, 2008.

Social relationships can create opportunities for turning points, or life transitions, which can either reinforce or counteract criminal behavior (Sampson and 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 and 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 and/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 (Pogrebin, Poole, and Regoli, 1984; Cocozza et al., 2005; Cuellar, McReynolds, and Wasserman, 2006). 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 et al., 1983). Unfortunately, evaluative similarities of juvenile diversions programs end in the definition of the key outcome variable. Results from the research are nearly as diverse as program characteristics themselves; therefore, only vague generalizations about diversion as a whole can be made (McCord et al., 2001).[3]

This paper evaluates the impact of a juvenile diversion program implemented in a medium-sized Midwestern town called Reading for Life (RFL). A unique and innovative alternative to

[3] See online Appendix B for a more comprehensive review of the literature on diversion programs.

prosecution in the court system, RFL allows low-status juveniles to study works of literature in small reading groups led by trained volunteer mentors. Informed by classic 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 Reading for Life 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 to other research. In their meta-analysis of 57 studies on this topic, Schwalbe et al. (2012) list 14 RCTs and only four have sample sizes larger than we use here.

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 11.2 percentage point reduction in the probability of having another offense of any type, which is 36 percent reduction over the control group mean. The program was particularly successful at reducing more serious offenses; prosecuted felonies fell by 68 percent over the control group mean (p-value < 0.001). Moreover, RFL was most effective at reducing more serious offenses for groups most likely to recidivate.

In the next section, we outline in detail the RFL program, the study protocols and data collection. In Section III, we outline how key variables are measured and the basic statistical model.

In section IV we present basic results and outline the heterogeneity in results across some various demographic groups. In Section V we make some cost effectiveness calculations, provide some concluding remarks and suggestions for future research.

II. The Reading for Life Diversion Program

a. Participants

The project evaluates the impact of Reading for Life (RFL), a juvenile diversion program run in a mid-size, Midwestern county. Before 2007, the county had a diversion program that consisted of 25 hours of community service over a 16-week period for first- or second-time juvenile offenders with a nonviolent record, aged 11-18. Two phases of pilot research enabled RFL to become the county's largest diversion program and successfully implement it as a randomized control trial (RCT). 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 non-violent offenders aged 11-18 who entered the juvenile justice system between June, 2010 and December 31, 2013. 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 the case a parole officer. Of these cases, 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 this group, 256 cases were referred to parole officers who handled diversion[4] 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 204 were assigned to the control group.

In Table 1, we report the ages of those enrolled in the treatment and control groups by year. In 2010, because volunteer mentor resources were scarcer, the probability of a candidate being assigned to treatment was set at 33 percent, explaining the low fraction entered into the treatment group during that year. In all other years, the probability of an arrestee being assigned to treatment was 50 percent. Accordingly, the fraction in treatment is roughly equal from 2011 through 2013. There is also rough equivalence in the age distribution across the two groups. The peak age for enrollees is 15-16, with 178 participants in this category. There are only 29 adolescents who entered the program aged 11-12.

The RFL program has a detailed intake assessment protocol; only the measures used in this analysis are discussed here. First, 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, family income, youth living situation, and parents'

[4] Parole offers will decide to handle the diversion if there were some previous offenses or a more serious offense that would make the arrestee ineligible for the experiment but maybe a good candidate for diversion. They 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.

education. Next, the RFL program 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, we report for purposes of comparison characteristics of adolescents aged 11-18 from the county of the intervention. This data was collected from the 2008-2012 American Community Survey.[5] In columns 2 and 3, we reports means for the treatment and control samples, respectively. The final column of the table contains the p-value for the test of the null hypothesis that means are the same across both samples. In no case can we reject the null at a p-value of 0.10.

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 11-18, roughly 10 percent are Hispanic, 17 percent are black and 66 percent are white, so black respondents are overrepresented in our sample while whites are under-represented. The average age of those diverted is 15.3 years, which is slightly older than the average age of 11-18 year-olds in the county. Because the program only takes non-violent offenders, a majority of program participants are female.[6] Only one-quarter of program participants are living with both biological parents, which is well below the average for children in the county (56.7%).

Parents were asked to provide annual family income and education levels for both the mother and father. Unfortunately, these two variables are missing in 19 and 33 percent of the time, respectively. When reported, average family income for those in the program is about 14.5 percent lower than the amount for families with children aged 11-18: $38,468 versus $44,989. Likewise,

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[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 (Office of Juvenile Justice and Delinquency Prevention, 2013).

maternal education in the study population appears well below the average education for mothers with children 11-18. Income and education are more likely to be missing in more at-risk families.[7] In our regression models, we produce a categorical variable for both measures and include as a group whether the variable is not reported.[8]

b. Diversion Program

Treatment group members are given a 3-Minute Reading Assessment (Rasinski and 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 ten weeks. RFL mentors are volunteers who have undergone extensive practical and theoretical training, including twelve 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. In his critically acclaimed book *After Virtue,* MacIntyre (1984) argues that our relativistic society is producing a generation of "moral stutters" who are incapable of discerning right from

[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 don't.

[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.

wrong. Sommers (1993) concurs, and suggests that one way out of this ethical dilemma is to explicitly teach virtue theory. There has been a recent revival in the use of stories to foster moral development (Bettlheim, 1976; Coles, 1989; Vitz, 1990; Bruner, 2003 and 2008; McGavock, 2007), specifically that of a virtuous nature (MacIntyre, 1984; Nussbaum, 1990; Carr, 1991; Summers, 1993; Cain, 2005). Literature is uniquely suited to facilitate moral development because of the vicarious experiences and contextual relationships provided within (Vitz, 1990; Cunningham, 2001). 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 frequently focus on personal life reflections that spring from the content of 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.

[9] For example, some groups that have read books which include sick children as main characters have done service projects at the local Ronald McDonald house. Some groups that read books with an environment theme had river clean-up days. Groups that have read books about the Holocaust have performed service projects for the local Jewish Federation.

RFL is distinctive along a number of dimensions, including instruction in classic virtue theory, the inclusion of literature to facilitate moral development, and the engagement of volunteer mentors. There is some data indicating 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 like RFL are more likely to be effective in mitigating recidivism. Cuellar et al. (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 (Lipsy et al., 2007; Lipsey et al., 2010; Landenberger and Lipsey, 2005; Pearson et al., 2002; Wilson et al., 2005; Heller et al., 2013), although models 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 and

[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 in a professional capacity

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

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 (Tolan et al., 2014; Dubois et al., 2002). Consistent with Sampson and Laub's 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.

III. Methods

a. Measuring the Impact on Recidivism

As noted above, the primary outcome in most studies of juvenile diversion programs is whether adolescents recidivate. We will use variants of this measure, as well as arrest counts over time, as our outcomes of interest. Are data are obtained from two sources. First, data on juvenile arrests are 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. RFL staff at the JJC maintain a separate, simplified database exclusively for study participants. This database contains demographic information, psychological and reading test scores (RFL participants only). To form our research dataset, we pulled data on all the re-arrests of study participants and matched to the RFL database by name and birth date. We pulled arrest data in early May of 2014.[12]

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 them we construct six different indicators of recidivism.

[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 data set. We will only "lose" data on participants if they commit a crime outside of the county of residence.

To construct the first three, we measure whether participants were arrested for any offense, then whether they were arrested for a misdemeanor or felony. These offenses may be prosecuted or non-prosecuted. Thus, to construct the final three indicators, we isolate 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 re-offenses 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 both "Did you have any offense?" and "Did you have a prosecuted offense?" will both be 1.

The data at the JJC only includes arrests before age 18. Many of the participants age into their adult years in a few years after completing either the treatment or control group program, so the this data will not accurately measure re-arrests for this older group. In Indiana, all court records are public and we obtained a subscription to the web page doxpop.com and downloaded all offences for study participants that turned 18 sometime during our follow-up. This data will have any arrest that leads to a court appearance so 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 where no such arrest would appear in the adult data.[13]

To complete our analysis, we examine program impact in three different samples. The first sample includes the 408 people that 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

[13] The public adult data from doxpp.com includes traffic violations which we did not include in our analysis.

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 followed for a year or longer and examine one-year recidivism rates, anyone that entered the program after April 31, 2013 would be dropped from the sample. We create our final two samples in this fashion. The second sample includes the 356 people that were followed for their first full year after they are assignment to treatment or control and use it to examine one year recidivism rates. The third is defined similarly for the 262 observations tracked for the first two full years after assignment. The increased size of the first sample must therefore be weighed against 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 difference in means. 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 simply $\hat{\delta} = (\bar{y} \mid d_i = 1) - (\bar{y} \mid d_i = 0) = \bar{y}_1 - \bar{y}_0$. This parameter is obtained by estimating the simple bivariate regression

(1) $$y_i = \alpha + d_i\delta + \varepsilon_i$$

where ε_i is a random error. The estimates in Table 2 indicate that the covariates 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 increase precision so we consider a second model where we estimate the multivariate regression

(2) $$y_i = \alpha + d_i\delta + x_i\beta + \varepsilon_i$$

where x_i is a vector of observed characteristics of program participants taken at the time of enrollment. 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. In these last two cases, one of the controls is whether the variable was not reported. In our tables and corresponding text, we call the estimates for $\hat{\delta}$ from equation (1) the simple difference in means and the corresponding estimates from equation (2) the OLS-adjusted difference in means. Because teens 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.

b. Measuring the Impact on the Counts of Arrests

The measure of recidivism in the previous section only measures 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 non-prosecuted 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 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. In the no-arrest column we see the 11.9 percentage point reduction from the middle columns of Table 3. Comparing treatment to control sample, we also see dramatically smaller counts of one (2.9 versus 12.3 percent), two (0.6 versus 2.1 percent), three (0.6 versus 1.1 percent), and four arrests (0.0 versus 0.5 percent).

The low counts and high fraction of zero re-arrests in Figure mean that OLS models may not provide an accurate way to estimate the impact of RFL on this outcome. Instead, we use a negative binomial model count data model and parameter values are estimated via maximum likelihood. This model is a generalization of the Poisson that allows for over-dispersion. If c_i are the counts of re-arrests for person *i*, then within the negative binomial model, the expected counts are defined as

(3) $$E[c_i \mid d_i, x_i] = \left[e^{\alpha + d_i\delta + x_i\beta}\right] / (1+\theta)$$

where the variables are parameters defined as above and θ is the over-dispersion parameter. If $\theta = 0$ then the model collapses to a standard Poisson count data model. In this model, the coefficient on the treatment dummy variable d_i is equal to $\delta = \ln\left(E[c_i \mid d_i = 1, x_i]\right) - \ln\left(E[c_i \mid d_i = 0, x_i]\right)$ which is approximately the percentage change in expected re-arrests between the treatment and control group. Standard errors are calculated using the same clustering procedure for the OLS regression outlined in the previous section. The approximation to percentage changes is only accurate for small values of δ 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.

IV. Results

a. Recidivism

Basic estimates for the six outcomes, three sample and two estimation methods 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=356) and in the final column, we look at outcomes for those we can follow for two years (n=262). For each sample/outcome combination, we report the mean of the outcome for the control sample, the simple difference in means and the OLS-adjusted difference in means. For the treatment effect 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 covariates did not significantly alter the estimated impacts and produced minor gains in precision. As a result, we will discuss the estimates for the simple difference in means. In the multivariate models, the coefficients on the other covariates are of an expected direction. In Appendix Table A1 we report the coefficients and standard errors on all covariates for the six regressions outcomes associated with offenses that occur any time after enrollment.[14]

In the first row of the table we consider whether a participant was re-arrested for any other offense. In the full sample, we find a 10.5 percentage point reduction in this probability (p= 0.024) which is a 27.7 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 10.5 percentage points (p=0.011), which is a 43.6 percent reduction over the control group mean. Even in the sample where we follow participants for two years and the sample size falls considerably, we find a 12.5 percent reduction (p=0.028). In the full sample, there is suggestive but imprecise evidence that the program reduces misdemeanor arrests (p=0.167). In the one year sample we find a11.9 percent point reduction in felony offenses (p<0.001) which is a 74.3 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

[14] The results in the appendix suggest that low income, younger, black males are more likely to recidivate.

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 11.8 percentage points (p= 0.006), which is a 38.3 percent reduction over the control group mean. The effect is most heavily concentrated in felony prosecutions. The reduction for this outcome is 11.0 percentage points (P<0.001) and represents a 58.8 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 (-0.116) represents an 86.6 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 12.2 percentage point reduction (p=0.004), which is a 62.9 percent reduction in the offense rate compared to the sample means for the comparison group.

Randomization assigns individuals to either treatment or control but compliance may be incomplete, so the simple estimates outlined by equations (1) and (2) and reported in Tables 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 12 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] 3

[15] For example, in the full sample the OLS-adjusted ITT estimate (standard error) [t-statistic] for all arrests is -0.099 (0.041) [-2.39]. The 2SLS model that generates the TOT is -0.113 (0.046) [-2.44]. Likewise, in the one-year follow-up samples, the ITT estimate for prosecuted felonies is -0.103 (0.028) [-3.67] while the TOT estimates generated by 2SLS are -0.115 (0.287) [-4.01].

b. 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 $\hat{\delta}$ and an implied reduction of 45.2 percent in arrest counts for felony offenses, but a statistically insignificant coefficient for misdemeanor offenses of around 6.3 percent. Looking at the most serious offenses—prosecuted felonies—we see that after one year, RFL participants experienced an 85.5 percent reduction in these counts and a 66.3 percent reduction after two years. Both of these estimates are statistically significant at conventional levels.

c. 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 breaking the sample into two broad groups at a time (e.g., males and females). In the table, we report the OLS-adjusted treatment effects for

[16] Although not reported in the table, we can easily reject the null that θ=0 in all models, suggesting the negative binomial is more appropriate than the Poisson in this context.

offenses committed one year after program completion. We produce results for the six different outcome measures used in Table 3. For each set of treatment effects, we present the OLS estimate of $\hat{\delta}$ from equation (2), 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.

We initially consider results for males and females. There is suggestive evidence that the program works for females. The parameter estimates are always negative but the p-values are frequently in excess of 0.05. Finding statistically significant program impacts is made more difficult in this case by the fact that re-arrest rates for females are about one-third of the rates for males. Nonetheless, the strongest results for females are for re-arrest among prosecuted felony offenses which fall by 7.7 percentage points [p=0.019] which is 77 percent of the control group mean. In contrast, the results for all offenses show a 5.9 percentage point reduction [p=0.240].

The results are much more precise for males, where we find a statistically significant reduction in arrests ($p \leq .05$) for any felony arrests and prosecuted felony offenses. These estimates are large; in both cases the treatment effect is greater than 90 percent of the sample mean for the comparison. Virtually none of the 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 re-arrest rate than those that enter at 16 or older. For both groups, we find no evidence that there is a reduction in misdemeanor offenses, but large changes in the probability of being re-arrested for prosecuted felonies.

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.

In general, the lower income group has higher recidivism rates across all types of offenses. For the prosecuted felony offenses, the baseline recidivism rate is much higher for the lower income/income not reported group (15 versus 10.4 percent), and 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 non-white participants. Among all crimes, in the control sample, whites have about a 10.7 percentage point lower recidivism rate compared to non-whites. For both groups we find large reductions in prosecuted felonies after one year with a 11.5 ($p=0.003$) and 12.2 percentage point ($p=0.008$) reduction for whites and non-whites, respectively. Among non-whites, for all offenses, RFL reduces recidivism rates by 19.0 percentage points, or 65.9 percent of the control group mean ($p = .002$). These same numbers for whites are a 5.2 percentage point reduction which is not statistically significant and is only 28.7 percent of the sample mean.

The results in Table 5 are only suggestive that the estimated impacts differ across groups. The pattern of results indicates that the program effects seems to be larger for those groups with a higher propensity to commit a crime. 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.

V. 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 with observed characteristics that would predict a greater likelihood to recidivate (e.g., males, non-whites, participants from lower income families). The effects are also large: participation in RFL reduces re-arrests for prosecuted felony offenses by 11.6 percent after one year and 12.2 percent after two years. These numbers are 86.6 and 62.9 percent of the sample mean recidivism rates 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. Total program costs have totaled about $224,000 since 2010 or roughly $1000/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 168 people that we could follow for one year are ($700)(168) = $117,600.

Estimates from Table 4 indicate that RFL assignment reduces counts of prosecuted offenses by 50.9 percent. Within the control group, there were 53 offenses within this category including 4 batteries, 7 robberies, 20 thefts, 2 cases of vandalism, and 1 case each of fraud and receiving stolen property; the rest were more minor offenses including disorderly conduct, marijuana possession and running away. In a recent paper, McCollister et al. (2010) estimate 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 53 crimes using the numbers in this paper and an estimate of $500/crime for the more minor offenses, the average cost per crime is $15,275, making the overall cost to society for these 53 crimes a total of $809,575. If we assume that offenses are reduced by the same amount across all categories, [1] then total costs would fall by 50.9 percent, saving society $412,074, about three and a half times the marginal cost of the program. From a cost/benefit standpoint, RFL is a highly effective program.

Despite the long-term secular declines in crime, the large numbers of adults incarcerated in the U.S. and the fact that most adult criminals 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 tried in another county and in the current county that is the focus of this work, the program has been expanded to include youths sentenced to detention. Key future goals include testing 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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Figure 1: Flow Chart of Teen Arrestees into Experiment

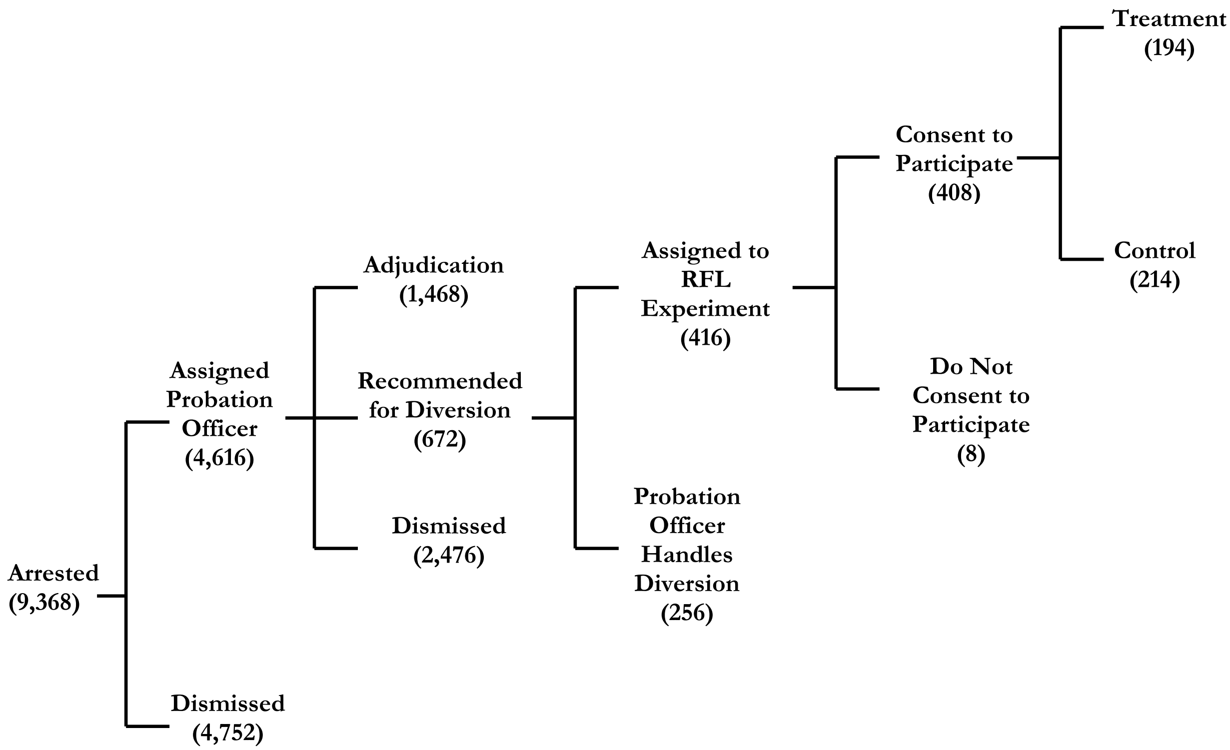
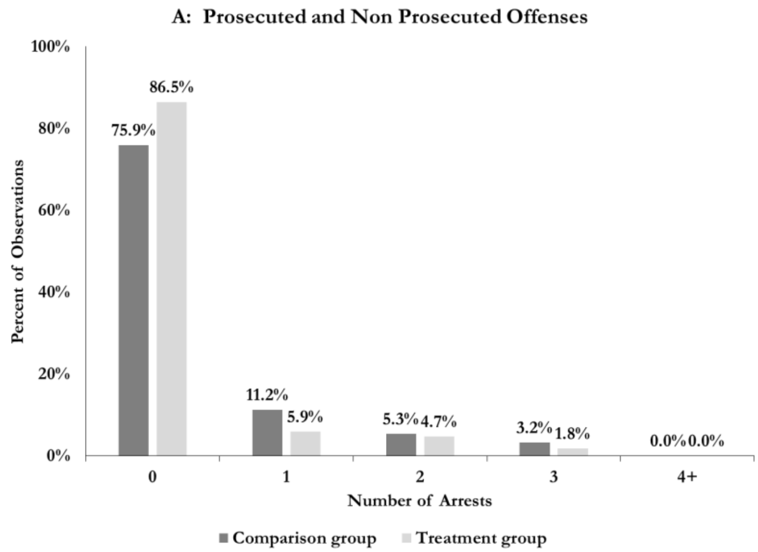


Figure 2
Histogram of Arrest Counts within the First Year



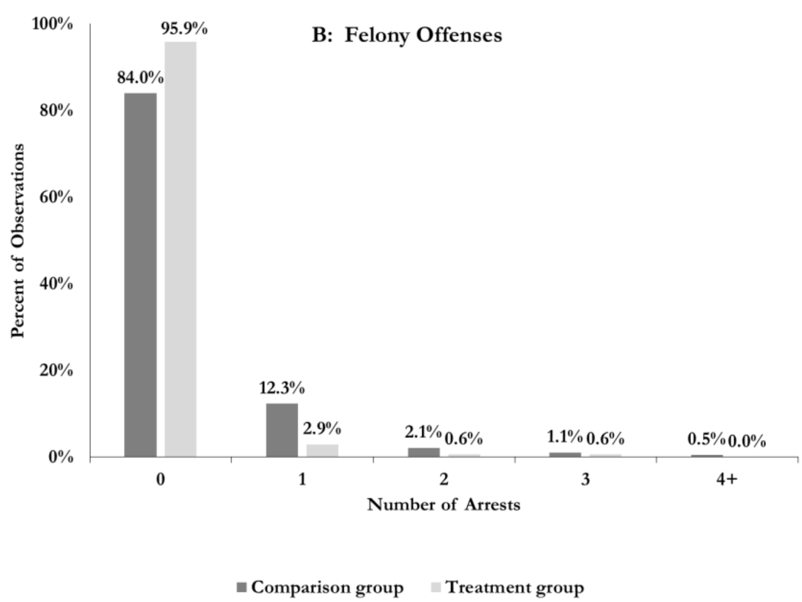


Table 1
Age of Participants by Year and by Program

	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

Table 2
Sample Characteristics for Treatment and Control Groups

	2008-2012 ACS, 11-18 years old in county of intervention	Proportion of Sample: Treatment group *Reading for Life*	Proportion of Sample: Control Group	P-value on test that means are the same across samples
Completed Program		0.887	0.888	0.979
Race/Ethnicity dummy variables				
White, non-Hispanic	0.664	0.479	0.435	0.365
Black, non-Hispanic	0.173	0.294	0.313	0.673
Asian, non-Hispanic	0.012	0.010	0.019	0.484
Hispanic	0.097	0.119	0.117	0.957
Multiracial/Ethnic	0.080	0.093	0.098	0.855
Other or unknown	0.017	0.005	0.014	0.365
Age upon entry	14.712	15.263	15.280	0.916
Male dummy variable	0.518	0.376	0.421	0.363
Household type dummy variables				
Both biological parents	0.567	0.258	0.282	0.588
Single parent	0.295	0.397	0.418	0.669
1 biological parent and partner*		0.247	0.224	0.583
Other relatives	0.035	0.098	0.061	0.168
Adopted or foster parents	0.014	0.021	0.023	0.845
Family Income				
Median if reported	44,989	37,770	39,317	0.648
Income not reported	0.075	0.175	0.210	0.373
Mother's education dummy variables				
Less than high school	0.097	0.119	0.136	0.609
High school diploma or GED	0.300	0.216	0.248	0.458
Some college education	0.238	0.170	0.150	0.572
College degree or higher	0.365	0.170	0.131	0.268
Mother's education not reported	N/A	0.325	0.336	0.802
Sample size	29,895	194	214	

*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 non-married household.

Table 3
Estimated Impact of Treatment on Recidivism
Estimated impact (Standard error) {P-value on null that impact is zero}

Seriousness of offense	Offense at any time after enrollment (n = 408) Mean of outcome in control group	Difference in means	OLS adjusted	Offense in first year (n = 357) Mean of outcome in control group	Difference in means	OLS adjusted	Offense in first two years (n = 263) Mean of outcome in control group	Difference in means	OLS adjusted
Prosecuted and Non-Prosecuted									
All offenses	0.379	-0.105	-0.098	0.241	-0.105	-0.112	0.367	-0.125	-0.127
		(0.046)	(0.046)		(0.041)	(0.039)		(0.057)	(0.058)
		{0.024}	{0.034}		{0.011}	{0.004}		{0.028}	{0.031}
Misdemeanor offenses	0.248	-0.057	-0.051	0.123	-0.035	-0.044	0.230	-0.077	-0.086
		(0.041)	(0.041)		(0.033)	(0.030)		(0.049)	(0.048)
		{0.167}	{0.208}		{0.289}	{0.151}		{0.116}	{0.077}
Felony offenses	0.220	-0.106	-0.095	0.160	-0.119	-0.113	0.237	-0.108	-0.097
		(0.037)	(0.034)		(0.032)	(0.032)		(0.048)	(0.044)
		{0.004}	{0.006}		{0.000}	{0.000}		{0.024}	{0.030}
Prosecuted Offenses									
All offenses	0.308	-0.118	-0.106	0.193	-0.122	-0.123	0.295	-0.182	-0.178
		(0.043)	(0.040)		(0.036)	(0.033)		(0.049)	(0.047)
		{0.006}	{0.009}		{0.001}	{0.000}		{0.000}	{0.000}
Misdemeanor offenses	0.192	-0.068	-0.054	0.080	-0.051	-0.051	0.158	-0.134	-0.127
		(0.036)	(0.037)		(0.024)	(0.025)		(0.035)	(0.037)
		{0.062}	{0.145}		{0.037}	{0.047}		{0.000}	{0.001}
Felony offenses	0.187	-0.110	-0.103	0.134	-0.116	-0.108	0.194	-0.122	-0.119
		(0.034)	(0.030)		(0.028)	(0.028)		(0.042)	(0.039)
		{0.001}	{0.001}		{0.000}	{0.000}		{0.004}	{0.002}

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, family structure, income quartile, and mother's education.

Table 4
Estimated Impact of Treatment on Counts of Arrest from Negative Binomial Model
Estimated impact (Standard error) {P-value on null that impact is zero}

	Offense at any time (n = 408)			Offense in first year (n = 356)			Offense in first two years (n = 262)		
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 Non-Prosecuted				
All offenses	0.944	-0.317 (0.184) {0.085}	-0.272 (0.134)	0.519	-0.710 (0.251) {0.005}	-0.508 (0.123)	0.906	-0.463 (0.230) {0.044}	-0.370 (0.145)
Misdemeanors	0.360	-0.236 (0.223) {0.291}	-0.210 (0.176)	0.166	-0.405 (0.341) {0.235}	-0.333 (0.227)	0.324	-0.403 (0.288) {0.162}	-0.332 (0.193)
Felonies	0.341	-0.602 (0.244) {0.014}	-0.452 (0.133)	0.219	-1.312 (0.430) {0.002}	-0.731 (0.116)	0.338	-0.601 (0.252) {0.017}	-0.452 (0.138)
					Prosecuted Offenses				
All offenses	0.598	-0.509 (0.194) {0.009}	-0.399 (0.117)	0.294	-1.086 (0.316) {0.001}	-0.663 (0.107)	0.525	-0.995 (0.273) {0.000}	-0.630 (0.101)
Misdemeanors	0.234	-0.364 (0.275) {0.186}	-0.305 (0.191)	0.091	-0.985 (0.664) {0.138}	-0.626 (0.248)	0.187	-1.768 (0.785) {0.024}	-0.829 (0.134)
Felonies	0.262	-0.858 (0.280) {0.002}	-0.576 (0.119)	0.160	-1.934 (0.651) {0.003}	-0.855 (0.094)	0.252	-1.087 (0.338) {0.001}	-0.663 (0.114)

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, family structure, income quartile, and mother's education.

Table 5
OLS Adjusted Impact of Reading for Life Treatment on Offenses in the First Year, By Subgroup
Estimates impact (Standard error) {P-value} [Mean outcome in control group]

		Prosecuted and non-prosecuted offenses			Prosecuted offenses		
Group	Obs.	All offenses	Misdemeanor offenses	Felony offenses	All offenses	Misdemeanor offenses	Felony offenses
By sex							
Males	141	-0.197	-0.031	-0.228	-0.187	-0.042	-0.172
		(0.071)	(0.062)	(0.058)	(0.058)	(0.041)	(0.054)
		{0.007}	{0.618}	{0.000}	{0.002}	{0.305}	{0.002}
		[0.338]	[0.182]	[0.234]	[0.247]	[0.091]	[0.182]
Females	215	-0.059	-0.060	-0.049	-0.088	-0.071	-0.077
		(0.050)	(0.037)	(0.039)	(0.046)	(0.034)	(0.033)
		{0.240}	{0.106}	{0.209}	{0.057}	{0.039}	{0.019}
		[0.173]	[0.082]	[0.109]	[0.155]	[0.073]	[0.100]
By age							
<16	172	-0.076	0.039	-0.154	-0.085	0.009	-0.121
		(0.060)	(0.047)	(0.049)	(0.055)	(0.032)	(0.047)
		{0.205}	{0.401}	{0.002}	{0.122}	{0.770}	{0.011}
		[0.217]	[0.098]	[0.174]	[0.174]	[0.054]	[0.141]
≥16	184	-0.152	-0.097	-0.107	-0.172	-0.093	-0.131
		(0.058)	(0.044)	(0.046)	(0.049)	(0.035)	(0.040)
		{0.009}	{0.029}	{0.021}	{0.001}	{0.009}	{0.001}
		[0.263]	[0.147]	[0.147]	[0.211]	[0.105]	[0.126]

Table 5 (Continued)
OLS Adjusted Impact of Reading for Life Treatment on Offenses in the First Year, By Subgroup
Estimates impact (Standard error) {P-value} [Mean outcome in control group]

		Prosecuted and non-prosecuted offenses			Prosecuted offenses		
Group	Obs.	All offenses	Misdemeanor offenses	Felony offenses	All offenses	Misdemeanor offenses	Felony offenses
By Income							
Income below median or missing	230	-0.155	-0.075	-0.115	-0.173	-0.063	-0.117
		(0.053)	(0.043)	(0.043)	(0.047)	(0.034)	(0.039)
		{0.004}	{0.086}	{0.008}	{0.000}	{0.067}	{0.003}
		[0.283]	[0.175]	[0.167]	[0.233]	[0.108]	[0.150]
Income above median	126	-0.045	0.009	-0.115	-0.044	-0.019	-0.102
		(0.065)	(0.038)	(0.055)	(0.057)	(0.034)	(0.044)
		{0.495}	{0.807}	{0.039}	{0.444}	{0.576}	{0.023}
		[0.164]	[0.030]	[0.149]	[0.119]	[0.030]	[0.104]
By Race							
White, non-Hispanic	165	-0.052	0.014	-0.100	-0.073	-0.025	-0.115
		(0.057)	(0.037)	(0.047)	(0.051)	(0.032)	(0.037)
		{0.368}	{0.702}	{0.035}	{0.155}	{0.433}	{0.003}
		[0.181]	[0.060]	[0.157]	[0.157]	[0.060]	[0.120]
Non-white	191	-0.190	-0.108	-0.135	-0.189	-0.073	-0.122
		(0.060)	(0.049)	(0.049)	(0.052)	(0.038)	(0.045)
		{0.002}	{0.031}	{0.006}	{0.000}	{0.056}	{0.008}
		[0.288]	[0.173]	[0.163]	[0.221]	[0.096]	[0.144]

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, family structure, income quartile, and mother's education.

Appendix Table A1

OLS Estimates of Recidivism Equations, Offenses Any Time after Enrollment

	All offenses			Prosecuted offenses		
Covariate	All offenses	Misd. Offenses	Felony offenses	All offenses	Misd. offenses	Felony Offenses
Reading for Life dummy	-0.099	-0.023	-0.104	-0.112	-0.036	-0.119
	(0.041)	(0.033)	(0.033)	(0.033)	(0.027)	(0.029)
Black, non-Hispanic	0.057	0.024	-0.029	0.026	0.004	0.004
	(0.052)	(0.045)	(0.043)	(0.044)	(0.035)	(0.039)
Hispanic	0.028	0.039	-0.084	-0.022	-0.012	-0.043
	(0.072)	(0.065)	(0.058)	(0.063)	(0.051)	(0.048)
Male	0.140	0.093	0.150	0.122	0.075	0.119
	(0.044)	(0.035)	(0.037)	(0.038)	(0.028)	(0.032)
Single Parent	0.040	0.027	0.014	-0.004	-0.024	0.065
	(0.059)	(0.054)	(0.048)	(0.050)	(0.040)	(0.041)
1 biological parent partner	0.024	-0.030	0.005	-0.033	-0.054	0.037
	(0.061)	(0.050)	(0.048)	(0.048)	(0.033)	(0.039)
Other relatives	-0.071	-0.109	-0.017	-0.102	-0.129	0.014
	(0.082)	(0.074)	(0.071)	(0.067)	(0.054)	(0.063)
Adopted or foster parents	0.006	-0.029	-0.130	0.053	0.013	-0.073
	(0.122)	(0.112)	(0.039)	(0.126)	(0.106)	(0.035)
First quartile income	0.187	0.152	0.147	0.244	0.149	0.137
	(0.072)	(0.058)	(0.069)	(0.068)	(0.042)	(0.057)
Second quartile income	0.070	0.103	0.022	0.068	0.061	0.038
	(0.067)	(0.052)	(0.062)	(0.060)	(0.040)	(0.049)
Third quartile income	0.092	0.075	0.059	0.096	0.079	0.032
	(0.066)	(0.047)	(0.058)	(0.056)	(0.036)	(0.044)
Income not reported	0.010	0.091	0.006	0.040	0.082	-0.002
	(0.073)	(0.051)	(0.064)	(0.066)	(0.042)	(0.050)
Mom < high school	0.094	-0.003	-0.033	0.032	0.013	-0.064
	(0.084)	(0.065)	(0.077)	(0.079)	(0.049)	(0.063)
Mom HS diploma/GED	0.123	0.089	0.055	0.086	0.082	0.019
	(0.069)	(0.054)	(0.064)	(0.065)	(0.043)	(0.056)
Mom some college	0.087	0.049	-0.053	-0.028	-0.038	-0.036
	(0.067)	(0.053)	(0.063)	(0.059)	(0.028)	(0.056)
Mom's educ. not reported	0.035	0.051	-0.016	0.005	0.060	-0.024
	(0.067)	(0.056)	(0.062)	(0.060)	(0.036)	(0.054)
Age 11	0.103	0.045	0.205	0.127	0.061	0.183
	(0.162)	(0.143)	(0.169)	(0.163)	(0.137)	(0.176)
Age 12	0.229	0.099	0.120	0.165	0.036	0.104
	(0.123)	(0.100)	(0.111)	(0.113)	(0.086)	(0.110)
Age 13	0.333	0.227	0.133	0.207	0.067	0.085
	(0.114)	(0.097)	(0.100)	(0.107)	(0.079)	(0.094)
Age 14	0.113	0.083	0.045	0.057	0.045	-0.051
	(0.100)	(0.076)	(0.091)	(0.093)	(0.067)	(0.083)
Age 15	0.136	0.077	0.086	0.049	0.042	0.020
	(0.089)	(0.065)	(0.083)	(0.083)	(0.060)	(0.080)
Age 16	0.089	0.031	0.075	0.049	0.007	0.028
	(0.087)	(0.063)	(0.082)	(0.084)	(0.055)	(0.080)
Age 17	-0.005	-0.009	-0.048	-0.038	-0.038	-0.070
	(0.083)	(0.055)	(0.075)	(0.079)	(0.048)	(0.073)
R^2	0.162	0.122	0.136	0.167	0.110	0.147

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.

Appendix Table A2

Sample Characteristics for Treatment and Control Groups, Males

	Proportion of Sample		
	Treatment group *Reading for Life*	Control Group	P-value on test that means are the same across samples
Completed Program	0.806	0.833	0.649
Race/Ethnicity dummy variables			
White, non-Hispanic	0.370	0.422	0.500
Black, non-Hispanic	0.315	0.333	0.806
Asian, non-Hispanic	0.014	0.000	0.268
Hispanic	0.151	0.133	0.753
Multiracial/Ethnic	0.137	0.078	0.221
Other or unknown	0.014	0.022	0.689
Age upon entry	15.096	14.967	0.621
Male dummy variable	1.000	1.000	.
Household type dummy variables			
Both biological parents	0.205	0.270	0.345
Single parent	0.452	0.416	0.645
1 biological parent and partner	0.233	0.222	0.873
Other relatives	0.096	0.067	0.510
Adopted or foster parents	0.014	0.022	0.683
Family Income			
Median if Reported	33,683	37,685	0.465
Income not reported	0.192	0.244	0.423
Mother's education dummy variables			
Less than high school	0.164	0.156	0.879
High school diploma or GED	0.164	0.189	0.686
Some college education	0.151	0.156	0.932
College degree or higher	0.096	0.122	0.596
Mother's education nor reported	0.425	0.378	0.546
Sample size	90	73	

Appendix Table A3
Sample Characteristics for Treatment and Control Groups, Females

	Proportion of Sample		
	Treatment group *Reading for Life*	Control Group	P-value on test that means are the same across samples
Completed Program	0.933	0.926	0.816
Race/Ethnicity dummy variables			
White, non-Hispanic	0.545	0.444	0.112
Black, non-Hispanic	0.281	0.298	0.765
Asian, non-Hispanic	0.008	0.032	0.186
Hispanic	0.099	0.105	0.884
Multiracial/Ethnic	0.066	0.113	0.202
Other or unknown	0.000	0.008	0.324
Age upon entry	15.364	15.508	0.500
Male dummy variable	0.000	0.000	.
Household type dummy variables			
Both biological parents	0.289	0.290	0.985
Single parent	0.364	0.419	0.374
1 biological parent and partner	0.256	0.226	0.580
Other relatives	0.099	0.056	0.213
Adopted or foster parents	0.025	0.024	0.976
Family Income			
Median if Reported	40,157	40,415	0.952
Income not reported	0.165	0.185	0.679
Mother's education dummy variables			
Less than high school	0.091	0.121	0.447
High school diploma or GED	0.248	0.290	0.457
Some college education	0.182	0.145	0.440
College degree or higher	0.215	0.137	0.110
Mother's education nor reported	0.264	0.306	0.469
Sample size	124	121	

Appendix Table A4
Sample Characteristics for Treatment and Control Groups, At or Below Median Income or Missing Income

	Proportion of Sample		
	Treatment group *Reading for Life*	Control Group	P-value on test that means are the same across samples
Completed Program	0.863	0.851	0.782
Race/Ethnicity dummy variables			
White, non-Hispanic	0.352	0.296	0.339
Black, non-Hispanic	0.376	0.400	0.693
Asian, non-Hispanic	0.008	0.015	0.609
Hispanic	0.152	0.141	0.798
Multiracial/Ethnic	0.112	0.126	0.730
Other or unknown	0.000	0.015	0.173
Age upon entry	14.976	15.089	0.602
Male dummy variable	0.416	0.467	0.413
Household type dummy variables			
Both biological parents	0.144	0.187	0.360
Single parent	0.544	0.537	0.914
1 biological parent and partner	0.216	0.185	0.537
Other relatives	0.104	0.075	0.408
Adopted or foster parents	0.008	0.030	0.203
Family Income			
Median if Reported	18,282	16,653	0.221
Income not reported	0.272	0.333	0.284
Mother's education dummy variables			
Less than high school	0.168	0.141	0.545
High school diploma or GED	0.224	0.289	0.234
Some college education	0.160	0.163	0.949
College degree or higher	0.088	0.030	0.044
Mother's education nor reported	0.360	0.378	0.768
Sample size	135	125	

Appendix Table A5
Sample Characteristics for Treatment and Control Groups, Above Median Income

	Proportion of Sample		
	Treatment group *Reading for Life*	Control Group	P-value on test that means are the same across samples
Completed Program	0.926	0.948	0.594
Race/Ethnicity dummy variables			
White, non-Hispanic	0.710	0.671	0.610
Black, non-Hispanic	0.145	0.165	0.744
Asian, non-Hispanic	0.014	0.025	0.644
Hispanic	0.058	0.076	0.666
Multiracial/Ethnic	0.058	0.051	0.845
Other or unknown	0.014	0.013	0.924
Age upon entry	15.783	15.608	0.469
Male dummy variable	0.304	0.342	0.630
Household type dummy variables			
Both biological parents	0.464	0.443	0.802
Single parent	0.130	0.215	0.179
1 biological parent and partner	0.304	0.291	0.862
Other relatives	0.087	0.038	0.216
Adopted or foster parents	0.043	0.013	0.252
Family Income			
Median if Reported	63,471	65,136	0.718
Income not reported	0.000	0.000	.
Mother's education dummy variables			
Less than high school	0.029	0.127	0.030
High school diploma or GED	0.203	0.177	0.693
Some college education	0.188	0.127	0.304
College degree or higher	0.319	0.304	0.845
Mother's education nor reported	0.261	0.266	0.946
Sample size	85	79	

Appendix Table A6
Sample Characteristics for Treatment and Control Groups, Below Median Age

	Proportion of Sample		
	Treatment group *Reading for Life*	Control Group	P-value on test that means are the same across samples
Completed Program	0.889	0.885	0.926
Race/Ethnicity dummy variables			
White, non-Hispanic	0.385	0.394	0.891
Black, non-Hispanic	0.363	0.327	0.603
Asian, non-Hispanic	0.022	0.010	0.487
Hispanic	0.154	0.135	0.704
Multiracial/Ethnic	0.077	0.106	0.490
Other or unknown	0.000	0.019	0.185
Age upon entry	13.758	13.875	0.486
Male dummy variable	0.462	0.519	0.424
Household type dummy variables			
Both biological parents	0.220	0.262	0.495
Single parent	0.396	0.476	0.264
1 biological parent and partner	0.275	0.192	0.175
Other relatives	0.121	0.068	0.207
Adopted or foster parents	0.022	0.010	0.492
Family Income			
Median if Reported	32,390	30,614	0.656
Income not reported	0.220	0.212	0.890
Mother's education dummy variables			
Less than high school	0.154	0.183	0.594
High school diploma or GED	0.165	0.221	0.324
Some college education	0.154	0.125	0.563
College degree or higher	0.110	0.106	0.927
Mother's education nor reported	0.418	0.365	0.458
Sample size	104	91	

Appendix Table A7
Sample Characteristics for Treatment and Control Groups, Above Median Age

	Proportion of Sample		
	Treatment group *Reading for Life*	Control Group	P-value on test that means are the same across samples
Completed Program	0.882	0.888	0.901
Race/Ethnicity dummy variables			
White, non-Hispanic	0.563	0.473	0.189
Black, non-Hispanic	0.233	0.300	0.272
Asian, non-Hispanic	0.000	0.027	0.092
Hispanic	0.087	0.100	0.754
Multiracial/Ethnic	0.107	0.091	0.699
Other or unknown	0.010	0.009	0.963
Age upon entry	16.592	16.609	0.850
Male dummy variable	0.301	0.327	0.681
Household type dummy variables			
Both biological parents	0.291	0.300	0.890
Single parent	0.398	0.364	0.607
1 biological parent and partner	0.223	0.255	0.595
Other relatives	0.078	0.055	0.498
Adopted or foster parents	0.019	0.036	0.457
Family Income			
Median if Reported	42,062	47,519	0.288
Income not reported	0.136	0.209	0.160
Mother's education dummy variables			
Less than high school	0.087	0.091	0.928
High school diploma or GED	0.262	0.273	0.862
Some college education	0.184	0.173	0.824
College degree or higher	0.223	0.155	0.201
Mother's education nor reported	0.243	0.309	0.282
Sample size	110	103	

Online Appendix B.

Related Literature on Juvenile Diversion Programs

The distinctive needs of accused juvenile offenders have led in recent years to an increased interest 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.[17] 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 (Sampson and Laub, 1997; Thornberry et al., 1994; Finn and Fontaine, 1985; Widom, 1989; Bernburg and Krohn, 2003). 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 (Pogrebin et al., 1984; Cocozza et al., 2005). 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

[17]National Criminal Justice Reference Center, 1999.

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% of all youth offenders referred to the juvenile justice system underwent some type of informal adjustment.[18] 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 et al., 1983). Unfortunately, evaluative similarities of juvenile diversions programs end in the definition of the key outcome variable. Results from the research are nearly as diverse as program characteristics themselves; therefore, only vague generalizations about diversion as a whole can be made (McCord et al., 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 an examination of correctional interventions for both juveniles and adults; nonetheless, several literature reviews that focused exclusively on juvenile diversion

[18] OJJDP, 2014.

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; Whitehead and Lab, 1989; McGrath, 2008; Schwalbe et al., 2012). 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 (1985) 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 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 and Marsh, 2005; Schwalbe et al., 2012). The 1979 National Academy of Science's (NAS) Panel on Research on Rehabilitative Techniques, 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 et al., 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.[19] Because of this, and the large number of at-risk adolescents who come into contact with these programs, researchers note 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 which 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 (Lipsy et al., 2007; Lipsey et al., 2010;

[19]NY State Division of Criminal Justice Services, 2006.

Landenberger and Lipsey, 2005; Pearson et al., 2002; Wilson et al., 2005; Heller et al., 2013), although models based on other theoretical orientations have rarely been tested with a sound experimental design.

A second class of interventions that have 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 – appeared to have 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 and 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 (Tolan et al., 2014; Dubois et al., 2002). In the context of Sampson and Laub's 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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