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CAUSAL EFFECTS OF MENTAL HEALTH TREATMENT ON EDUCATION OUTCOMES FOR YOUTH IN THE JUSTICE SYSTEM

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Causal Effects of Mental Health Treatment on Education Outcomes for Youth in the Justice System
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ABSTRACT

This study assesses whether mental health interventions can improve academic outcomes for justice-involved youth. Only a limited number of studies have linked justice policies to outcomes beyond crime, particularly education, which carries large monetary and non-monetary benefits. The current study relies on detailed administrative data and unique policy rules under which youth are assigned to behavioral treatment programs. The administrative data allow for a rich set of controls for observed family- and youth-specific heterogeneity. In addition, the treatment assignment rules create a discontinuity among youth who are deemed eligible or not eligible for treatment, rules which the study exploits empirically to address the non-random selection bias in estimating plausibly causal effects of treatment eligibility and treatment receipt. Estimates indicate that certain types of intensive mental health intervention can lower dropout and increase high-school completion for justice-involved youth. Effects on grades are negative or not significant, possibly due to the greater retention of less academically-skilled students. We also assess heterogeneity in the treatment effects, and find that the effects on dropout tend to be greater among youth believed to be less academically engaged prior to treatment.

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I. Introduction

In 2010 juveniles accounted for 1.6 million non-traffic related arrests, ranging from curfew violations to violent crimes (Sickmund and Puzzanchera, 2014). Youth in the juvenile justice system typically are at least 10 years old and most are age 16 or 17. They have, by definition, exhibited antisocial behaviors that lead them to the attention of the justice system. A variety of adolescent intervention programs have been created which target criminal and antisocial behaviors (Greenwood, 2008; Cuellar 2015). These programs stand in contrast to broad, primary prevention approaches which target young children often before significant problems emerge. In part, the debate surrounding the most effective policies centers on whether adolescence is too late to improve the trajectory of anti-social behaviors or academic achievement (Cook, et al. 2014).

For youth with relatively significant mental health and behavior problems, who may have a lengthy history of serious behavior and family troubles, particular programs have been developed called multi-systemic therapy (MST) and functional family therapy (FFT). These intervention programs are relatively intensive and costly, as much as $7,000 per treated youth. Yet they have the potential to reduce social costs across a variety of dimensions, not only those related to violent and property crimes, but also those associated with substance abuse, early teen births, and low educational outcomes.

Although MST and FFT can reduce criminal recidivism, their high cost has impeded widespread program availability in the justice system, even to youth who would meet the relatively stringent eligibility criteria based on individual mental health and social factors. One possible reason is that the full extent of societal benefits from these programs has not been explored. Broader and durable societal benefits could be achieved if MST and FFT affected outcomes such as high school dropout and completion rates. Such improvements would have long-lasting effects on employment, income and participation in social programs and, thus, modify any cost-benefit calculus of the interventions.

Multi-systemic Therapy (MST), Functional Family Therapy (FFT) and to a lesser extent Aggression Replacement Training (ART) are among the most studied programs for youth in the justice system. With these programs justice agencies seek to reduce recidivism and potentially criminal behavior that could persist into adulthood. Thus, the focus of research on MST and FFT has been largely on criminal outcomes. Indeed, there is some evidence that these programs achieve reductions in recidivism. However, less well studied is their impact on academic success.

This study assesses the short and medium-run academic benefits of mental health treatment among a population of youth in the juvenile justice system, using rich administrative data from Washington State and a unique policy rule under which youth are assigned to these treatment interventions. We have extensive data on each youth allowing us to control for observed differences in family and youth characteristics. In addition, the treatment assignment rules in Washington create a discontinuity among youth who are deemed eligible or not eligible for treatment, rules which we exploit empirically to address the non-random selection bias in estimating plausibly causal effects of treatment eligibility. We are then able to deploy our rich data and compare secondary education outcomes for these high-risk youth. Among education outcomes we are able to observe secondary school completion, transfers, dropouts, suspensions, and average grades, allowing us to assess the impact of mental health treatment across a range of important measures of human capital acquisition.

We find consistent evidence that FFT and MST, the more intensive mental health treatment programs, have broader spillover benefits among high-risk youth. Specifically, both the intent-to-treat and the treatment-on-the-treated effects for both of these programs are significantly in the direction of reducing dropout rates and raising secondary school completion. Assessment of heterogeneity in these treatment effects further confirms that these programs improve educational outcomes even among the higher-risk individuals – those who had weaker school engagement prior to their treatment screening.

The rest of the study is laid out as follows. Section 2 reviews the previous literature regarding mental health treatment for youth in the justice system and experiments to improve their educational outcomes. Section 3 discusses our data and Section 4 outlines our empirical approach in exploiting the natural experiment afforded by WA's discontinuous eligibility guidelines. The results are discussed in Section 5, followed by some policy implications in the concluding section.

2. Background

Measuring the causal link between mental health treatment and education is a challenging task, as a result of which many researchers have resorted to small-scale randomized trials of treatments. We review relevant strands from two literatures, the first on several treatments that have focused on youth in the justice system. In addition, we include studies that have focused on educational outcomes among high-risk adolescents where behavior problems are also prevalent.

Among the most intensive and costly programs for justice-involved youth is Multi-Systemic Therapy. The Blueprints for Violence Prevention Initiative estimates the year-one cost of two MST teams which could serve 132 families at $933,000 or $7,068 per family.[1] MST combines family-based and cognitive-behavioral therapies with a range of individualized support services. MST therapists intervene with the family, peers, schools, and other treatment agencies over a period lasting from 4 to 6 months. Treatment involves weekly home visits and typically includes parent-focused behavior management and communication skills training as well as collaborative links between families and school (Henggeler, et al, 1998). There is some experimental evidence that MST reduces arrests and convictions relative to treatment as usual (Littell, et al, 2005).

Another intervention for youth with behavioral problems, conduct disorder, delinquency and substance abuse is Functional Family Therapy, which although multi-faceted it is not as intensive as MST (Littell, et al 2007). The Blueprints estimate for the year-one cost of FFT is $1,679,000 or $2,080 per youth or family served.[2] Families are trained to modify communication that contributes delinquency and dysfunction, to negotiate more effectively, and to set clear rules and responsibilities. The FFT intervention is highly structured and typically lasts 3 months. It is designed to prevent further delinquency and violence by promoting communication and enhancing support within the family. While longer term goals include lower reduced recidivism, substance abuse and sibling criminal involvement, school outcomes are not explicitly part of the formal intervention model[3] although one might posit that greater problem-solving and communication skills could lead to improvements in at least some education outcomes. While earlier trials of FFT found reductions in reoffending, a more recent trial found

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[1] These estimates were accessed at http://www.blueprintsprograms.com/programCosts.php?pid=cb4e5208b4cd87268b208e49452ed6e89a68e0b8

[2] These estimates were accessed at http://www.blueprintsprograms.com/programCosts.php?pid=0a57cb53ba59c46fc4b692527a38a87c78d84028

[3] A logic model is provided by the Blueprints for Violence Prevention group at http://www.blueprintsprograms.com/factSheet.php?pid=0a57cb53ba59c46fc4b692527a38a87c78d84028

no differences between FFT and the control group with respect to reoffending (Sexton and Turner, 2010).

Separately, a natural experiment-based study of FFT was conducted in Washington State which examined a specific sub-population of justice-involved youth, those who had been incarcerated (Lucenko, et al 2011). A policy change occurred under which youth were released from commitment facilities and received FFT in one year, but in the following year youth who were released no longer received FFT due to budget cuts. Both groups were observed for 12 months after release from incarceration. The study used propensity score matching to compare incarcerated youth before and after the program cut and finds that arrest rates declined, while employment rates increased. The change in arrest and employment is attributed to FFT. However, the study is not able to control for secular trends in youth crime or employment. Potentially, a recession that led to both the JRA programmatic cut and lower availability of jobs could confound the employment results while cuts in police or probation workforces could have led to less crime detection over time and confounded the arrest results. Education outcomes were not measured.

Aggression Replacement Training (ART) has less empirical support than either MST or FFT, but it is also less intensive, relies on a group therapy format, and last only 10 weeks (Washington State Institute for Public Policy, 2004). ART teaches youths how to control their angry impulses and take perspectives other than their own through moral reasoning with the goal of reducing aggressive behavior and violence (Office of Juvenile Justice and Delinquency Prevention). There is no explicit academic component or educational goal, although improved peer and teacher relationships in school could conceivably lead to greater school success. One study among justice-involved youth used a waiting list comparison and found a 16% lower rate of felony recidivism among those treated with ART. A second, small study examined ART in school settings and found that parents and teachers reported improved social skills, including such dimensions as self-control and cooperation, as well as improvements in problem behaviors (Gundersen and Svartdal, 2006).

All three mental health interventions, MST, FFT and ART, use mental health therapy to address youth conduct problems, but it is not clear whether this is sufficient to remediate academic problems specifically. Other interventions for at-risk adolescents attempt to directly target school achievement, for example, through school-based interventions or tutoring. In some cases the intervention combines mental health treatment and academic components. Often the academically focused interventions target adolescents in high-risk schools, not specifically youth in the justice system.

Using an exclusively school-based approach, a study conducted by Fryer (2011) introduced instructional change with a math tutoring component. The study finds significant improvements on math achievement scores from the intervention highlighting the possibility of significant improvements in math skills at relatively late stages, i.e., among 6th and 9th graders. In contrast, the Pathways to Education Program was broader (Oreopoulos, Brown, and Lavecchia, 2014). It was offered in a housing project in Toronto and spanned case management, intensive tutoring, group activities, and financial support for school, college, and transportation expenses. Although it is not possible to unpack the effect of these different components, the Pathways study found improved grades, large increases in high school graduation rates (rising from 38% to 58%) as well as higher college entry.

Another intervention took place with males in 7th through 9th grade in Chicago Public Schools and focused on crime and school outcomes (Heller, et al, 2013). The study randomized over 2,700 youth to school as usual, a group-based mental therapy program called "Becoming A Man (BAM)" offered in school, BAM offered with after-school programming, or after-school programming only. There was no tutoring group in this particular study. The BAM groups performed better on their grade point averages, but not on drop-out by year end or on average days of school attendance.

Finally, Cook and colleagues (2014) conducted a study among 10th and 11th grade inner-city boys, using both group therapy and academic remediation in the form of math tutoring.[4] While the pilot reports no statistically significant differences in disciplinary incidents in school or in days suspended out of school between those who received treatment and those who did not, it does find significant effects on math achievement.

Our study addresses key knowledge gaps and makes contributions along several dimensions. First, our study bridges the two literatures reviewed above, the first studying the effects of mental health interventions on crime outcomes among justice involved-youth and the second studying the effects of academically-focused interventions on educational outcomes among high-risk youth. Virtually all of the prior work on non-academic behavioral interventions, targeted at justice-involved youth, have understandably focused on first-order effects – studying criminal and conduct outcomes, which these interventions were intended to improve. Furthermore, virtually all of the literature on adult and juvenile crime has also understandably focused on the effects of justice policies and interventions on crime.[5] We add to the very sparse literature linking crime-based policies to domains other than crime. In the process, we provide some of the first causal evidence on whether, and the extent to which, these often-endorsed

[5]One recent exception in the juvenile justice literature is Aizer and Doyle (2013), who use random judge assignment in Chicago to study the effects of juvenile incarceration on schooling and future adult crime. They find that juvenile incarceration results in large decreases in the likelihood of completing high school and large increases in the likelihood of being incarcerated as an adult.

behavioral treatment programs, some of which do not have an explicit academically-focused component, nevertheless have broader spillover benefits on human capital development through educational acquisition. Given the vast literature linking education to improvements in health behaviors, morbidity, and mortality, reduction in future crime and delinquency, and other non-monetary benefits, an unbiased cost-benefit calculus of any intervention would need to account for any positive (or negative) effects on educational attainment. Second, the school-based studies above included students attending school and by design excluded youth who had dropped out. In our study, we start from a population of youth in the justice system and examine whether mental health intervention improves their academic achievement using several measures, two of which are dropping out of secondary school and completion since our sample is not selected on current students. Third, the focus on justice-involved youth – an inherently high-risk sample of adolescents – is particularly salient to the debate on primary versus secondary prevention, and our study contributes evidence on whether behavioral interventions can be effective in affecting positive change among older youth who have already started to exhibit delinquent behaviors. Fourth, much of the juvenile justice debate has also centered on the effectiveness of harsher sentencing such as detention and confinement. However, most juvenile offenders are not incarcerated, and states differ in terms of how to deal with these offenders who are released back into the community.[6] Our study thus adds to the sparse literature on the causal effects of alternatives targeted at youth who are not sentenced to confinement, and provides valuable policy guidance on whether behavioral interventions provide any benefits over other less-costly local sanctions comprising probation, small fines, or some community restitution.

3. Data

[6] Among 112,766 offenders entering the juvenile justice system in WA between 2004-2009, about 4,211 unique offenders were confined in a Juvenile Rehabilitation Center at some point over this period (authors' calculations from the administrative data).

Administrative Data from WA

In order to perform detailed analyses of how mental health treatment in the justice system affects education outcomes, data were received from two agencies in Washington State, the state-wide juvenile justice system and the statewide school system. These person-level administrative data were linked using last name, first initial, and date of birth. The justice data include all youth who were screened or assessed for treatment services from January 2004 to April 2009. Our observation is a person and screening date pair. We exclude youth who ever appear in the data as deceased. We also exclude youth who ever report being incarcerated in long-term commitment facilities because these youth would receive education in those facilities and not through the regular education system.

The justice-system screening instrument captures criminal and social risk factors, as well as ratings of aggressive behavior and problems with family and living situation. Together these elements define eligibility for the treatment programs as described below. The screening instrument also captures youths' history of mental health, alcohol and drug abuse problems, academic problems, and prior employment. In addition, the justice data capture whether a youth was deemed eligible for treatment by treatment type (MST, FFT, or ART), whether they were referred for treatment, and reasons why the services may not have been received, such as lack of available openings or refusal.

The school data capture enrolled youth for the 2004/5 through 2008/9 academic years. These monthly data include information on the school district code[7], demographics (age, gender, race/ethnicity), details on participation in school programs (free lunch, special education, reading

[7] There are 295 school districts in Washington.

or math assistance), enrollment status (graduation or transfer), as well as grade point averages for youth in higher grades.

Because the data include detailed monthly school information, we are able to construct multiple outcomes each measured over the 12 months after the youth was screened for treatment eligibility. We constructed four dependent variables: 1) indicator for whether a given youth dropped out of school; 2) an indicator for whether a youth transferred schools, which captures general disruptions in the youth's academic experience; 3) a broad indicator for being absent, which captures whether the youth dropped out of school, was suspended, or just did not show up to school; and 4) grade point average. In order to assess whether any potential benefits realized from not dropping out of school are persistent, we also assess high school or GED completion over the 24 months after the youth was screened for treatment eligibility. After merging the screening and treatment data with the school data, our sample includes 35,020 observations over the 2004-2009 period.

Treatment Allocation Rules

Our study relies on a unique natural experiment afforded by discontinuous treatment allocation rules, which we exploit along with the rich information in the administrative records to identify the causal effects of treatment. In our study context, Washington State, treatment services are delivered to youth in the juvenile justice system based on fixed criteria. Youth are assessed after referral to the justice system, which is analogous to an adult arrest. Based on their assessment scores youth are deemed eligible for MST, FFT, or ART.[8] Although the agency refers designated justice-involved teens for treatment, the treatment itself is delivered in the

[8] Some low-risk youth also are offered a low intensity program called Coordination of Services (COS) which we do not study here. Youth who receive COS services are excluded from our analyses.

community. Even when youth are found to be eligible, not all youth are assigned to treatment as they may refuse to participate or treatment slots may not be available.

Eligibility for each treatment type is determined by scores calculated from the youth assessment instruments in several key areas. The key dimensions on the assessment instrument are broader categorizations of criminal risk and social risk, and the component domains of family living situation, and aggression-related domains. Criminal risk and social risk together capture a youth's recidivism risk level. Criminal risk scores (ranging from 0-31) are calculated from a series of questions related to prior offending, including the age at which the youth first committed an offense, the types of previous offenses, and any prior incarcerations. Social risk scores (ranging from 0-18) are calculated from questions about difficulties in school, history of voluntary or court-ordered placement out-of-home, such as in foster care, relationships with peers, whether a parent is serving time in jail, parent control problems, and history of abuse, neglect, and mental health problems. Criminal and social risk are combined to determine whether a youth is considered low, moderate or high risk, as shown in Figure 1.

One component of social risk, family living arrangements, summarizes questions on whether the child is living with his or her parents, whether the family is supportive of the youth, whether there are sibling problem behaviors, whether the parents exercise appropriate supervision and control over the youth and whether reward and punishment behavior is appropriate and consistent. Deficits in these areas signify family instability and are fundamental to the appropriate allocation of FFT and MST therapies.

Another component domain, aggression, includes specific questions related to aggression, including tolerance for frustration, hostile interpretations of actions and intentions of others, belief in verbal and physical aggression as a legitimate means to resolve conflicts, and reports of past aggression problems. In addition, there are questions about pro-social attitudes and behaviors, such as attitudes about past crimes, optimism, empathy, impulsiveness, and respect for authority figures and others. Finally, there are questions related to skills, including goal setting, consequent thinking, basic social skills and dealing with emotions. Deficits in these areas are indicative of a need for ART.

Figure 2 summarizes how scores in these domains result in treatment eligibility. To be eligible for treatment, youth must first meet the appropriate criterion for recidivism risk, and second must meet cut-off scores for either family instability or aggression (Figure 2). For MST eligibility a youth must be rated high-risk and have a score of at least an 8 out of 34 on family instability. For FFT, the family instability situation criterion is the same (8 out of 34), but the youth must be at moderate recidivism risk or greater. For ART, the youth must be of at least moderate recidivism risk, in addition to having aggression problems (2 on a scale of 13), or attitude problems (5 on a scale of 23), or skills problems (4 on a scale of 18). We utilize these eligibility thresholds, essentially comparing youth on either side of the threshold who are otherwise similar on all other observed measures of risk, in order to assess the causal effects of the treatment interventions, as detailed below.

4. Analytical Framework

The objective of this study is to assess the causal impact of behavioral interventions on educational outcomes for justice-involved youth. We frame this question, and the empirical methodology, within an intent-to-treat analysis. Consider the following education production function relating eligibility for a particular treatment to post-treatment educational outcomes.

(1) $$E_{ist+k} = \alpha + \beta\, Elig_{it} + X_{ist+k}\Phi + \text{District}_k\Psi + Time_{t+k}\Omega + \mu_{it} + \varepsilon_{it+k}$$

Specifically, educational outcome (E; for instance, dropping out of school, GPA, or high school completion), for the i^{th} juvenile offender in school district s, realized over some period k beyond their intake at time t, is a function of their eligibility (*Elig*) for treatment subsequent to their offense and arrest. The outcomes are related to the three mental health behavioral interventions described above: ART, FFT, and MST. All models control for district-level fixed effects (*District*), to account for any stable heterogeneity across the 295 school districts in WA (for instance, related to services offered, district-specific policies and enforcement, district-level demographics and peer groups, and quality of educational inputs), and year fixed effects (*Time*), to account for statewide trends in schooling outcomes (for instance, related to state-level policy changes or general economic conditions affecting all school districts). We estimate models for dichotomous outcomes (for instance, dropout, transfer, school completion) via probit regression and models for continuous outcomes (for instance, GPA) via OLS.[9] Standard errors are adjusted for arbitrary correlation in the error term (ε) for a given individual over time.

The parameter of interest is β, which captures the causal intent-to-treat (ITT) effect of making an offender eligible for a given treatment program on their schooling outcomes. This parameter expressly identifies the reduced-form impact of the initial treatment eligibility, and as such is distinguished from the structural effect of the "treatment on the treated" (TOT), which would capture the impact of actually receiving the treatment. The ITT analysis is meaningful for three reasons. First, it provides valuable information on the likely direct effects of an important policy tool. State juvenile justice programs can manipulate the criteria determining treatment diversion and thus expand eligibility. However, eligibility does not necessarily translate into treatment receipt due to attrition and also because the treatment program may be unavailable in

[9] Results are not sensitive to alternate estimation methods and specifications, including logit and OLS models for dichotomous outcomes, and log-linear models for GPA.

the district or there are no slots available. Thus, the ITT is informative in gauging the "real world" policy response to expanded treatment diversion after accounting for offenders who may fail to start the program for various reasons or dropout prior to completion.

Second, structural estimation of the TOT is problematic since actual treatment receipt is endogenous. Offenders who actually receive their assigned treatment are not a random subset of all offenders, and even among those eligible for diversion into a specific behavioral intervention, starting and completing the program are likely correlated with the offender's unobserved traits such as effort, risk and time preference, own- and family-based motivation, parental supervision, peer support, and expectations. In focusing on the ITT, however, we are able to bypass non-random selection into treatment diversion by exploiting discrete discontinuities and cut-offs in the eligibility criteria, which are plausibly orthogonal to such unobserved offender-specific heterogeneity (μ). With detailed information on the offenders' social and criminal history, discontinuous differences in eligibility around the various cut-offs and thresholds within fine homogeneous cells can be exploited for credible identifying variation to bypass selection bias (described below).

Third, with the ITT in hand and using the information on average take-up rates for the behavioral interventions, the ITT estimates can be scaled upwards to impute the TOT effect of starting treatment. Alternately, the TOT can also be derived from an instrumental variables (IV) estimation of the structural equation directly relating treatment take-up to educational outcomes.

The empirical models proceed in a stepwise manner in order to address and inform these and other specific issues. First, we estimate a parsimonious version of equation (1), controlling for basic offender demographics along with district and year fixed effects. These models establish the potential selection bias at play. Given that eligibility for treatment diversion is limited to relatively higher-risk youth who have significant family difficulties and problems with their aggression, attitude, and coping skills – factors which would also predict poor school performance and enrollment – these naïve estimates of the ITT are expected to be biased downwards (upwards) for positive (negative) measures of school performance. We therefore expand this specification to further control for lagged schooling outcomes prior to the treatment screen, capturing the offender's GPA, whether he was suspended, was eligible for accommodations and services under Section 504, and their special education status (Model 2).[10] Such pre-treatment measures may partly account for selection bias from unobserved student- and family-specific factors (Angrist and Pischke 2009; Dave and Colman 2012). Comparing estimates across these two specifications can inform the likely direction of this bias, though the lagged controls are not expected to bypass all unmeasured selection.

Hence, in the next set of specifications, we specifically exploit variation in eligibility as driven by the discontinuous thresholds, within increasingly finer cells of homogeneous offenders. Model 3 non-parametrically controls for the criminal history score (ranging from 0-31) and the social history score (ranging from 0-18), and also adds linear and quadratic forms of the respective component scores that drive eligibility for ART, FFT, and MST.[11] Identification in this specification (3) is driven by the discontinuous jump in eligibility around the respective component score (for instance, the family instability score threshold used to determine treatment through FFT and MST), within groups of offenders who alternately have either the same total

[10] Section 504 is a part of the Rehabilitation Act of 1973 that prohibits discrimination based upon disability. Each district generally addresses students' needs and services under Section 504 differently, which would be captured by the district-specific fixed effects. Examples of such accommodations include highlighted textbooks, extra textbooks for home use, rearranged schedules, extended time on tests and assignments, preferred seating in the classroom, frequent feedback, peer assistance with note taking, oral exams in lieu of written exams, taping class lectures, and computer-aided instruction.

[11] We assessed sensitivity of all estimated effects to higher-order polynomials (cubic and quartic) for the component scores. Results (available upon request) are not materially affected. Also see Gelman and Imbens (2014), who recommend against controlling for higher-order polynomials in such regression discontinuity-based models.

criminal history risk score or the same total social risk history score. However, there still may exist considerable heterogeneity across offenders even with the same criminal or social history scores. In contrast to a conventional regression discontinuity design, wherein the discontinuity exists on a single forcing variable, with respect to the treatment diversion programs that we are studying, several discontinuous thresholds are relevant. Hence, the next three models attempt to exploit the discontinuities within progressively finer sub-cells of offenders who are more homogeneous in terms of their observables and presumably also on their unobservables.

Model 4 includes indicators for various key component measures that form the criminal history and social history scores, other than those in the component score used to determine treatment eligibility. For instance, the family instability score (which is part of the offender's broader social history) is based on measures such as the offender's current living arrangements, household income, problem history with parents and siblings, family support, parental supervision, and criminal history of other household members. This score governs eligibility into FFT or MST treatment (see Figure 2). Models do not control for these components separately as they are subsumed in the family instability score (which we control for through a second-order polynomial). However, we can control for a rich vector of other offender-specific characteristics from their social history and criminal history risk scores such as age of first offense, prior misdemeanor and felony arrests, prior detention and confinement, mental health and substance abuse history, peer relationships, and history of other abuse and neglect. Model 5 expands the specification to further include indicators for each of the nine cells in Figure 1, interacting low, moderate, and high social history risk with similar gradations in criminal history risk.

Model 6 is the fully-saturated model and is our preferred specification. It adds indicators for interactions between each score for social history and criminal history risk (31 x 18 = 558 indicators). The identification in this model is driven by individuals who are identical in terms of both their social and criminal histories (that is, they have exactly the same combination of social *and* criminal history scores), have similar pre-treatment educational outcomes, have similar histories with respect to peer and gang relationships, mental health, substance abuse, age at first offense, prior misdemeanors and felony arrests, prior detention and confinement, and various other component measures noted above, but differ only in being on either side of the eligibility score threshold, after accounting for a smooth second-order effect of this score. The thought experiment underlying this model to compare two youths from the same school district and period, both the same with identical social and criminal history scores, similar prior educational outcomes, and similar mental health, substance abuse, and offending history. The only observable difference relates to both youth being on either side of the eligibility threshold, thus making one eligible for treatment and the other ineligible.

Comparison of the estimates across models 1-6 provide valuable information on the direction of the selection bias, and whether this bias is being attenuated as the identifying variation is arguably becoming more credible.

Estimates from Models 1-6 represent the ITT effect averaged over the entire juvenile justice population in WA. Model 7 presents a robustness check by excluding those risk-groups who would not be eligible for a particular behavioral intervention; for instance, low-risk offenders are not eligible for diversion into ART and FFT, and both low- and moderate-risk offenders are not eligible for MST (see Figure 2). Thus, excluding these groups, as appropriate to the intervention being studied, should increase the magnitude of the estimated ITT, in the spirit of a dose-response check.

With the overall ITT effects in hand, we next assess heterogeneity in these effects across gender as well as across prior school performance measures reflected in the offenders' initial screening assessment. Specifically, we explore whether program effectiveness varies across measures of academic performance (grades), attendance, and the interviewer's assessment of the likelihood that the offender will stay in and graduate from high school. It should be noted that these performance measures are predetermined and predate the treatment. These analyses are important towards understanding whether the treatment interventions can improve schooling among those who are at the greatest disadvantage and at a relatively higher-risk of reoffending.

Finally, we supplement the ITT analyses with some evidence on the effects of the "treatment on the treated". Based on observed take-up of the programs conditional on being eligible, the ITT can be rescaled to derive an estimate of the implied TOT. Alternately, as a robustness check and point of comparison with the rescaled estimates, the TOT can also be directly estimated via an instrumental variables (IV) strategy. Eligibility for treatment diversion, in the context of the discontinuities noted above, would constitute a plausibly valid IV for treatment take-up. Additionally, many offenders are unable to commence their assigned treatment program because the particular program is not available in their area at the time or because all slots in the program are filled. These supply-side constraints can also be used as an additional IV in the structural estimation of the TOT.

5. Results

Table 1 presents means for key variables over our analysis sample. The average juvenile offender is 16 years of age, male (76% of the sample), and White (60% of the sample). Given the high-risk nature of this sample of justice-involved youth, school performance measures are expectedly low. Over the one-year period after screening in the juvenile justice system, the school dropout rate is relatively high at 27.6%, with about 51.5% suspended or leaving school this period, and the average GPA is 1.53. Among offenders ages 17 or above, only about 23.4% complete high school or their GED over the two-year period after screening. Columns 2 and 3 present means for offenders stratified across their eligibility status for treatment with FFT, and the final two columns present means based on eligibility for MST. Youths who are eligible for these intensive treatment programs are not a random subset of all justice-involved youths. About 68% of FFT-eligible youth had low grades prior to their treatment diversion and only 18.1% attended school regularly. This compares with FFT-ineligible youth, among whom 43.1% had low grades and 52.7% attended school regularly. Furthermore, youth who are eligible for treatment services are relative higher-risk in terms of both their criminal history as well as their social history, and more likely to have experienced family problems. These systematic differences are also expected to confound post-treatment schooling outcomes, which on average are generally worse among the eligible youth. For instance, the one-year school dropout rate is 32.8% among those who are FFT-eligible, compared with 23.9% among those who are ineligible. The multivariate models presented below address this non-random selection bias.

Table 2 presents estimates of the ITT effect of ART eligibility on schooling outcomes, measured over a period of 12 months subsequent to treatment screening. Column 1 presents estimates from a parsimonious version of equation (1), controlling only for the offender's socio-demographics in conjunction with district and year fixed effects. Across all outcomes, these estimates suggest that eligibility for diversion into ART is associated with worse school performance (significantly higher dropout and lower GPA) and bring to light the substantial selection bias at play. Observed and unobserved risk factors underlying the youth's social and criminal history, which are key drivers of eligibility into the behavioral therapy program, are also predictive of poor school outcomes. Thus, these naïve estimates are biased towards showing that the intervention is less effective in improving schooling than may be the case. Models 2-7 add richer controls to account for this selection, while exploiting more selective variation surrounding the discontinuous eligibility cut-offs within increasingly finer cells of like youth. Comparing estimates across specifications 1-7 suggests that the bias is attenuated in the expected direction as we employ more credible identifying variation. However, estimates from our preferred specification (model 6) do not show any significant effects on these measures of education.

It should be noted that ART is implemented through group therapy format, and is the least intensive of the three mental health interventions, lasting only 10 weeks. While ART teaches youths to control their angry impulses and aggression, and develop their moral reasoning skills, there is no explicit educational goal. In this case, we do not find that ART leads to any discernible academic improvements over a one-year follow-up.

Tables 3 and 4 present the estimated ITT effects for the more intensive of the treatment programs, FFT and MST respectively. Specifically, Panel A of Table 3 assesses how eligibility for FFT affects a broad measure of school absentee status, which includes both dropping out of school as well as suspensions. As before, models 1 and 2 suggest that eligibility is associated with a higher likelihood of dropping out, an association that is largely driven by the positive selection bias. This bias diminishes, and effect sizes shift towards becoming more negative as successive models exploit more credible variation. Our preferred specification (model 6) controls for all criminal and social risk factors non-parametrically and controls for the third component risk factor (family instability score) parametrically. By specifying the third factor parametrically we achieve identification from the discrete jump in eligibility at the cut-off for this domain.

Here we find that the ITT effect of a 100% increase in FFT eligibility is to reduce the 12-month dropout rate by about 3.1 percentage points (about 5% relative to the baseline mean), though this effect is imprecisely estimated (p-value of 0.18) due to reduction in sample size and the variation being exploited.[12] In model 7, the sample size is limited only to those youth who are classified as moderate or high risk. It is validating that excluding the low-risk individuals from the sample, who are not eligible for FFT, causes the magnitude to increase from 3.1 to 4.0 percentage points (p-value of 0.13). We do not find any significant or meaningful effects of FFT eligibility on the probability of being suspended.[13] Thus, all of the effects on the broad measure of absentee status are driven by decreases in the likelihood of dropping out of school. Panel B of Table 3 confirms this, suggesting that a 100% increase in FFT eligibility reduces the propensity to drop out of school over the next 12 months by about 5 percentage points (about 18% relative to the sample mean).

Panels C and D suggest slight negative effects on both GPA (by about 0.03 index points) and the likelihood of transferring to another school (by about 1.3 percentage points). Though none of these estimates are statistically significant in the saturated model, they are consistent with the reduction in dropout. The lower likelihood of transfers is potentially less disruptive and may promote the likelihood of school retention. Prior work on other educational inputs has also

[12] As models become progressively richer, we lose observations due to missing information on the pre-screen. The observations with missing information on the pre-screen are not significantly different (beyond a Type I error) with respect to all other observable characteristics relative to the full sample. We further confirm that the progression of the direction and magnitude of the effects across models 1-6 is not driven by the reduction in sample size; the patterns and magnitudes are quite robust to estimating all models for the most limited sample utilized in model 6.

[13] Results are not reported for economy of space, and available upon request.

generally been hard pressed to find positive beneficial effects on GPA and academic achievement (Cullen, et al 2013). In our case, the greater selection of higher-risk and lower-achieving students staying in school may be responsible for the slight negative effects on GPA.

The ITT effects for MST, which is the most intensive of the behavioral mental health interventions, are presented in Table 4. Generally, these effects mirror those for FFT (in Table 3), suggesting significant decreases in the likelihood of dropping out of school. The effect magnitudes for the overall sample are quite similar to those estimated for FFT eligibility – on the order of 3-4 percentage points decrease in dropout – though when the sample is restricted to only the high-risk youth (low- and moderate-risk youth are not eligible for MST) in model 7, the magnitudes expectedly increase. The last column suggests an 8-10 percentage point decrease in the probability of dropping out of school due to a 100% increase in MST eligibility. This higher ITT effect may reflect both the greater intensity of the intervention as well as a differentially larger effect due to the more higher-risk individuals being targeted for this treatment. In later stratified analyses, we further explore this question of heterogeneous treatment effects based on schooling-related risk measures.

The analyses thus far suggest that the intensive interventions, FFT and MST, increase the likelihood of staying in school in the short term. Table 5 assesses whether these effects are persistent and translate into a greater likelihood of completed secondary school or its equivalent through the GED. As before, models 1 and 2 suggest that there is strong negative selection bias leading to higher eligibility being associated with a lower likelihood of graduation/GED completion. This selection bias is attenuated in models 3-7, however, which exploit the discontinuities and fully saturate the non-parametric controls for interactions between social risk and criminal history risk. These models suggest an increase in the probability of completing secondary school on the order of 3-5 percentage points (10-15 % relative to the mean). The effect sizes are higher in the final specification (5-10 percentage points), when the sample is restricted to only those risk-levels which pass the first stage of eligibility requirements for FFT and MST.

Justice-involved youth are predominantly males, about 75% of the sample. Furthermore, a large literature suggests differences in problem behaviors and coping mechanisms across boys and girls. Thus, Table 6 explores heterogeneity in the ITT effects across gender (columns 1 and 2). In general, we find that both FFT and MST eligibility are effective in reducing dropout for males and females; however, due to inflated standard errors, we cannot rule out that there is no difference in the ITT effects, though the ITT effect for MST is several orders of magnitude higher for females than for males. A larger ITT effect may reflect either an increase in take-up rates and/or an increase in the effectiveness of actually receiving the treatment (TOT). Over the sample period, the take up rate for MST is not significantly higher among females, and thus the suggestively higher ITT effect for MST for females may reflect a greater effectiveness of the program for this group.

Given that FFT and MST diversion appears to be effective in raising school retention and graduation in general among those who are made eligible, an important question is whether these interventions are effective for those youth whose educational prospects are among the weakest – those youth who are at higher risk of dropping out of school and recidivating. Models 3-8 assess heterogeneity in the ITT effects based on predetermined schooling outcomes and assessment, measured prior to the treatment screening. In general, higher eligibility for both FFT and MST impart a positive benefit for students, regardless of their prior school performance and attendance. Even among students who previously were performing poorly in school (low grades and/or poor attendance record), the ITT effect for both interventions is to reduce the likelihood of dropping out of school. The policy relevance of these effects is particularly prominent in columns 7 and 8. The prescreen included the interviewer's assessment, based on the various domains and their own impressions of the youth, his/her family, and the surroundings, on how likely is the youth to graduate from high school. While the effects are slightly weaker, there is evidence that these interventions can impart important positive educational spillovers to at-risk youth whose prospects of graduating from high school were particularly poor.

We note that the effects discussed thus far are intent-to-treat effects, measuring the effects of eligibility rather than the effects of actual treatment receipt (TOT). The two diverge since take-up rates of the treatment interventions are far less than 100%. Given the credible reduced-form effects of eligibility on education, we can nevertheless derive the structural TOT through two alternate methods. First, based on the observed program take-up rates, the ITT can be scaled upwards by an appropriate factor to inform the magnitude of the TOT (Corman et al. 2014). With respect to MST, the unconditional take-up rate is about 20% and the conditional take-up rate (from a first-stage model relating eligibility to take-up; see Table 7) is about 30%. This means that between 20-30% of MST-eligible youth actually start the program; the rest do not receive the treatment for various reasons, related to the program not being offered in the area, no slots being available, or refusal to enter the program. Thus, the ITT needs to be scaled upwards by a factor of 4 in order to derive the structural effect of entering the program on educational acquisition. Doing so implies that actually starting FFT for a youth who is made eligible, by being just across the score threshold, reduces the likelihood of dropping out of school by about 12-20 percentage points, and raises the likelihood of completing secondary school by about 11-19 percentage points.[14] Take-up rates for MST are far lower, about 4-8% (based on first-stage models presented in Table 7), mostly because program participation is quite intensive and also because of greater supply-side constraints; significantly more eligible youth do not enter MST because the program is not offered in their area or because of no available slots relative to FFT-eligible youth. Thus the ITT effects for MST would need to be rescaled upwards by a factor of about 16 to impute the TOT. This process implies that starting the MST program reduces the probability of dropping out of school (over the next 12 months) by about 42-45 percentage points, and raise the probability of completing high school by about 24-41 percentage points. Implicit TOT effects rescaled in this manner should be interpreted with caution since small changes in the denominator (in this case, the FFT and MST program take-up rates) and the underlying ITT estimates can lead to large differences. Nevertheless, these findings are consistent with the IV-based models presented in Table 7, which directly estimate the TOT.

In the fully-saturated model, eligibility for the particular treatment program under study and an indicator for whether the youth did not start the program due to supply-side constraints (it was not offered in the area, or there were no available spots) are plausible IVs for program take-up. Indeed, the first-stage estimates reported in Table 7 confirm this, and show that eligibility significantly and positively predicts program take-up while supply-side constraints significantly reduce take-up. Particularly for ART and FFT, the IVs strongly predict take-up, with the F-statistic on the excluded instruments ranging from 107 to 564. The IVs are jointly significant at the 1-percent level for MST take-up, though the F-statistic is lower (ranging from 13 to 32) due to the weaker take-up rate of the program among eligible youth.[15] The overidentification test

[14] These estimates are based on the ITT effects reported in models 5 and 6.

[15] This is due to the reasons noted in the text. There is a higher rate of refusal because the MST program is intensive and requires a significant level of participation and effort from the youth and their families; also, supply constraints are more binding given the intensive resources required for offering MST.

also supports the orthogonality of the IVs from the structural error; while this is statistically validating, we note that the test should be interpreted with caution since the two instruments (eligibility and supply constraints) are likely working at different margins with heterogeneous treatment effects across these margins.[16]

Columns 1-3 present the TOT effects for ART take-up on dropout and high school completion (over 12- and 24-months post screening). Consistent with the weak to null ITT effects, we do not find any significant or meaningful effects of ART take-up on drop-out or graduation over the short-term. Column 3 is suggestive of some positive cumulative effect on completion rates over a 24-month period. Columns 4-6 report the TOT effects of FFT take-up, which are fully in line with the earlier-report ITT effects. Take-up of FFT reduces the likelihood of dropping out of school by about 9 percentage points and raises the likelihood of graduating by about 5-8 percentage points. It is validating that these directly estimated TOT magnitudes are similar to the lower-bound TOT effects derived by rescaling the ITT effects above. The final three models present the TOT estimates for MST participation. These also confirm that MST take-up reduces the likelihood of dropping out of school by about 24 percentage points and increases the likelihood of completing secondary school by between 25-53 percentage points – estimates which are again consistent with the ITT effects and the rescaled TOT effects. That is, while most youths who are eligible for MST do not enter treatment, those who do significantly benefit from it in the form of greater educational attainment.

6. Conclusions

The study adds to the sparse literature linking justice policies to outcomes other than crime – most notably, education acquisition – which has important benefits both monetarily and

[16] The Hausman test also confirms that program take up is expectedly endogenous.

non-monetarily. Our analyses suggest that intensive behavioral interventions, such as FFT and MST, can have positive impacts on academic achievement for troubled teens. The evidence is consistent with improvements in school completion rates for justice-involved youth. Moreover, the results suggest that the effects may be larger for girls and for youth who are believed to have worse education prospects. The evidence to support ART, a less intensive intervention, is not as strong.

According to the Blueprints Initiative, FFT costs $2,800 and MST costs $7,068 per youth. Our estimates suggest that actual FFT take-up reduces drop-out by about 10 percentage points, and MST take-up reduces drop out by about 24% (based on the IV estimates). The average high-school dropout earns $20,241 (U.S. Census) annually. The return to an extra year of education, even if this person does not finish high school is 7-8% (Angrist and Krueger, 1991). Hence, an extra year of schooling is predicted to raise earnings by about $1,500. FFT participation will therefore, on average, raises earnings by about $150/year ($1,500*0.075) and MST participation will raise earnings by about $375 ($1,500*0.24). In present value terms, the lifetime increase would more than compensate for the cost of both FFT and MST, even at the conservative benefit levels of just reducing dropout (not even counting high school completion).

The results of this study are important because they suggest that interventions for youth in the justice system have broad societal benefits outside the health care and juvenile justice sectors. Consequently, a strong argument could be made for making these interventions much more widely available through education-system investments.

We conclude that mental health treatment interventions offered by the justice system can have important impacts on human capital development. Consistent with other recent studies, we find evidence that intervention programs can make an academic difference for adolescents Our results show that these interventions are effective even among the highest-risk youth with poor schooling prospects. Future work should focus on interventions modified for adolescents with other problems, such as substance abuse.

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Figure 1

Recidivism Risk-level definitions based on Criminal History and Social History Risk Scores			
Criminal History Risk Score	**Social History Risk Score**		
	0 – 5	**6 – 9**	**10 – 18**
0 – 2	Low	Low	Moderate
3 – 7	Low	Moderate	High
8 – 31	Moderate	High	High

Figure 2

Treatment Eligibility Determination		
Treatment Type	**Risk-level**	**Other**
Multi-systemic Therapy (MST)	High	Family Instability: Risk Factor Score of at least 8 out of 34 on specific measures of family instability within the Social History component
Family Functional Therapy (FFT)	Moderate	Family Instability: Risk Factor Score of at least 8 out of 34 on specific measures of family instability within the Social History component
Aggression Replacement Training (ART)	Moderate	Aggression, Attitude, or Social Skills Problems: Risk Factor Score ≥ 2 (out of 13) on the Aggression component domain of Social History risk Risk Factor Score ≥ 5 (out of 23) on the Attitudes/Behavior component domain of Social History risk Risk Factor Score ≥ 4 (out of 18) on the Skills component domain of Social History risk

Table 1
Sample Means
Matched Intervention-Education Records from WA, 2004-2009

Variable	1 All	2 FFT Eligible	3 FFT Not Eligible	4 MST Eligible	5 MST Not Eligible
Age	15.92	15.80	16.00	15.79	15.97
Male	0.7557	0.7576	0.7545	0.7765	0.7470
White	0.6004	0.5702	0.6208	0.5523	0.6205
Black	0.1145	0.1313	0.1032	0.1440	0.1022
Asian	0.0342	0.0260	0.0397	0.0244	0.0382
Hispanic	0.1574	0.1637	0.1531	0.1649	0.1542
Race/Ethnicity missing	0.0936	0.1087	0.0833	0.1144	0.0849
Absent (Dropout, Suspended or No-show) (1-year)	0.6362	0.7614	0.5483	0.7848	0.5721
Dropout (1-year)	0.2756	0.3276	0.2391	0.3336	0.2505
Transfer (1-year)	0.8844	0.9135	0.8640	0.9184	0.8697
Suspended or Left (1-year)	0.5147	0.6464	0.4222	0.6783	0.4440
GPA (1-year)	1.5319	1.2676	1.7041	1.2483	1.6450
High School / GED Completion (2-year) (all ages)	0.1013	0.0560	0.1342	0.0495	0.1248
High School / GED Completion (2-year) (ages 17+)	0.2338	0.1170	0.2985	0.0979	0.2804
ART Eligible	0.6332	0.9831	0.3963	0.9905	0.4839
FFT Eligible	0.4037	1	0	1	0.1545
MST Eligible	0.2948	0.7302	0	1	0
Social History Score	6.6363	9.2985	4.8337	10.2517	5.1250
Criminal History Score	7.7280	9.9231	6.2417	11.0925	6.3215
Risk Level – Low	0.3396	0	0.5696	0	0.4816
Risk Level – Moderate	0.2543	0.2698	0.2437	0	0.3605
Risk Level – High	0.4061	0.7302	0.1867	1	0.1579
Family Dysfunction Score	7.0578	13.3640	2.7878	13.9146	4.1914
Family Dysfunction Score ≥ 8	0.4245	0.9865	0.0440	0.9887	0.1887
Low Grades – GPA ≤ 2.0 (Pre-Treatment)	0.5326	0.6823	0.4312	0.7103	0.4583
Attended School Regularly – Few or No Unexcused Absences (Pre-Treatment)	0.3874	0.1809	0.5272	0.1386	0.4914
Interviewer's assessment: Very likely that the youth will stay in and graduate from high school or equivalent (Pre-Treatment)	0.1694	0.0957	0.2193	0.0593	0.2155

Notes: Maximum number of observations is 35,020; sample size is lower for some variables due to missing information (see text).

Table 2
Effects of Aggression Replacement Training (ART) Eligibility on 12-month School Outcomes

Model	1	2	3	4	5	6	7
Panel A	Outcome: Absent (Dropout, Suspended, or No-show)						
Eligibility - ART	0.2421***	0.1823***	0.0333	0.0382*	0.0425	0.0463	0.0564
	(0.0075)	(0.0115)	(0.0230)	(0.0232)	(0.0374)	(0.0382)	(0.0393)
Observations	23389	10385	7339	7305	7305	7076	5688
Panel B	Outcome: Dropout						
Eligibility - ART	0.0837***	0.0431***	0.0221	0.0247	0.0241	0.0264	0.0487
	(0.0067)	(0.0100)	(0.0209)	(0.0209)	(0.0332)	(0.0333)	(0.0332)
Observations	23311	10268	7295	7262	7262	7031	5701
Panel C	Outcome: GPA						
Eligibility - ART	-0.3645***	-0.0296***	-0.0238	-0.0225	0.0109	0.0152	0.0172
	(0.0157)	(0.0103)	(0.0195)	(0.0191)	(0.0319)	(0.0317)	(0.0335)
Observations	19728	10191	7244	7210	7210	7210	5875
Panel D	Outcome: Transfer						
Eligibility - ART	0.0390***	0.0575***	-0.0195	-0.0155	-0.0235	-0.0208	-0.0090
	(0.0045)	(0.0082)	(0.0127)	(0.0126)	(0.0198)	(0.0218)	(0.0216)
Observations	23117	10189	7056	7023	7023	6421	5104
District indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-screening school outcomes	No	Yes	Yes	Yes	Yes	Yes	Yes
Relevant Score(s) / Score-squared	No	No	Yes	Yes	Yes	Yes	Yes
Crim. History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Social History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Detailed components of Criminal & Social History	No	No	No	Yes	Yes	Yes	Yes
Interactions: Social History Risk Level * Crim. History Risk Level (9 cells: see Fig. 1)	No	No	No	No	Yes	Yes	Yes
Interactions: Social History Score * Criminal History scores (558 cells: see Fig. 1)	No	No	No	No	No	Yes	Yes
Sample	All	All	All	All	All	All	Moderate or High Risk

Notes: Marginal effects from probit regression models are presented for absent, dropout, and transfer; coefficients from OLS models are presented for GPA. Standard errors are clustered at the individual level, and reported in parentheses. Youth who receive FFT, MST or COS are excluded from the sample. All models control for socio-demographics (age, gender, race/ethnicity), and district and year fixed effects. Pre-screening school outcomes include GPA, suspension, eligibility for Section 504 services, and special education. "Relevant score(s)" and "score-squared" refer to the family instability and other scores, which determine eligibility for the particular treatment program, as denoted in Figure 2. Detailed components of criminal and social history include indicators for age of first offense, categorical indicators for the number of prior misdemeanor referrals and for prior felony referrals, indicators for the number of prior sexual misconduct misdemeanor referrals and for prior such felony referrals, categorical indicators for prior detention and for prior Juvenile Rehabilitation Administration (JRA) confinement episodes, history of and current pro-social/anti-social friends/peers, history of and current involvement in gangs, history of truancy/running away, history of neglect, history of court-ordered out-of-home placement, and history of mental health problems, alcohol and drug problems, and abuse. Asterisks denote statistical significance as follows: *** p-value ≤ 0.01; ** 0.01 < p-value ≤ 0.05; * 0.05 < p-value ≤ 0.10.

Table 3
Effects of Functional Family Treatment (FFT) Eligibility on 12-month School Outcomes

Model	1	2	3	4	5	6	7
Panel A	Outcome: Absent (Dropout, Suspended, or No-show)						
Eligibility - FFT	0.2007***	0.1370***	-0.0329	-0.0393*	-0.0312	-0.0314	-0.0398
	(0.0072)	(0.0113)	(0.0218)	(0.0221)	(0.0226)	(0.0235)	(0.0265)
Observations	22179	9925	9923	9890	9890	9659	5823
Panel B	Outcome: Dropout						
Eligibility - FFT	0.0866***	0.0368***	-0.0509***	-0.0581***	-0.0501***	-0.0517***	-0.0573**
	(0.0072)	(0.0103)	(0.0173)	(0.0174)	(0.0178)	(0.0179)	(0.0266)
Observations	22107	9805	9805	9773	9773	9542	5807
Panel C	Outcome: GPA						
Eligibility - FFT	-0.3370***	-0.0405***	-0.0340*	-0.0293	-0.0296	-0.0298	-0.0419
	(0.0166)	(0.0121)	(0.0198)	(0.0199)	(0.0206)	(0.0208)	(0.0284)
Observations	18739	9729	9729	9696	9696	9696	5968
Panel D	Outcome: Transfer						
Eligibility - FFT	0.0346***	0.0497***	-0.0138	-0.0074	-0.0056	-0.0132	-0.0114
	(0.0042)	(0.0075)	(0.0149)	(0.0147)	(0.0149)	(0.0160)	(0.0168)
Observations							
District indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-screening school outcomes	No	Yes	Yes	Yes	Yes	Yes	Yes
Relevant Score(s) / Score-squared	No	No	Yes	Yes	Yes	Yes	Yes
Crim. History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Social History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Detailed components of Criminal & Social History	No	No	No	Yes	Yes	Yes	Yes
Interactions: Social History Risk Level * Crim. History Risk Level (9 cells: see Fig. 1)	No	No	No	No	Yes	Yes	Yes
Interactions: Social History Score * Criminal History scores (558 cells: see Fig. 1)	No	No	No	No	No	Yes	Yes
Sample	All	All	All	All	All	All	Moderate or High Risk

Notes: See Table 2. Youth who receive ART, MST or COS are excluded from the sample.

Table 4
Effects of Multi-systemic Therapy (MST) Eligibility on 12-month School Outcomes

Model	1	2	3	4	5	6	7
Panel A	Outcome: Absent (Dropout, Suspended, or No-show)						
Eligibility - MST	0.2031***	0.1333***	-0.0456**	-0.0497**	-0.0323	-0.0373	-0.0834**
	(0.0080)	(0.0130)	(0.0225)	(0.0228)	(0.0264)	(0.0278)	(0.0343)
Observations	20299	9269	9267	9236	9236	9010	3060
Panel B	Outcome: Dropout						
Eligibility - MST	0.0747***	0.0278**	-0.0291*	-0.0346**	-0.0259	-0.0282	-0.0991**
	(0.0085)	(0.0118)	(0.0167)	(0.0168)	(0.0203)	(0.0205)	(0.0420)
Observations	20197	9150	9150	9120	9120	8890	3130
Panel C	Outcome: GPA						
Eligibility - MST	-0.2991***	-0.0283*	-0.0425**	-0.0298	-0.0569**	-0.0610**	-0.0562
	(0.0198)	(0.0145)	(0.0213)	(0.0214)	(0.0267)	(0.0271)	(0.0469)
Observations	17227	9090	9090	9059	9059	9059	3314
Panel D	Outcome: Transfer						
Eligibility - MST	0.0370***	0.0533***	-0.0212	-0.0171	-0.0153	-0.0202	0.0064
	(0.0048)	(0.0084)	(0.0163)	(0.0161)	(0.0186)	(0.0203)	(0.0234)
Observations	20036	9080	9066	9035	9035	8456	2693
District indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-screening school outcomes	No	Yes	Yes	Yes	Yes	Yes	Yes
Relevant Score / Score-squared	No	No	Yes	Yes	Yes	Yes	Yes
Crim. History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Social History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Detailed components of Criminal & Social History	No	No	No	Yes	Yes	Yes	Yes
Interactions: Social History Risk Level * Crim. History Risk Level (9 cells: see Fig. 1)	No	No	No	No	Yes	Yes	Yes
Interactions: Social History Score * Criminal History scores (558 cells: see Fig. 1)	No	No	No	No	No	Yes	Yes
Sample	All	All	All	All	All	All	High Risk

Notes: See Table 2. Youth who receive ART, FFT or COS are excluded from the sample.

Table 5
Effects of FFT & MST Eligibility on 24-month High School / GED Completion

Model	1	2	3	4	5	6	7
Panel A							
Eligibility - FFT	-0.0666***	-0.0599***	0.0257	0.0249	0.0261	0.0476***	0.0479
	(0.0057)	(0.0106)	(0.0199)	(0.0200)	(0.0199)	(0.0080)	(0.0372)
Panel B							
Eligibility - MST	-0.0631***	-0.0480***	0.0433**	0.0395*	0.0247	0.0144	0.1052***
	(0.0061)	(0.0106)	(0.0229)	(0.0225)	(0.0244)	(0.0405)	(0.0294)
District indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-screening school outcomes	No	Yes	Yes	Yes	Yes	Yes	Yes
Relevant Score / Score-squared	No	No	Yes	Yes	Yes	Yes	Yes
Crim. History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Social History Score indicators	No	No	Yes	Yes	Yes	Yes	Yes
Detailed components of Criminal & Social History	No	No	No	Yes	Yes	Yes	Yes
Interactions: Social History Risk Level * Crim. History Risk Level (9 cells: see Fig. 1)	No	No	No	No	Yes	Yes	Yes
Interactions: Social History Score * Criminal History scores (558 cells: see Fig. 1)	No	No	No	No	No	Yes	Yes
Sample	All	All	All	All	All	All	FFT: Moderate or High Risk MST: High Risk

Notes: See Tables 2-4. Observations range from 10137 to 1541 as models include more detailed information on social and criminal history.

Table 6
12-month Dropout
Heterogeneity across Gender and Prior (Pre-screening) School Performance

Model	1	2	3	4	5	6	7	8
Sample	Males	Females	Low Grades	High Grades	Irregular Attendance	Regular Attendance	Assessment: Uncertain / Unlikely to Graduate	Assessment: Likely to Graduate
Panel A								
Eligibility - FFT	-0.0613***	-0.0529	-0.0771***	-0.0516*	-0.0588*	-0.0458*	-0.0406*	-0.0498
	(0.0223)	(0.0416)	(0.0286)	(0.0263)	(0.0339)	(0.0239)	(0.0208)	(0.0442)
Observations	7075	1947	4606	3485	3639	4472	7920	1173
Panel B								
Eligibility - MST	-0.0245	-0.1028**	-0.0314	-0.0814**	-0.0182	-0.0448	-0.0302	-0.0482
	(0.0244)	(0.0431)	(0.0311)	(0.0268)	(0.0364)	(0.0292)	(0.0227)	(0.0592)
Observations	6593	1785	4232	3273	3330	4202	7404	1038
District indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-screening school outcomes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relevant Score / Score-squared	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crim. History Score indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Social History Score indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Detailed components of Criminal & Social History	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions: Social History Risk Level * Crim. History Risk Level (9 cells: see Fig. 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions: Social History Score * Criminal History scores (558 cells: see Fig. 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See Tables 2-4. Sample stratified across school performance utilize predetermined measures which refer to the period prior to the treatment screening.

Table 7
TOT Effects of Treatment Take-up on Dropout and Completion
IV Estimation

Treatment Intervention	ART			FFT			MST		
Outcome	Dropout – 12 months	School Completion - 12 months	School Completion – 24 months	Dropout – 12 months	School Completion - 12 months	School Completion – 24 months	Dropout – 12 months	School Completion - 12 months	School Completion – 24 months
TOT Effect of Treatment Take-up	0.0341	-0.0004	0.1142**	-0.0901*	0.0479*	0.0761	-0.2390	0.2469	0.5250**
	(0.0541)	(0.0313)	(0.0554)	(0.0498)	(0.0287)	(0.0587)	(0.4847)	(0.2292)	(0.2582)
First-stage: Instruments									
Eligibility	0.4178***	0.4424***	0.4378***	0.3164***	0.3229***	0.3090***	0.0384***	0.0426***	0.0761***
	(0.0208)	(0.0179)	(0.0424)	(0.0123)	(0.0109)	(0.0226)	(0.0068)	(0.0063)	(0.0164)
Treatment not available / Capacity constraint	-0.1534***	-0.1604***	-0.1921***	-0.0942***	-0.1022***	-0.1223***	-0.0148***	-0.0167	-0.0251***
	(0.0071)	(0.0060)	(0.0165)	(0.0066)	(0.0058)	(0.0148)	(0.0027)	(0.0025)	(0.0067)
F-statistic on Excluded IVs	373.27***	563.54***	110.07***	352.17***	485.35***	106.74***	21.39***	32.36***	13.01***
District indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-screening school outcomes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relevant Score(s) / Score-squared	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crim. History Score indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Social History Score indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Detailed components of Criminal & Social History	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions: Social History Score * Criminal History scores (558 cells: see Fig. 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8240	11026	2919	11401	14937	3540	11401	14937	3540

Notes: Coefficients from models estimated via two-stage least squares are reported. Standard errors are clustered at the individual level, and reported in parentheses. See Table 2 for the full list of control variables.