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

A. D. Seroczynski
(Center for Children and Families, University of Notre Dame)

William N Evans
(Wilson Sheehan Lab for Economic Opportunities, University of Notre Dame and NBER)

Amy D. Jobst
(Thomas N. Frederick Juvenile Justice Center)

Luke Horvath
(Wilson Sheehan Lab for Economic Opportunities, University of Notre Dame)

Giuliana Carozza
(Wilson Sheehan Lab for Economic Opportunities, University of Notre Dame)

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Abstract

We present results of an evaluation of Reading for Life (RFL), a diversion program for non-violent, first-time 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 the chance of future serious offenses and reducing recidivism for those groups with the highest propensity for future offenses.

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Corresponding author: A. D. Seroczynski, William J. Shaw Center for Children and Families; University of Notre Dame; 1602 N. Ironwood; South Bend, Indiana 46635; seroczynski.1@nd.edu.

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We present results of an evaluation of Reading for Life (RFL), a diversion program for non-violent, first-time 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 the chance of future serious offenses and reducing recidivism for those groups with the highest propensity for future offenses.

I. Introduction

Although juvenile crime rates have fallen considerably over the past decade and a half,[1] 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 perpetrators.[2] By locking up 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.[3] 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. Controlling for demographic and individual characteristics, arrested and incarcerated youth are significantly less likely to graduate from high school than non-delinquents, and those who do graduate have much lower four-year college enrollment rates (Kirk and Sampson, 2013). 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).

Juvenile delinquency is also a strong predictor of criminal activity as an adult (McCord and Esminger, 1997; Nagin and Paternoster, 2000). Research on criminal activity over the life course falls broadly into two areas. The first suggests that criminal disposition,

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[1] http://johnjayresearch.org/rec/files/2013/03/databit2013_01.pdf
[2] http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2012/crime-in-the-u.s.-2012
[3] http://aspe.hhs.gov/hsp/08/boys/factsheets/jd/report.pdf

or criminal propensity, is an intrinsic character trait (or collection of traits) that develops early in life and varies across individuals (Nagin and Paternoster, 2000). The theory focuses on identifying different types of criminal trajectories (such as persisting in criminal activity, desisting from crime after early brushes with the law, or avoiding criminal activity altogether) and what factors predict in which group youths fall.[4] Within this paradigm, the impact of a crime committed today has no causal impact on the propensity to commit criminal acts in the future, but instead, simply indicates selection into the lifestyle.

Another strand of literature stresses that future actions are influenced by the accruing, dynamic impact of past ones. Thus, the observed correlation between past and future crime results not from heterogeneity, but from adolescent behavior that increases the likelihood of future criminal activity, perhaps by increasing incentives to commit crimes, damaging social ties, or lowering inhibitions that would otherwise deter a youth to commit crimes (Nagin and Paternoster, 2000). This hypothesis is consistent with theories such as labeling which assert that delinquency can alter one's life course by negatively impacting the offender's self-image, provoking society to treat the individual with apprehension, disdain, or a lack of trust (Becker, 1963; Link et al., 1989; Matsueda, 1992), or creating a sense of fatalism which may encourage criminal activity (Piquero, 2014). Within this state-dependent paradigm, crimes committed as a juvenile have a causal impact on crimes later in life.

A more recent theory that combines the two models outlined above notes that initial heterogeneity is predictive of subsequent criminal behavior, but documents that events throughout life also play an important role in determining outcomes (Laub and Sampson, 1993). This work suggests that even for youth with a high probability of continuing criminal

[4] Research has demonstrated that these criminal trajectories are correlated with many observed characteristics such as sex (Côté et al., 2002; D'Unger et al., 2002), contextual differences in school, peer, neighborhood, and cultural influences, (Maldonado-Molina et al., 2009) life circumstances (Laub et al., 1998; Piquero et al., 2002), and parenting style (Hoeve et al. 2008, Wadsworth 2000).

behavior, life events can provoke subsequent criminal desistence. The evolution of social relationships creates opportunities for turning points, or life transitions, which can either reinforce or counteract criminal behavior. While losing a job or moving to a crime-ridden neighborhood may exacerbate or prolong delinquency, positive turning points and social ties in adulthood – particularly labor force attachment, marriage, and military service – can counteract it (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 in the long term (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, such as reducing future involvement with the court system, lowering stigma associated with having a criminal record, increasing system efficiency, and lowering court costs (Pogrebin et al., 1984; Cocozza et al., 2005). 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. 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). Despite years of experience with diversion programs, there is uncertainty in the research, criminal justice, or public safety communities about the most effective practices (Cocozza et al., 2005). 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 research with credible evaluation designs, such as random assignment experiments. In a recent review of 57 experimental and quasi-experimental studies, Schwalbe et al. (2012) found only 14 of which were random assignment and of this set, only five had more than 300 subjects combined in the treatment and comparison samples.

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 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 classical virtue theory, the program was designed to foster moral development in at-risk adolescents through personal mentoring relationships and moral discussion. It endeavors to be a catalyst for transformative and enduring virtuous life changes by engaging, educating, and empowering its participants. For the current study, program participants are non-violent, often first-time offenders aged 11-18 who entered the juvenile justice system during or after 2010. A unique aspect of the program is that students were randomly assigned to either RFL or 25 hours of community service as an alternative diversion program. This randomized controlled trial allows for ease of evaluation and is considered the gold-standard for identifying program impact. Since 2010, a total of 224 offenders were randomly assigned to the RFL treatment and 225 were assigned to the control group. .

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 12.1 percentage point reduction in the probability of having another offense of any type, which is 38 percent reduction over the control group mean and is statistically different from zero at a p-value of 0.008. The program was particularly successful at reducing more serious offenses; prosecuted felonies fell by 68.5 percent over the control group mean (p-value = 0.0002) which is a 12 percentage point drop in the probability of a re-arrest. Moreover, RFL was most effective at reducing more serious offenses for groups most likely to recidivate: males, offenders from low-income households, and minorities.

In the next section, we outline in detail the RFL program, the study protocols and data collection. In Section III, we outline some related literature on juvenile diversion programs and 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, and in Section VI we make some concluding remarks and suggestions for future research.

II. An Outline of the Reading for Life Diversion Program

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 aged 11-18 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).

At pretest, the 3-Minute Reading Assessment (Rasinski and Padak, 2005) is given to determine group placement. Groups consist of no more than five participants of comparable reading ability and two trained mentors, who 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 classical virtues from Aristotle and Thomas Aquinas' virtue theory: justice, prudence, temperance, fortitude, fidelity, hope, and charity. The objective is to facilitate moral development through immersion in the narrative of the literature, the vicarious experiences of the characters, and contextual relationships provided within the story. The journaling exercises frequently focus on reflections on the discussion content's relevance to each participant's personal life. 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. 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. Not all participants have chosen to do so.

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. 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 wrong. Hoff-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), specifially 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).

Reading for Life also relies on volunteers as mentors. This reduces program costs and makes the program more fiscally attractive to other jurisdictions. More importantly, qualitative evidence from program participants suggests that some have reacted positively to the fact that the mentors volunteered instead of being paid. As we note in the next section, although there are a few diversion programs that show success, programs utilizing mentors have shown more promise than most alternative models.

In Table 1, we report the ages of those enrolled in the treatment and control groups from 2010 through 2014. There is rough equivalence in the sizes of the two groups and in the age distribution across the two groups. The peak age for enrollees is 15-16 with 199 in total, while there are only 30 adolescents who entered the program at aged 11-12. In general, the program has been getting larger over time and the small numbers enrolled for 2014 reflect that we only have enrollment data for the first few months of that year.

The RFL program has a detailed intake assessment protocol; discussed here are only the measures used for this paper's analysis. 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' education. Lastly, 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.

[5] This data was downloaded from usa.ipums.org (Ruggles et al., 2010).

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 22 and 33 percent of the time, respectively. When reported, average family income for those in the program is about 10 percent lower than the amount for families with children aged 11-18: $38,399 as compared to $44,989. Likewise, maternal education in the study population appears well below the average education for mothers with children 11-18.

Pooling the treatment and control samples, the average chance a participant came from a family with both biological parents is 28.6 percent if income is reported, but 15.7 percent if it is not. Likewise, among all participants, the fraction who lived with both natural parents is 32.5 for those who report maternal education, but only 12.9 percent for those who don't. For subsequent analyses, we produce a categorical variable for both measures and include as a group whether the variable is not reported. 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.

[6] Nationwide in 2011, among youths arrested, males 82 percent of violent offenses were male and only 18 percent were female (Office of Juvenile Justice and Delinquency Prevention, 2013).

III. 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.[7] 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 tenents, programs differ substantially

[7] https://www.ncjrs.gov/html/ojjdp/9909-3/div.html

from one another, and few national standards have been established. Diversion programs have taken the form of boot camps, community service projects, individual, group, and family counseling, case management services, and structured in-home family interventions (Cocozza et al., 2005). Programs differ not only in the services they offer, but also in a number of other ways. The point of contact could be with the police, with probation officers, or in court; sometimes the offender is fully adjudicated and sentenced, other times charges may be held in abeyance or expunged; the target population ranges from "Persons in Need of Supervision" and status offenders to felons (Cocozza et al., 2005).

Juvenile diversion programs are widespread; in 2011, about 46% of all youth offenders referred to the juvenile justice system underwent some type of informal adjustment.[8] 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

[8] From "Easy Access to Juvenile Court Statistics": http://ojjdp.gov/ojstatbb/ezajcs/asp/selection.asp

both juveniles and adults; nonetheless, several literature reviews that focused exclusively on juvenile diversion treatments arrived at a similar conclusion. 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).

One reason for the ambiguity in results is that only a small fraction of studies have taken advantage of experimental designs and, 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 attributed 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.[9] 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; Landenberger and Lipsey, 2005; Pearson et al., 2002; Wilson et al., 2005), 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

[9] http://www.criminaljustice.ny.gov/osp/downloads/bestpracticestables.pdf

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 which emphasize emotional development and those which 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.

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 an RCT, providing the greatest possibility for internal validity. Second, as we outlined above, 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, as we outline in Section IV below, 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.

IV. Results

a. Impact on Recidivism

As noted above, the primary outcome in most studies of juvenile diversion programs is whether the adolescent recidivates. We will use as outcomes of interest variants of this measure as well as counts of arrests. All data was obtained from county records at the Juvenile Justice Center. 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. We construct six different indicators of recidivism. First, we measure whether participants were arrested for any offense—prosecuted or non-prosecuted—and isolate felony and misdemeanor events as subsets of all offenses. In a similar fashion, we measure whether the participant was arrested for an eventually prosecuted offense, considering prosecuted misdemeanors and felonies as subsets of prosecuted offenses.[10]

For each of these six measures, we examined program impact in three different samples. In practice, one would likely follow all participants over a fixed window of time and examine whether RFL reduced one or two-year recidivism rates. Unfortunately, because participants are still being enrolled in the sample and we pulled the offense data in the spring of 2014, increasing the years of follow-up reduces the sample sizes. Therefore, to maximize the number of observations in the analysis, we also consider a sample that includes all

[10] Prosecuted offenses are not a proper subset of all offenses. In our data, first offenses can be either prosecuted or non-prosecuted. Prosecuted offenses can either be the first offense, or a subsequent offense that was prosecuted. Therefore, a participant could have had both a non-prosecuted offense and a subsequent prosecuted one.

participants for whom we have data and examine whether they have committed an offense at any time after program completion. The larger sample sizes must be weighed against the fact that we are not holding all else equal for program participants. Some participants have four years of post-program follow-up while some have only been followed for a few months.

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, 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.

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 (n=412).[11] In the second column, we examine outcomes for all people we can follow for at least one year (n=355) 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, then the simple difference in means and the OLS-adjusted difference in means. For these estimates, we report the parameter estimate, the standard error in parentheses, and in curly brackets, the p-value on the test of the null hypothesis that the coefficient equals zero. As expected, the addition of covariates did not significantly alter the estimated impacts. There is also little precision gain by OLS adjusting the estimates. 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.[12]

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 12.1 percentage point reduction in this probability (p= 0.008) which is a 38 percent reduction in control group mean of 0.319. We

[11] In Table 1 we report that there 449 people assigned to either treatment or control but at the time when we pulled arrest data, only 412 had completed the program.
[12] In this instance, only a few variables have the same sign and are statistically significant across dependent variables. These variables are male, age, race and income. The results suggest that low income, younger, black males are more likely to recidivate. We should note that since only those who have an offense make it into the sample, demographic should be less informative about this outcome given the sample selection.

find smaller incidence rates in the comparison sample when we follow participants for one year (0.220), and treatment is estimated to reduce offenses by 10.3 percentage points (p=0.01) which is 47 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 9.5 percent reduction (p=0.086). In the full sample, there is suggestive but imprecise evidence that the program reduces misdemeanor arrests (p=0.495). In the one year sample we find an 11.5 percent point reduction in felony offenses (p=0.003) which is a 72 percent reduction over the sample mean in the comparison group.

The results in the top half of the table are suggestive 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 whole sample, the chance of being arrested for a prosecuted offense falls by 12.9 percentage points (p= 0.007), which is a 53 percent reduction over the control group mean. The effect is most heavily concentrated in felony prosecutions. The reduction for this outcome is 12 percentage points (p=0.0002) and represents a 69 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 (-0.111) represent an 86 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 12.4 percentage point reduction (p=0.003) which is a 66 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 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.

b. *Impact on the Number of Arrests*

An alternative outcome to whether a participant was re-arrested is to examine whether the program altered counts of arrests. As the numbers in Table 3 indicate, the vast majority of participants in both the treatment and control groups are not re-arrested but there are some with high numbers of re-arrests during the follow-up periods. In Figure 1, we report counts of arrests within the first year for those in the treatment and comparison samples. In Figure 1a we report these counts for all offenses (prosecuted and non-prosecuted felonies, misdemeanors and status offenses); in Figure 1b 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, two and four plus arrests. These differences are much starker in Figure 1b. In the no-arrest column we see the 11.2 percentage point reduction from Table 3. Comparing treatment to control sample, we also see dramatically smaller counts of one (5.6 versus 13.9 percent), two (3.0 versus 4.6 percent) and three arrests (1.9 versus 0.5 percent).

We want to generate an omnibus measure of the reduction in arrest counts associated with program participation within a statistical framework. The low counts and high fraction of zero re-arrests means that OLS models may not provide an accurate way to estimate the impact of Reading for Life 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 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 are 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. The approximation to percentage changes is only accurate for small values of δ and as was demonstrated in Table 3, the impact of the program is rather large so for all models, we will report the percentage change in expected counts as the more accurate value $e^{\delta} - 1$. In this case, we calculate the standard error on this percent using the "delta" method.

In Table 4 we report the maximum likelihood estimates for the negative binomial regressions.[13] 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

[13] 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.

dummy variable, while the third is the percent reduction in arrest counts (and it's 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 50 percent in arrest counts for felony offenses, but a statistically insignificant coefficient for misdemeanor offenses of around 8.4 percent. Looking at the most serious offenses—prosecuted felonies—we see that after one year, RFL participants experienced an 85.4% reduction in these counts and a 68.7 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 if $\hat{\delta}$ 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 percentage points [p=0.027] which is 77 percent of the control group mean. In contrast, the results for all offenses show a 4.3 percentage point reduction [p=0.145].

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. For the prosecuted felony offenses, the baseline recidivism rate is much higher for the lower income/income not reported group (15.1 versus 9 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 samples, whites have about a 12 percentage point lower recidivism rate compared to non-whites. For both groups we find large reductions in prosecuted felonies after one year with a 10.1 (p=0.006) and 12.2 percentage point (p=0.006) reduction for whites and non-whites, respectively. Among non-whites, for all offenses, RFL reduces recidivism rates by 17.3 percentage points, or 63 percent of the control group mean (p = .004). These same numbers for whites are a 4.3 percentage point reduction which is not statistically significant and is only 27 percent of the sample mean.

V. Conclusion

The evidence above suggests 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, no-whites, participants from lower income families). The effects are also large, participation in RFL reduces re-arrests for prosecuted felony offenses by 11.1 percent after one year and 12.4 percent after two years. These numbers are 86 and 66 percent of the sample mean recidivism rates for the control group.

A 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 65 percent. Within the control group, there were 54 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 and these costs range 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 44 crimes using the numbers in this paper and using an estimate of $500/crime for the more minor offenses, the societal costs for these 44 crimes total $827,836. If we assume that offenses are reduced the same amount across all categories,[14] then total costs would fall by 65 percent, saving society $538,093, which is more than 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 US 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

[14] This is of course a strong assumption. However, it most likely understates the decline in more serious offenses. As we note in Tables 3 and 4, RFL did a much better job reducing the incidence of more serious felony offenses compared to misdemeanor and status offenses.

literature to highlight these virtues, and the use of trained volunteer mentors, etc. Although this is a large RCT compared to others in the juvenile diversion nexus, it is was 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. Key future goals include demonstrating that the program can be replicated and isolating the causal pathways that lead to the program's success.

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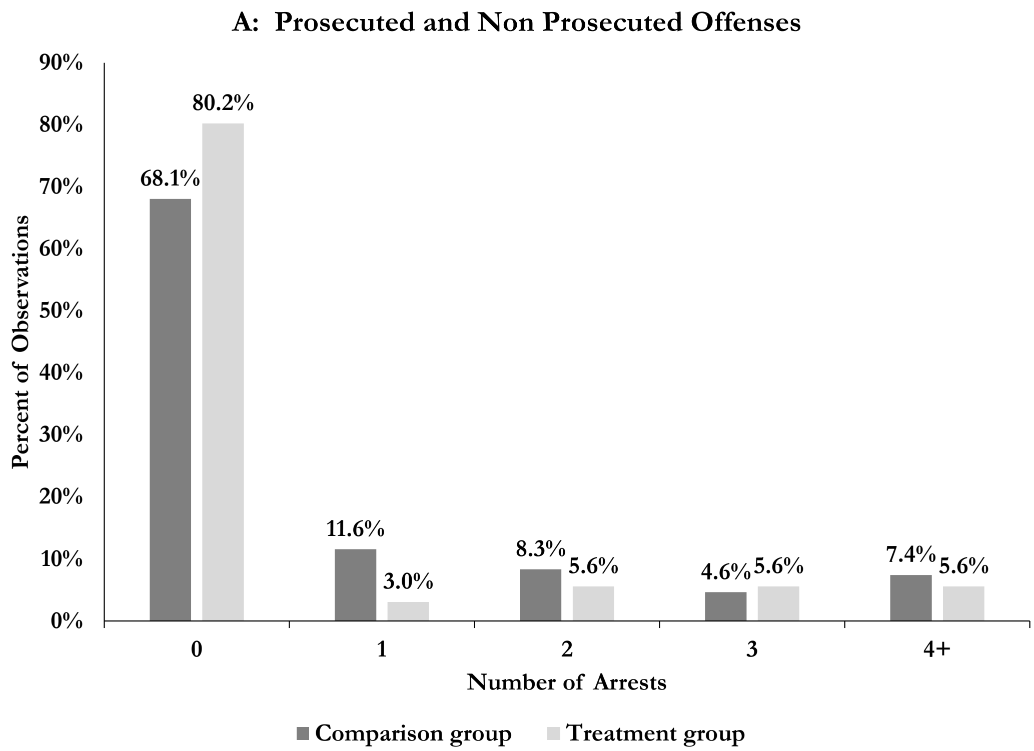
Figure 1
Histogram of Arrest Counts within the First Year



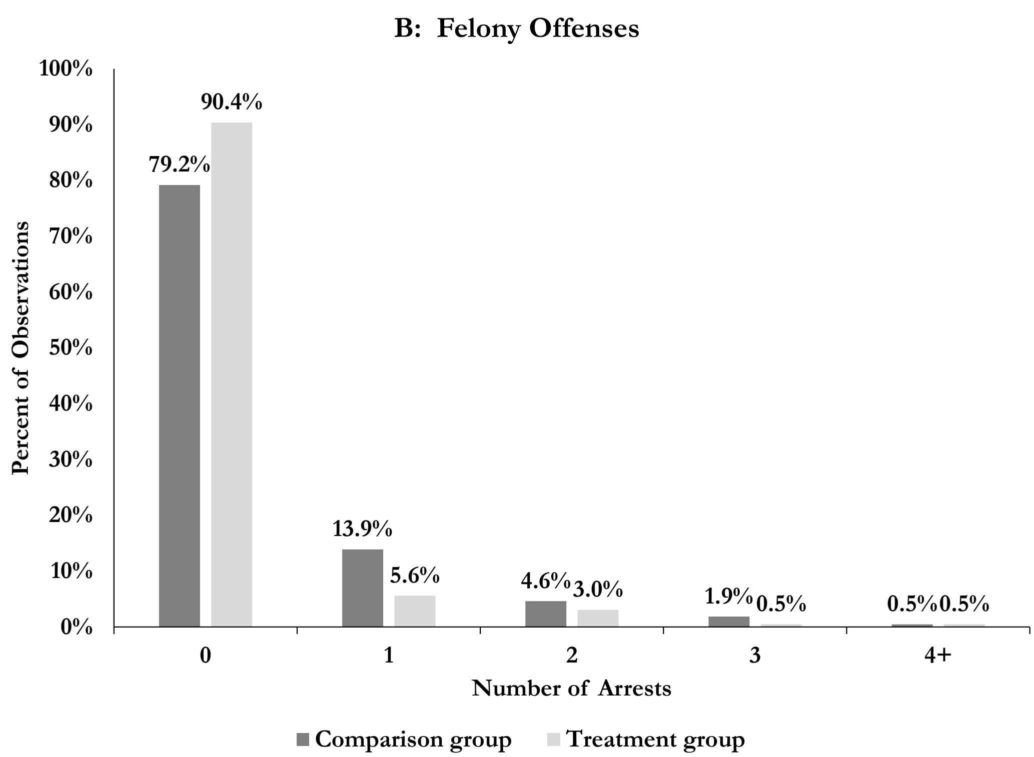


Table 1
Age of Participants by Year and by Program

	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	15	29	18	66
2013	3	10	29	18	60
2014	1	4	14	10	29
Total	15	50	98	61	224
Community Service Control					
2010	8	11	24	8	51
2011	3	13	20	11	47
2012	4	8	22	17	51
2013	0	13	28	22	63
2014	0	3	7	3	13
Total	15	48	101	61	225
Total	30	98	199	122	449

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*	Control group	P-value on test that means are the same across samples
Completed Program		0.888	0.888	0.999
Race/Ethnicity dummy variables				
White, non-Hispanic	0.664	0.473	0.431	0.371
Black, non-Hispanic	0.173	0.299	0.324	0.563
Asian, non-Hispanic	0.012	0.009	0.022	0.257
Hispanic	0.097	0.129	0.111	0.551
Multiracial/Ethnic	0.080	0.085	0.098	0.635
Other or unknown	0.017	0.004	0.013	0.318
Age upon entry	14.712	15.334	15.316	0.901
Male dummy variable	0.518	0.406	0.427	0.662
Household type dummy variables				
Both biological parents	0.567	0.246	0.276	0.470
Single parent	0.295	0.415	0.422	0.880
1 biological parent and partner	**	0.241	0.218	0.558
Other relatives	0.035	0.094	0.062	0.214
Adopted or foster parents	0.014	0.022	0.031	0.565
Family Income				
Median if Reported	44,989	38,399	39,456	0.744
Income not reported	0.075	0.192	0.204	0.741
Mother's education dummy variables				
Less than high school	0.097	0.125	0.133	0.793
High school diploma or GED	0.300	0.210	0.240	0.445
Some college education	0.238	0.169	0.156	0.687
College degree or higher	0.365	0.183	0.129	0.114
Mother's education nor reported	N/A	0.313	0.342	0.503
Sample size	29,895	224	225	

** 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}

Serious of offense	Offense any time after enrollment (n=412) Mean of outcome in control group	Diff. in means	OLS adjusted	Offense in first year (n=355) Mean of outcome in control group	Diff. in means	OLS adjusted	Offense in first two years (n=262) Mean of outcome in control group	Diff. in means	OLS adjusted
				Prosecuted and non-Prosecuted offenses					
All offenses	0.319	-0.121	-0.100	0.220	-0.103	-0.106	0.319	-0.095	-0.089
		(0.043)	(0.041)		(0.040)	(0.040)		(0.055)	(0.055)
		{0.008}	{0.017}		{0.010}	{0.008}		{0.086}	{0.108}
Misdemeanor offenses	0.162	-0.035	-0.023	0.102	-0.032	-0.038	0.167	-0.022	-0.025
		(0.035)	(0.034)		(0.030)	(0.030)		(0.050)	(0.048)
		{0.313}	{0.495}		{0.293}	{0.202}		{0.614}	{0.584}
Felony offenses	0.208	-0.112	-0.103	0.160	-0.115	-0.108	0.231	-0.112	-0.100
		(0.035)	(0.035)		(0.031)	(0.032)		(0.050)	(0.047)
		{0.002}	{0.003}		{0.003}	{0.008}		{0.017}	{0.035}
				Prosecuted offenses					
All offenses	0.245	-0.129	-0.110	0.177	-0.113	-0.110	0.261	-0.157	-0.147
		(0.038)	(0.036)		(0.035)	(0.035)		(0.047)	(0.046)
		{0.007}	{0.003}		{0.001)	{0.002}		{0.001}	{0.002}
Misdemeanor offenses	0.115	-0.055	-0.036	0.065	-0.041	-0.039	0.109	-0.085	-0.074
		(0.028)	(0.028)		(0.022)	(0.022)		(0.031)	(0.031)
		{0.052}	{0.194}		{0.062}	{0.084}		{0.006}	{0.017}
Felony offenses	0.175	-0.120	-0.117	0.129	-0.111	-0.103	0.188	-0.124	-0.121
		(0.031)	(0.030)		(0.028)	(0.028)		(0.041)	(0.041)
		{0.0002}	{0.0001}		{0.0006}	{0.0003}		{0.003}	{0.004}

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}

Serious of offense	Offense any time after enrollment (n=412) Mean Count of outcome in control group	Regression Adjusted	Percentage Change in Counts of Arrests	Offense in first year (n=355) Mean Count of outcome in control group	Regression Adjusted	Percentage Change in Counts of Arrests	Offense in first two years (n=262) Mean Count of outcome in control group	Regression Adjusted	Percentage Change in Counts of Arrests
				Prosecuted and non-Prosecuted offenses					
All offenses	0.792	-0.353 (0.199) {0.076}	-0.297 (0.140)	0.483	-0.738 (0.277) {0.008}	-0.522 (0.132)	0.812	-0.341 (0.235) {0.146}	-0.289 (0.167)
Misdemeanor offenses	0.241	-0.088 (0.263) {0.737}	-0.084 (0.241)	0.134	-0.382 (0.388) {0.325}	-0.318 (0.265)	0.239	-0.070 (0.304) {0.819}	-0.067 (0.283)
Felony offenses	0.307	-0.692 (0.273) {0.011}	-0.500 (0.137)	0.215	-1.289 (0.433) {0.003}	-0.724 (0.119)	0.326	-0.624 (0.317) {0.035}	-0.464 (0.170)
				Prosecuted offenses					
All offenses	0. 463	-0.649 (0.252) {0.010}	-0.477 (0.131)	0.269	-1.048 (0.357) {0.003}	-0.650 (0.125)	0.449	-0.874 (0.325) {0.007}	-0.582 (0.135)
Misdemeanor offenses	0.134	-0.330 (0.363) {0.364}	-0.281 (0.261)	0.069	-0.867 (0.620) {0.162}	-0.580 (0.261)	0.123	-1.12 (0.658) {0.090}	-0.672 (0.216)
Felony offenses	0.226	-1.06 (0.346) {0.002}	-0.654 (0.120)	0.156	-1.92 (0.629) {0.0003}	-0 854 (0.091)	0.224	-1.16 (0.430) {0.007}	-0.687 (0.135)

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	-0.012	-0.226	-0.185	-0.020	-0.171
		(0.073)	(0.062)	(0.060)	(0.064)	(0.043)	(0.06)
		{0.007}	{0.848}	{0.0003}	{0.004}	{0.647}	{0.003}
		[0.329]	[0.158]	[0.237]	[0.250]	[0.079]	[0.184]
Females	214	-0.043	-0.053	-0.040	-0.063	-0.054	-0.070
		(0.047)	(0.030)	(0.038)	(0.041)	(0.025)	(0.031)
		{0.364}	{0.076}	{0.288}	{0.130}	{0.030}	{0.027}
		[0.145]	[0.064]	[0.100]	[0.127]	[0.054]	[0.091]
By age							
<16	172	-0.106	0.012	-0.155	-0.084	0.014	-0.120
		(0.060)	(0.049)	(0.048)	(0.054)	(0.035)	(0.045)
		{0.080}	{0.796}	{0.002}	{0.125}	{0.728}	{0.008}
		[0.220]	[0.099]	[0.176]	[0.176]	[0.074]	[0.143]
≥16	183	-0.123	-0.074	-0.095	-0.150	-0.077	-0.120
		(0.056)	(0.038)	(0.044)	(0.047)	(0.028)	(0.036)
		{0.030}	{0.052}	{0.034}	{0.002}	{0.007}	{0.001}
		[0.221]	[0.105]	[0.137]	[0.179]	[0.074]	[0.116]

Table 5 (continued)

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	-0.126	-0.041	-0.114	-0.149	-0.035	-0.117
		(0.053)	(0.044)	(0.042)	(0.048)	(0.032)	(0.039)
		{0.019}	{0.354}	{0.007}	{0.002}	{0.283}	{0.003}
		[0.261]	[0.143]	[0.168]	[0.218]	[0.084]	[0.151]
Income median or above	125	-0.072	-0.024	-0.109	-0.058	-0.040	-0.096
		(0.062)	(0.029)	(0.053)	(0.049)	(0.023)	(0.038)
		{0.246}	{0.413}	{0.042}	{0.239}	{0.084}	{0.012}
		[0.149]	[0.030]	[0.134]	[0.104]	[0.030]	[0.090]
By race							
White, non-Hispanic	164	0.043	0.016	-0.086	-0.056	-0.016	-0.101
		(0.056)	(0.033)	(0.047)	(0.050)	(0.025)	(0.036)
		{0.440}	{0.634}	{0.072}	{0.259}	{0.528}	{0.006}
		[0.157]	[0.036]	[0.145]	[0.133]	[0.036]	[0.108]
Non-white	191	-0.173	-0.089	-0.134	-0.173	-0.056	-0.122
		(0.061)	(0.050)	(0.047)	(0.052)	(0.037)	(0.044)
		{0.004}	{0.077}	{0.004}	{0.001}	{0.132}	{0.006}
		[0.272]	[0.155]	[0.165]	[0.214]	[0.087]	[0.146]

Appendix Table A1
OLS Estimates of Recidivism Equations, Offenses Any Time after Enrollment

Parameter estimates and (Standard errors)

	Prosecuted and non-prosecuted offenses			Prosecuted offenses		
Covariate	All offenses	Misd. Offenses	Felony offenses	All offenses	Misd. offenses	Felony Offenses
Reading for Life dummy	-0.099	-0.023	-0.103	-0.110	-0.036	-0.117
	(0.041)	(0.034)	(0.035)	(0.036)	(0.028)	(0.031)
Black, non-Hispanic	0.064	0.027	-0.022	0.032	0.005	0.008
	(0.050)	(0.041)	(0.042)	(0.044)	(0.033)	(0.037)
Hispanic	0.037	0.043	-0.075	-0.014	-0.007	-0.037
	(0.072)	(0.059)	(0.061)	(0.063)	(0.048)	(0.054)
Male	0.133	0.089	0.143	0.117	0.072	0.115
	(0.043)	(0.035)	(0.036)	(0.038)	(0.029)	(0.032)
Single parent	0.031	0.020	0.008	-0.010	-0.029	0.062
	(0.055)	(0.046)	(0.047)	(0.049)	(0.037)	(0.042)
1 biological parent partner	0.031	-0.025	0.008	-0.028	-0.052	0.042
	(0.057)	(0.047)	(0.048)	(0.050)	(0.038)	(0.043)
Other relatives	-0.074	-0.111	-0.022	-0.103	-0.129	0.013
	(0.093)	(0.077)	(0.079)	(0.082)	(0.063)	(0.055)
Adopted or foster parents	-0.007	-0.031	-0.131	0.036	0.004	-0.073
	(0.136)	(0.112)	(0.115)	(0.120)	(0.091)	(0.102)
First quartile income	0.191	0.154	0.148	0.246	0.151	0.137
	(0.074)	(0.061)	(0.062)	(0.065)	(0.050)	(0.055)
Second quartile income	0.072	0.103	0.021	0.067	0.061	0.035
	(0.071)	(0.058)	(0.060)	(0.062)	(0.047)	(0.053)
Third quartile income	0.088	0.071	0.054	0.092	0.084	0.028
	(0.014)	(0.058)	(0.059)	(0.062)	(0.052)	(0.053)
Income not reported	0.014	0.091	0.005	0.041	0.083	-0.002
	(0.078)	(0.064)	(0.066)	(0.069)	(0.052)	(0.058)
Mom < high school	0.094	-0.003	-0.033	0.031	0.012	-0.064
	(0.084)	(0.070)	(0.071)	(0.075)	(0.056)	(0.064)
Mom HS diploma/GED	0.121	0.087	0.051	0.084	0.079	0.016
	(0.073)	(0.060)	(0.061)	(0.064)	(0.049)	(0.055)
Mom some college	0.080	0.047	-0.059	-0.033	-0.040	-0.040
	(0.077)	(0.063)	(0.065)	(0.068)	(0.051)	(0.058)
Mom's educ. not reported	0.025	0.046	-0.022	-0.003	0.055	-0.030
	(0.071)	(0.059)	(0.060)	(0.063)	(0.048)	(0.054)
Age 11	0.110	0.052	0.208	0.133	0.066	0.190
	(0.185)	(0.152)	(0.157)	(0.164)	(0.124)	(0.139)
Age 12	0.265	0.121	0.142	0.193	0.050	0.129
	(0.132)	(0.109)	(0.111)	(0.116)	(0.088)	(0.099)
Age 13	0.348	0.238	0.142	0.220	0.076	0.097
	(0.114)	(0.094)	(0.096)	(0.100)	(0.076)	(0.086)
Age 14	0.121	0.088	0.050	0.065	0.050	-0.040
	(0.111)	(0.091)	(0.094)	(0.098)	(0.088)	(0.083)
Age 15	0.146	0.084	0.092	0.058	0.047	0.031
	(0.105)	(0.086)	(0.089)	(0.092)	(0.070)	(0.079)
Age 16	0.103	0.039	0.083	0.061	0.014	0.039
	(0.103)	(0.085)	(0.087)	(0.091)	(0.069)	(0.077)
Age 17	0.008	0.002	-0.039	-0.026	-0.029	-0.057
	(0.103)	(0.085)	(0.087)	(0.091)	(0.69)	(0.077)
R^2	0.1642	0.1210	0.1341	0.1677	0.1085	0.1460