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 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 for those groups with the highest propensity for future offenses.

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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, Piquero, and Jennings (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 & Waldfogel, 1995; Nagin, Waldfogel, & Lott, 1990). 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 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 & Esminger, 1997; Nagin & Paternoster, 2000), although not all youth embroiled in the justice system become adult offenders (Laub & Sampson, 1993; Sampson & Laub, 2003). Even among youth with a high probability of continuing criminal behavior, positive life events may intervene. Social

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

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 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, & Regoli, 1984; Cocozza et al., 2005; Cuellar, McReynolds, & 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, Wilderman, & Pogrebin, 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, Widom, & Crowell, 2001).[3]

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

[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 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 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 and colleagues (Schwalbe, Gearing, MacKenzie, Brewer, & Ibrahim, 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, as well as 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, 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. 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[4] who entered the juvenile justice system between June, 2010 and December 31, 2013. In Figure 1, we use a flow

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

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 probation officers who handled diversion[5] 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.

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 entering the treatment group 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. 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' education. The

[5] Probation officers decide to handle the diversion if there are 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.

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 11-18 from the county of the intervention. This data was collected from the 2008-2012 American Community Survey.[6] 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.[7] 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,

[6] This data was downloaded from usa.ipums.org (Ruggles et al., 2010).
[7] 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.[8] In our regression models, we produce a categorical variable for both measures and include as a group whether the variable is not reported.[9]

b. Diversion Program

Treatment group members are given a 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 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. There has been a recent revival in the use of stories to foster moral development (Bettlheim, 1976; Coles, 1989; Vitz, 1990; Bruner, 2003, 2008; McGavock, 2007), specifically that of a virtuous nature (MacIntyre, 1984; Nussbaum, 1990; Carr, 1991; Hoff-Summers, 1993; Cain, 2005).

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[8] 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.

[9] 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.

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

[10] 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.

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, 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 (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. [11] 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, Henry, Schoeny, Lovegrovw, & Nichols, 2014).[12] This is consistent with the prevailing view that mentoring programs are most beneficial for at-risk participants (DuBois, Holloway, Valentine, & Cooper, 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 (Tolan et al., 2014; Dubois et al., 2002). Consistent with Sampson and Laub's

[11] 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.

[12] 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, Grossman, Kaugh, Feldman, & McMacken, 2007) and the Buddy system (O'Donnell, Lydgate, & Fo, 1979).

(2003; Laub & Sampson, 1993) 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 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 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 from the county on all 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.[13]

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.

[13] 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 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 1.

The data at the JJC only includes arrests before age 18. Some of the participants age into adulthood a few years after completing their respective programs, so this data will not accurately measure re-arrests for this older group. In our state, all adult court records are public; therefore, we downloaded all offenses for study participants who turned 18 sometime during our follow-up.[14] 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 where no such arrest would appear in the adult data.[15]

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,

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

2013 would be dropped from the sample. Therefore, the second sample includes 356 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 262 participants tracked for two full years after program 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

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

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

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

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 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.

b. Measuring the Impact on the Counts of Arrests

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 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 2b 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 with parameter values 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

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

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.[16]

In the first row of Table 3, 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 when 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 an 11.9 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 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

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

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 conditions, 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.[17]

b. *Counts of Arrests*

In Table 4 we report the maximum likelihood estimates for the negative binomial regressions.[18] 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

[17] 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].

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

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 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 impact 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 who 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 an 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 seem to be larger for those groups at higher risk for recidivism. 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 66 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 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 $14,975, making the overall cost to society for these 66 crimes a total of $988,350. If we assume that offenses are reduced by the same amount across all categories, then total costs would fall by 50.9 percent, saving society $503,070, about four 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. 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 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

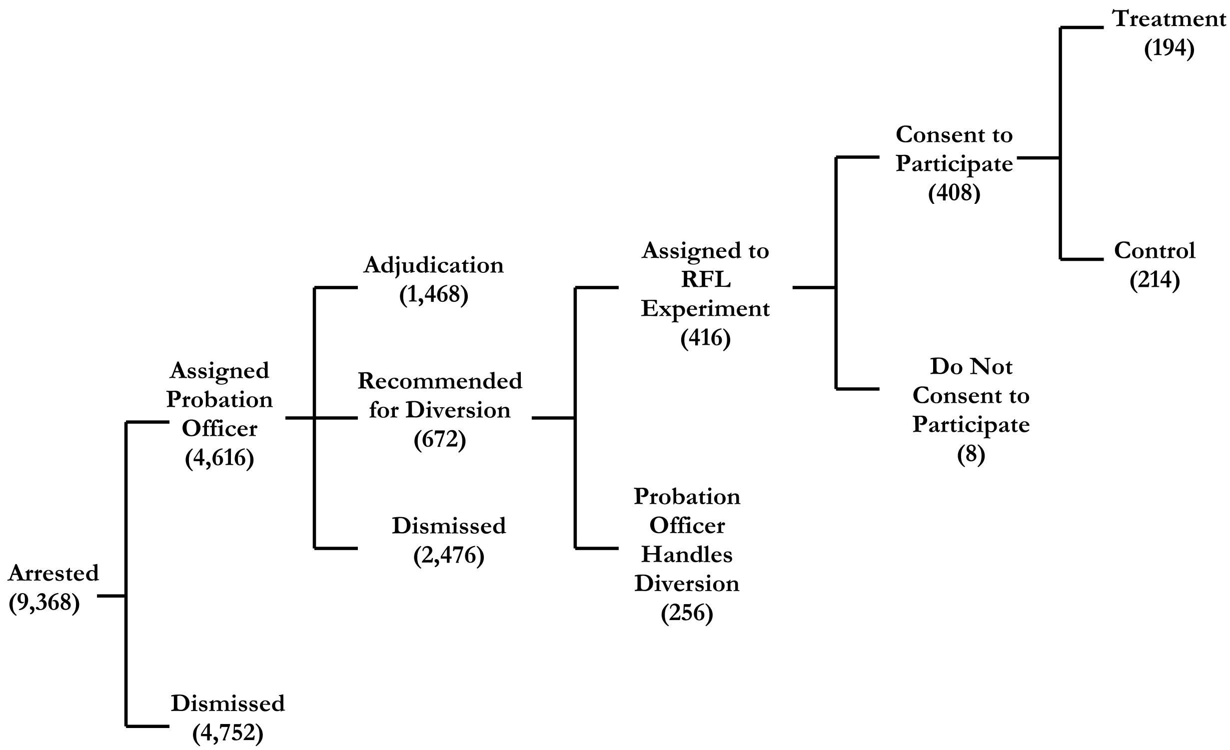
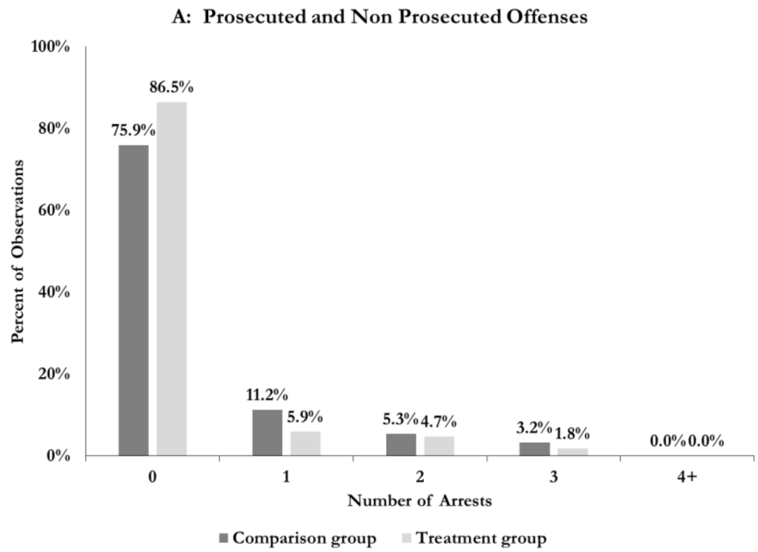


Figure 2
Histogram of Arrest Counts within the First Year



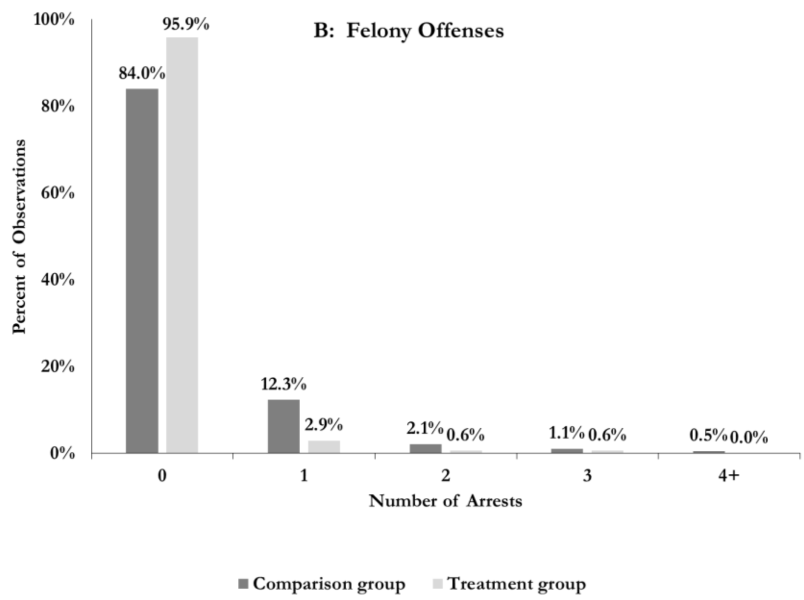


Table 1
Age of Participants by Year and by Program

	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		P-value on test that means are the same across samples
		Treatment group *Reading for Life*	Control Group	
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 (0.046) {0.024}	-0.098 (0.046) {0.034}	0.241	-0.105 (0.041) {0.011}	-0.112 (0.039) {0.004}	0.367	-0.125 (0.057) {0.028}	-0.127 (0.058) {0.031}
Misdemeanor offenses	0.248	-0.057 (0.041) {0.167}	-0.051 (0.041) {0.208}	0.123	-0.035 (0.033) {0.289}	-0.044 (0.030) {0.151}	0.230	-0.077 (0.049) {0.116}	-0.086 (0.048) {0.077}
Felony offenses	0.220	-0.106 (0.037) {0.004}	-0.095 (0.034) {0.006}	0.160	-0.119 (0.032) {0.000}	-0.113 (0.032) {0.000}	0.237	-0.108 (0.048) {0.024}	-0.097 (0.044) {0.030}
Prosecuted Offenses									
All offenses	0.308	-0.118 (0.043) {0.006}	-0.106 (0.040) {0.009}	0.193	-0.122 (0.036) {0.001}	-0.123 (0.033) {0.000}	0.295	-0.182 (0.049) {0.000}	-0.178 (0.047) {0.000}
Misdemeanor offenses	0.192	-0.068 (0.036) {0.062}	-0.054 (0.037) {0.145}	0.080	-0.051 (0.024) {0.037}	-0.051 (0.025) {0.047}	0.158	-0.134 (0.035) {0.000}	-0.127 (0.037) {0.001}
Felony offenses	0.187	-0.110 (0.034) {0.001}	-0.103 (0.030) {0.001}	0.134	-0.116 (0.028) {0.000}	-0.108 (0.028) {0.000}	0.194	-0.122 (0.042) {0.004}	-0.119 (0.039) {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
Estimating the Impact of Treatment on Arrest Counts with Negative Binomial Models
Estimated impact (Standard error) {P-value on null that impact is zero}

	Offense at any time after enrollment (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 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]

Group	Obs.	Prosecuted and non-prosecuted offenses			Prosecuted offenses		
		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.