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# FINAL TECHNICAL REPORT

## Finding effective ways to reduce truancy:

## An evaluation of the Ramsey County Truancy Intervention Programs

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# Abstract

Each year, approximately 1 in 7 students in the U.S. are deemed chronically absent (defined as missing 15 or more days of school). Chronic absenteeism has been linked to lower math and reading achievement outcomes, decreased educational and social engagement, delinquency, and later criminal justice involvement. Failure to complete high school disproportionately affects youth from low-income households, youth of color who are from single-parent homes, and youth who attend large, urban public high schools.

After a certain number of unexcused absences, with the specific number varying across states, absenteeism is considered educational neglect among elementary students and truancy among middle and high school students. Many school districts have implemented diversion strategies to reduce educational neglect petitions to family court or truancy petitions to juvenile court. The most widely-implemented diversion model has three steps: 1) a group meeting with truant students and parents, 2) development of a formal attendance-improvement plan with the student and family that includes, as appropriate, referrals to social service agencies, and 3) a petition to juvenile court.

We conducted a causal study using a rigorous quasi-experimental design to determine whether two truancy court-diversion programs in Ramsey County, Minnesota increased student attendance. The programs were developed by the Ramsey County Attorney's Office and are run jointly by the county attorney's office and all school districts in the county. The Truancy Intervention Program (TIP) serves adolescents aged 12–17 years old and the Family Truancy Intervention Program (FTIP) serves children ages below age 12 and their families. In addition, we examined whether there were ethnic or racial disparities in referral to TIP or FTIP, conditional on the student's level of attendance.

To estimate a causal relationship, we linked longitudinal administrative data from multiple state and local agencies and implemented difference-in-difference analyses using a

matched comparison group of students from neighboring school districts that were not involved with TIP or FTIP and did not offer similar programs. The datasets were linked and kept secure by the Minn-LInK Project at the University of Minnesota. After linking the data we matched students in the intervention to students in Hennepin County, the immediately adjacent metropolitan county that was demographically and geographically similar to the intervention county and had experienced similar macro-economic and demographic shifts over time.

The linked dataset contained 4,412 students in grades 7–10 referred to TIP between 2006 and 2010 and 1,285 students in grades 2–5 referred to FTIP. The average daily attendance rate among students referred to TIP was 85%, the equivalent of missing 27 days of school in a full academic year. The average daily attendance rate of program-referred elementary students was 89%, the equivalent of missing 20 days of school in a full academic year for the elementary students.

We tested the effects of the intervention on all students referred by schools to TIP or FTIP. The intervention and matched comparison groups had statistically indistinguishable attendance trends in the years after the intervention—except for students referred to TIP in $8^{th}$ grade. In this grade, students in the intervention group had lower attendance in the 2–4 years after the intervention. The program also did not increase attendance among the subset of students whose parents attended the parent meeting relative to a matched comparison group.

We also found that at each level of total or overall absenteeism, measured using the annual absenteeism rate, White students were referred to TIP at significantly lower rates than students in all other racial and ethnic groups. In contrast, there were no statistically significant racial/ethnic differences in the proportion of students referred to TIP at each level of unexcused absences. In the elementary schools, a higher proportion of Black and American Indian/Alaskan Native students were referred to FTIP, compared to all other

students, at each level of total absences, as calculated from the average daily attendance rate. In contrast, there was no statistically-significant racial disparity in referral to FTIP when attendance was measured as the number of unexcused days, the actual referral criteria for FTIP

Findings from this study highlight the importance of having a strong quasi-experimental or experimental design for studying truancy programs. Students in FTIP experienced short-term rebounds in their attendance and student in TIP experienced a short-term leveling out in a general pattern of declining attendance. However, these same patterns of rebound occurred in the matched comparison groups due to regression to the mean, a statistical phenomenon that always occurs when interventions are applied to extreme cases in a sample distribution (e.g., to students on the low end of the attendance distribution). If a single-group pretest-posttest study had been implemented, regression to the mean could be easily misinterpreted as a positive program effect.

Despite the rigorous quasi-experimental design and sensitivity analyses, our study has some limitations: 1) no documentation of implementation fidelity, 2) the possibility that the counterfactual is non-equivalent to the intervention group on time-varying unobserved characteristics related to the outcome, and 3) incomplete data on unexcused absenteeism for some schools.

Since the National Institute of Justice funded the present evaluation, this type of three-step court diversion program for chronic absenteeism has become the single-most common truancy intervention in the United States. Given the high prevalence of court diversion models, which are increasingly codified in state laws, along with limited research and conflicting study findings, the clearest policy implication is the need for more causal research on school attendance and graduation.

# Table of Contents

**List of Figures**

**List of Tables**

# Introduction and Background Literature

## The Prevalence of Chronic Absenteeism

Chronic absenteeism from school is an educational issue, a social issue, and a criminal justice issue worthy of public and private attention. Excessive absence from school is a barrier to academic achievement and, ultimately, to graduation. It is estimated that one-third of all public high school students in the United States (U.S.) fail to graduate high school; and, on average, 7,000 students drop out of school every day (Swanson, 2004). During the 2013–2014 academic year, more than 6 million children or 1 in 7 students in the U.S., were chronically absent (defined as missing 15 days or more of school; U.S. Department of Education, 2016). Failure to complete high school disproportionately affects low-income students, youth of color who are from single-parent homes, and students who attend large, public high schools in the inner city (Bridgeland, Dilulio, & Morison, 2006). Black, Hispanic or Native American students finish public high school with a diploma at a lower rate than White or Asian students (approximately 50% compared to 75–77%, respectively) and, on average, female students graduate at slightly higher rates than males (Swanson, 2004). Although chronic absenteeism is often concentrated in high school, the problem is also relevant in U.S. elementary schools (Balfanz & Byrnes, 2012; Gase, Butler, & Kuo, 2015). One in 10 elementary students is chronically absent, which totals an estimated 2.4 million students per year.

Eleven percent of youth reported skipping school within the past 30 days according to estimates from two nationally representative surveys (Henry, 2007; Vaughn, Maynard, Salas-Wright, Perron, & Abdon, 2013). With an estimated 17 million students enrolled in grades 9–12 (Davis & Bauman, 2013), approximately 2 million students skip school at least once in a given month, with some cities reporting truancy rates as high as 30% (Garry, 1996; Spaethe, 2000; Teasley, 2004; Trujillo, 2006).

**Consequences of Chronic Absenteeism**

For students who are chronically absent from school, the prospects are dire. As early as elementary school, students with a higher proportion of unexcused absences to total absences are at academic risk, especially in math achievement (Gottfried, 2009). Children who were chronically absent in the 1st grade are far less likely to read at 3rd grade and children who fall behind the basic reading standard by the end of 3rd grade are four times more likely to drop out of high school compared to the children with higher reading proficiency (Hernandez, 2011). Children with attendance problems in earlier grades often lack the resources to make up for what they have missed at school, which leads to educational inequity, as the marginal benefit of education is larger for children who are already disadvantaged in multiple socio-economic dimensions (Downey, Von Hippel, & Broh, 2004; Ready, 2010). Using a nationally representative dataset of kindergarten students from the 2010–2011 academic year, Gottfried (2014) found that chronic absenteeism reduced math and reading achievement outcomes, educational engagement, and social engagement.

Chronic absenteeism is also a risk factor for dropping out of school. Approximately 1.23 million students fail to graduate from high school each year and students of color are over-represented among non-graduates (EPE, 2007a). Graduation rates for the state of Minnesota as a whole (2003–2004) revealed a higher graduation rate compared to the national average (78.7% versus 69.9%, respectively); however, compared to the national average, students in Minnesota who identify as Black non-Hispanic, American Indian, and Asian/Pacific Islander graduate at lower rates (42.7%, 41.3%, and 64.4%) when compared to the national average (53.4%, 49.3%, and 80.2%; EPE, 2007b).

Truancy is also a powerful predictor of delinquent behavior. Attendance problems among 1st grade students predicted violent behavior 25 years later (Farrington, 2003), and truancy between the ages of 12–14 years predicted chronic offending throughout the life

course (Farrington & West, 1993; Nagin, Farrington, & Moffitt, 1995). Frequent truancy also predicted criminal onset (Zara & Farrington, 2010). Using data from a nationally representative sample, Katsiyannis, Thompson, Barrett, and Kingree (2013) found that the likelihood of being charged with a violent crime in adulthood increased as attendance and grade completion decreased.

The negative effect of chronic absenteeism on later life outcomes includes occupational outcomes (Alexander, Entwisle, & Horsey, 1997; Rocque, Jennings, Piquero, Ozkan, & Farrington, 2017), anti-social behavior (Garry, 1996), substance abuse (Hallfors et al., 2002; Henry & Huizinga, 2007; Vaughn et al., 2013) and criminal justice system involvement (Zara & Farrington, 2010). The unemployment rate in 2013 among individuals without a high school diploma was nearly three times the rate of those with a Bachelor's degree or higher (11% versus 4%; U.S. Department of Labor, 2014). The national cost of low wages, taxes, and productivity attributable to high school dropout totals more than $200 billion annually, not including the criminal justice costs attributable to dropping out. Truancy often leads to delinquency, as well as adult criminal activity (Zara & Farrington, 2010; Zhang, Katsiyannis, Barrett, & Willson, 2007). Over 40% of the total incarcerated population did not graduate high school, compared to 18% of the general population (Harlow, 2003).

**Costs of Chronic Absenteeism**

Chronic absenteeism has significant costs to the individual. Because students who are chronically absent are more likely to perform poorly in school and to drop out, they have lower earning potential over their lifetimes (Attwood & Croll, 2006; Garry, 1996). Students with early dropout rates tend to have fewer job opportunities and be unemployed or struggle finding decent employment (Trujillo, 2006). The median weekly earnings in 2013 for an individual without a high school diploma were $472, compared to $651 for individuals with a diploma (U.S. Department of Labor, 2014). Over their lifetimes, high school dropouts will

earn $375,000 less than high school graduates and approximately $1 million less than college

graduates (Center for Labor Market Studies, 2009). If students who dropped out of the Class

of 2011 had graduated, the national economy would have benefitted from nearly $154 billion

in additional income over the course of their lifetimes (Alliance for Excellent Education,

2011). In 2011 in Minnesota, there were an estimated 14,311 young people who did not

graduate on time, and these students would have an additional lifetime income of over $2

billion if they had graduated (Alliance for Excellent Education, 2011).

The substantial costs to communities associated with truancy and dropping out of

school include higher rates of criminal activity, failure of citizens to productively contribute

to the community, and higher government spending for social services (Baker, Sigmon, &

Nugent, 2001). In 2001, 40% of 16–24 year olds who had dropped out of high school

received some form of government assistance (Burrus & Roberts, 2012). In addition, each

individual who drops out of high school and subsequently engages in drug use or criminal

activity costs society between $1.7 and $2.3 million over his or her lifespan (Bridgeland et

al., 2006). A one-year cohort of students who drop out of school costs the nation more than

$240 billion over their lifetimes (Dembo & Gulledge, 2009; Schoeneberger, 2011). Efforts

that target school absenteeism have the potential to increase individual educational

achievement and employment opportunities, while reducing substantial economic burden on

communities (Trujillo, 2006).

**Responses to Chronic Absenteeism**

Although the literature on chronic absenteeism is voluminous, it is mostly focuses on

risk factors and consequences of absenteeism rather than on rigorous tests of interventions.

Findings of two recent meta-analyses demonstrated that truancy interventions modestly

improve attendance, academic achievement, and school completion (Klima, Miller, &

Nunlist, 2009; Maynard, McCrea, Pigott, & Kelly, 2013). Unfortunately, however, after

intervention, the mean rates of absenteeism remained above 15 days per year, an often-used definition of chronic absenteeism. During the 1990s and 2000s, jurisdictions turned to juvenile courts as a solution when intervention programs did not work. Between 1995 and 2007, the number of court-petitioned truancy cases processed by juvenile courts increased from 34,000 cases to 61,000, far outpacing the growth in population of school-aged children (Maynard et al., 2013). After 2007, the number of cases fell to 54,000 (Hockenberry & Puzancherra, 2018)

Juvenile justice-based strategies focus on reducing unexcused absences, which typically represent fewer than half of all absences (Broward County Public Schools, 2017; Gottfried, 2009). Most states have laws that allow or, in some cases, mandate schools to refer students with a certain number of unexcused absences to juvenile court. Historically, schools have followed these mandates by sending out a series of warning notices to parents and then issuing a petition to court. However, over the past two decades, evidence emerged to show that although adolescent status offenses do not typically result in adult criminal behavior, formal processing by the juvenile justice system for non-criminal offenses does, on average, increase criminal involvement (Huizinga, Schumann, Ehret, & Elliott, 2004; Moffitt & Caspi, 2001). In response to this new evidence, many school districts adopted diversion strategies to reduce truancy petitions to juvenile court (Development Services Group, 2017). Moving away from the pre-2000s correctional models that involved the court system as the primary response to absenteeism, the most common type of truancy intervention is now a court diversion program. The most widely-implemented diversion model has three steps: 1) a group meeting with truant students and parents, 2) development of a formal attendance-improvement plan with the student and family that includes, as appropriate, referrals to social service agencies, and 3) a petition to juvenile court. Referral to the next step occurs only if attendance does not improve after the prior step. A random sample of 90 U.S. school districts

from the National Center for Education database revealed that 63% of the sampled districts were implementing court-diversion programs (Carpenter & McNeely, 2018).

A pilot randomized controlled trial of this three-step model for students ages 10–16 in Queensland Australia suggested that the model has promise (Mazerolle, Antrobus, Bennett, & Eggins, 2017). Among the 51 students randomly assigned to the intervention, absenteeism declined, on average, from 27 days in the three terms prior to program implementation to 20 days in the three terms following implementation, compared to a decline from 25 days to 23 days for the same-sized comparison group. The authors called for a larger evaluation to examine long-term effects on attendance and whether program effects differ across subgroups.

**Diversion programs.** Diversion programs were first promoted in the 1970s as a way to hold young people accountable without formal court involvement (Development Services Group, 2017). The underlying theory was first articulated by Becker (1963) and later termed labelling theory (Akers & Sellers, 2000). According to labelling theory, the stigma created by involvement in the juvenile justice system causes a young person to identify with the role of delinquent and enact even more delinquent acts. However, empirical studies did not support the idea that labelling by the juvenile justice system increased delinquency or status offenses (Akers & Sellers, 2000), and interest in diversion programs waned after the 1980s (Bishop & Decker, 2006). In the last decade, however, diversion has re-emerged as an intervention of choice. This shift was propelled by several factors, including research findings that harsh punishment did not deter delinquency or status offenses among adolescents (Huizinga et al., 2004; McAra & McVie, 2016) and that zero-tolerance policies exacerbated racial disparities in referrals to the juvenile justice system (Skiba et al., 2014).

The theory and empirical evidence underlying diversion strategies all point to what not to do but they do not provide a clear model of behavior change to guide what should be

done. As a result, diversion programs vary widely in their aims and strategies. Some programs aim to reduce or avert formal system involvement entirely, with the explicit intent to not replace it with any alternative strategies. This is diversion in its truest sense. A more common diversion model replaces involvement in the juvenile justice system with involvement in other community services to support the young person's rehabilitation and development (Frazier & Cochran, 1986).

**Evidence for diversion programs.** Evidence for the effectiveness of court diversion programs is mixed. Fain, Turner and Greathouse's (2014) evaluation of the Abolishing Chronic Truancy (ACT) program in Los Angeles included 3,144 referred students who had fewer absences in the six months following program referral compared to the number of absences accrued during six months preceding the referral (9.2 days versus 16.1 days, respectively). A meta-analysis of 28 randomized controlled trials of diversion programs for non-criminal offenses, such as truancy, found that only restorative justice programs with active involvement of researchers reduced the problem behavior (Schwalbe, Gearing, MacKenzie, Brewer, & Ibrahim, 2012). In contrast, a meta-analysis by Wilson and Hoge (2012) found that diversion programs reduced recidivism. This meta-analysis expanded the study inclusion criteria to include any evaluation of a diversion program that employed a comparison group, resulting in a sample of 78 programs. As with Schwalbe et al. (2012), the researchers documented that active involvement of researchers in program implementation led to more positive results. The authors of one small randomized trial evaluating truancy diversion suggested that diversion is a promising approach to reducing absenteeism (Mazerolle et al., 2017). An important limitation of prior research is poor documentation of the standard practice in the comparison group. Since state education laws mandate that schools intervene to prevent truancy, rarely do the comparison groups receive no intervention.

Despite some documented program successes, there is little evidence that there has been a significant reduction in the prevalence of truancy or absenteeism. Using nationally representative data, Maynard et al. (2017) reported that truancy rates among middle and high school students remained constant between 2002 and 2014. Researchers have also cited that there is a strikingly low number of evaluations of truancy programs and that many current studies suffer from low sample sizes and weak designs that lack legitimate counterfactual comparison groups (Dembo & Gulledge, 2009; Maynard et al., 2013; Sutphen, Ford, & Flaherty, 2010).

Key to interpreting the effectiveness of diversion programs is understanding the comparison condition. Because most schools are mandated by state law to respond to truancy, and because most teachers and staff truly care about keeping students in school, students in the comparison groups typically receive both documented interventions (e.g., direct referral to juvenile court) and undocumented interventions (e.g., teacher supports such as socio-emotional engagement or practices such as giving zero credit for work missed or failing a course after a certain number of unexcused absences in the course, regardless of course performance). This is true even for highly-controlled randomized designs, in which non-experimental interventions received by the intervention and/or the comparison group are rarely documented.

The authors of three meta-analyses on the effects of truancy interventions and dropout prevention programs on school attendance found few studies of sufficient rigor to include in the analyses (Klima et al., 2009; Maynard et al., 2012; Tanner-Smith & Wilson, 2013). As Klima et al. (2009) wrote: "Overall, the state of knowledge about the effectiveness of truancy and dropout programs is lacking. Most programs are not evaluated, and those that are evaluated generally use research designs and methodologies that do not permit us to draw conclusions about causality" (p. 5). The authors identified five methodological problems

characteristic of most research: 1) lack of equivalent comparison groups, 2) high attrition rates in the program, 3) small sample sizes, 4) lack of long-term follow-up, and 5) lack of determination of how program effects differ across racial/ethnic and socioeconomic groups (Klima et al., 2009; Maynard et al., 2013). Most scarce were rigorous studies of interventions that involved the juvenile justice system.

For the research and practice community, these meta-analyses left more questions than answers. The conclusion was that rigorous evaluations are desperately needed. Three specific questions remain unanswered. First, which types of programs are most effective at improving attendance? Second, are truancy intervention programs more effective for boys or girls or for students from different racial/ethnic, immigrant, and socioeconomic backgrounds? Given the enormous disparities in school attendance and academic outcomes for students of color (Editorial Projects in Education [EPE], 2007a), answering this question is essential. Third, what are the longer-term effects of truancy intervention programs? Few of the studies in the previously-cited meta-analyses followed students for longer than one year, which significantly limits the ability to measure improvement over time.

## Purpose of the Study

Our study addressed the methodological concerns cited by Klima et al. (2009) and Maynard et al. (2013). We conducted a causal study using a rigorous quasi-experimental design to determine the effects of two truancy intervention programs in Ramsey County, Minnesota (which includes St. Paul) on student attendance. Developed and run by the Ramsey County Attorney's Office, the Truancy Intervention Program (TIP) targets adolescents aged 12–17 years old and the Family Truancy Intervention Program (FTIP) targets children aged 5–11 years. To estimate a causal relationship, we implemented matched sampling analysis using a matched comparison group of adolescents from neighboring school districts who were not involved with TIP or FTIP. We also implemented matched sampling

analysis using a comparison group of students from within the same school districts who were not referred to TIP or FTIP. The study goal was to conduct a longitudinal individual-level outcome study of students in elementary, middle, and high school who received TIP and FTIP services and expand the knowledge base of truancy interventions to reduce absenteeism. Thus, the question answered in this study is whether TIP and FTIP work better than the standard practice of direct referral to juvenile court in a population exposed to a relatively rich array of other programs and strategies to improve attendance.

**Research Questions**

We evaluated the common three-step truancy diversion model as implemented in a large metropolitan county in a Midwestern U.S. state. We addressed each of the limitations of existing research identified in the previously-cited meta-analyses by using a panel dataset containing 11 years of linked, individual-level administrative data from a variety of local and state agencies and difference-in-differences methods with matched samples. Because randomization was not possible in our context, we conducted extensive sensitivity analyses to test for selection bias. The dataset had low attrition and allowed for the examination of long-term outcomes among multiple subgroups.

The research questions were:

**Research question 1.** To what extent does referral to the Truancy Intervention Program (TIP) improve overall school attendance among middle and high school students compared to similarly truant students who do not receive TIP?

**Research question 2.** To what extent does referral to the Family Truancy Intervention Program (FTIP) improve overall school attendance among elementary school students compared to students with similar absenteeism who do not receive FTIP?

**Research question 3.** Are there racial or ethnic disparities in the rates of referral to TIP or FTIP?

**Description of the Ramsey County Truancy Intervention Programs**

Minnesota State Law requires that all children between the ages 7–17, and ages 5 and 6 if they are enrolled, attend school every day unless lawfully excused by the school. If the child is age 12 or older, the failure to attend school is truancy. In Minnesota, for children ages 12–17, the parent(s) or student can be petitioned to court for truancy. If a child is under age 12, the failure to attend school is presumed to be educational neglect committed by the child's parents or guardians. For children under age 12, the parent(s) can be petitioned to court for educational neglect. Both educational neglect and truancy are grounds for filing a Child in Need of Protection or Services (CHIPS) petition in juvenile court. Any school in Ramsey County may refer a child to TIP or FTIP.

**Overview of the Truancy Intervention Program (TIP).** TIP involves a three-step process providing progressively intensive interventions to improve a student's attendance. Step 1 begins when the school refers students with five or more unexcused absences to TIP. The student and parent(s) are required by the Ramsey County Attorney's Office to attend a meeting at the school. At this meeting, an assistant County attorney explains Minnesota's Compulsory Attendance Law (i.e., the expectation for full attendance) and the legal and social consequences of poor school attendance, as well as the TIP process. Students and families are warned that they will be referred to Step 2 of TIP, or ultimately petitioned to court (Step 3), if the child's attendance does not improve.

Students who fail to improve their attendance complete an in-school contract with school personnel and their parent(s). If, after this contract, the student continues to be truant, the student is referred to Step 2 of the program: a School Attendance Review Team (SART) hearing. At this step, school administrators, school social workers or counselors, an assistant County attorney, a youth engagement worker, and the parent(s) and student meet to create a plan for successful school attendance. The plan is formalized into a written attendance

contract, which is signed by all of the SART hearing participants. Referrals to social service agencies, chemical dependency evaluations, mental health evaluations, and individual or family counseling are often included as terms of the contract to assist the family in dealing with the underlying causes of poor attendance. Students are required by the SART contract to attend each of their classes, every day and on time, unless they have a lawful excuse. Parents are also required, as a part of the SART plan, to take action to ensure their child's successful school attendance. Those actions can include such things as waking their child on time for school, making sure their child has reliable transportation to school, and making sure that their family follows through with services that are required by the SART contract. A student may also be assigned a school monitor to check on daily attendance of the child and report the results to the SART team.

If attendance does not improve after the SART hearing, the process moves to the third and final step of TIP, the filing of a truancy petition in Juvenile Court and expedited hearing. Dispositions usually include a period of supervision and an order that the student follow the youth engagement worker's terms and conditions of probation and recommendations for services or programming. Students are also ordered by the court to attend school daily unless they have a lawful excuse that they properly communicate to their school. It is the goal of TIP, through the first two steps of the program, to avoid the filing of truancy petitions whenever possible.

To improve school attendance, the County Attorney's Office collaborates with school districts, community-based organizations, and the County's Community Corrections, Community Human Services Department, and Juvenile Court. All of these agencies work together to send a clear and consistent message that education, personal responsibility, and respect for the law are important values. These values are modeled by adults' commitment to

and personal investment in students' education. TIP is an intervention that is easy to access and provides continuous support of school and parent efforts to help students learn.

Since its inception in 1996, 35,836 students have been referred to TIP (25,371 since 2001 when we will begin following participants). In the 2012–2013 academic year, for example, 90 schools referred 1,562 students to the first step of the program. Of those, 603 students were referred to a SART hearing and 411 were petitioned to Juvenile Court. These numbers have been consistent for the past several years. The background of students referred to TIP in 2012–2013 was 53% male, 41% African American, 19% Asian, 18% Caucasian, 16% Hispanic, and 3% Native American. These students were disproportionately students of color compared to the population of Ramsey County.

**Overview of the Family Truancy Intervention Program (FTIP).** Started in 1999, the Family Truancy Prevention Program (FTIP) follows a three-step model similar to TIP. Step 1 consists of a one-on-one or small group meeting with the parent(s) of each child referred by the school for educational neglect. At this meeting, the parent(s) are left with three clear messages: 1) school attendance for elementary age children is mandatory and parents are responsible for ensuring their child's regular attendance; 2) attending school and receiving an education improves a child's quality of life; and 3) the County Attorney's Office will take legal action if the child's attendance does not improve.

After the informational meeting, attendance is closely monitored by school personnel. Children who continue to miss school without lawful excuse are referred to Step 2: a School Attendance Review Team (SART) hearing. A referral to Step 2 produces a simultaneous report of maltreatment to the Ramsey County Child Protective Services (CPS). A case worker completes an assessment with the family prior to the SART hearing. At the SART hearing, the school representative, child protection worker, assistant County attorney and the parent(s) discuss the reasons for the child's poor attendance. An attendance contract is created, linking

the family to services to address and eliminate the problems causing the child to be absent from school and committing the parents to improving their child's attendance.

After the SART hearing, school staff and the child protection worker monitor the parents' compliance with the attendance contract/plan. If the contract/plan fails (i.e., the child's attendance does not improve), a Child in Need of Protection or Services petition for educational neglect is filed with the Juvenile Court. Petitioning a parent to court for educational neglect is the third step of the FTIP process. Upon a finding of educational neglect by the Court, the family is ordered to accept child protection services from the Department of Human Services. The case remains open until the attendance problem has been abated, with regularly scheduled reviews in court at a minimum of every 90–180 days.

The three steps of the program do not begin anew each school year. This means, for example, a child can be referred to Step 1 of the program, the parent meeting, in 1st grade. If attendance again becomes a problem in 4th grade, the child can be referred to Step 2, the SART hearing, at that time. FTIP/TIP staff track each child referred and where the child is in the process until the child is 18 years old. This approach is consistently identified by the referring schools as both unique to Ramsey County and a key to successful intervention. Since its inception in 1999, over 13,000 students have been referred to FTIP (11,143 since 2001). The demographic characteristics of the students eligible for FTIP are as follows: 54% male, 53% African American, 16% Hispanic, 16% White, 10% Asian, and 5% Native American.

## Method

Here, we briefly described data and measures for the overall project. Later in the report, we provided more detail on the data and measures used to answer each research question.

**Data Sources**

Data for this project came from four sources: the Ramsey County Attorney's Office TIP and FTIP records, daily attendance records from school districts in Ramsey County, and Minnesota's Departments of Education (MDE) and Human Services (DHS). The latter two data sources were made available through Minnesota Linking Information for Kids (Minn-LInK).

We linked data from these multiple sources to create a panel dataset containing all students enrolled in a public school in the state between 2004 and 2015. The core dataset came from MDE, which maintains individual-level data on all students in public school, including charter and alternative schools. To this dataset, we linked TIP/FTIP program data provided by the Ramsey County Attorney's Office, information on child welfare involvement from DHS, and daily absenteeism data from five large public school districts in the county, resulting in longitudinal, individual-level data on 308,491 students.

**TIP/FTIP records.** The TIP and FTIP databases contained data on all participants since the programs started in 1996 and 1999, respectively, including dates of initial referral, dates at which students participated in each of the steps of TIP/FTIP (meeting with the county attorney, SART hearing, and referral to juvenile court), and student identifier.

**Minnesota Department of Education and Minnesota Department of Human Services.** The Minnesota Department of Education (MDE) data included information on students' attendance, special education status, primary disability status, English language learner status, school disruptions, eligibility for free/reduced price meals (an economic indicator proxy), and graduation and dropout. The MDE data also included demographic information and academic test score data. The Department of Human Services (DHS) data included information on allegations of child maltreatment and information on whether the student ever had an out-of-home placement.

MDE and DHS data were made available through data-sharing agreements between each state agency and Minnesota Linking Information for Kids, or Minn-LInK. Minn-LInK is an integrated, cross-system data project housed at the Center for Advanced Studies in Child Welfare at the University of Minnesota-Twin Cities' School of Social Work. Minn-LInK was developed in response to the recognition that some of the state's most vulnerable children and families were served in multiple systems, yet there was no method to form these broader pictures of multi-system involvement. Minn-LInK projects are developed and carried out with a cross-system perspective, linking longitudinal data from multiple systems to answer questions about the impacts of policies, programs, and practice on the well-being of children. Because individual researchers are not able to access identified state data, MDE and DHS data were linked with TIP/FTIP and school attendance data by Minn-LInK staff.

**Measures**

**Program participation.** A student was defined as having participated in the program if a student or parent(s) received a letter from the County Attorney's Office stating that they had more than five unexcused absences and were required to attend the parent meeting on a specific date (Step 1). To understand whether the program improved attendance for students whose parent(s) actually attended a parent meeting, we created a dichotomous indicator of whether the parent attended a parent meeting.

**School attendance.** The primary outcome variable was the student's annual attendance rate. This was defined as the proportion of days a student was enrolled in any public school in the state (called "membership days") that the student attended. This variable was provided by MDE. Even though program referral is based on unexcused absences, our outcome measure did not differentiate unexcused and excused absences. The rationale was that the goal of the program is to improve overall attendance, not to increase the proportion of absences that are excused.

As a secondary outcome we used the number of excused and unexcused days absent in the months following referral to TIP. These data were provided by the individual school districts in the intervention county as daily- or period-level excused and unexcused absences. In accordance with the school districts' policies, middle and high school students were considered absent for a full day if they had three or more full-period absences. Elementary school students were considered absent for a full day if they missed a half-day or more. Daily attendance data were not available for students in the comparison county.

**Demographic characteristics.** Race/ethnicity was measured as American Indian or Alaskan Native, Black (not of Hispanic Origin), White (not of Hispanic Origin), Hispanic, and Asian or Pacific Islander. Socioeconomic background was measured by eligibility for free or reduced lunch in each year the child was in school as well as homelessness in each year. Children who lacked a fixed, regular, and adequate nighttime residence were homeless, as defined by federal law (42 U.S.C. § 103(a) (1) (2) of P.L. 100-77 (McKinney-Vento Education for Children and Youth who are Homeless). Homelessness was an indicator variable that was summarized as: Doubled-up, Hotel/Motel, Sheltered, or Unsheltered. In addition, we explored whether program impact varied by whether the student was limited English proficient or speaks a primary language at home other than English. This information was obtained from the Home Language Questionnaire and signed by the parent. Additionally, the English Language Learners (ELL) students must complete the annual Test of Emerging Academic English to be considered eligible for state Limited English Proficiency (LEP) funding the following school year. All demographic variables except race/ethnicity were treated as time-varying covariates in the analysis.

**Variables for matching**. One aspect of the analysis involved matching with students from demographically-similar school districts that do not implement TIP/FTIP on potential confounders of the relationship between TIP/FTIP and the outcomes. The following school

characteristics were available in or could be calculated from the MDE dataset: proportion of students eligible for reduced or free lunch, proportion homeless, proportion English language learners, proportion who speak a language other than English at home, and proportion who score at basic and advanced levels on MCA tests in math, reading, and science.

In addition, matching was based on the following individual-level characteristics available from the MDE dataset: race/ethnicity, age, grade and gender, student attendance in current year and prior years, student scores on the Minnesota Comprehensive Assessment (MCA) test in math, reading, and science for all grade levels the test is given, history of suspensions and expulsions, history of school transfers, homelessness, English language learner status, migrant status, and special education status.

Because average daily attendance in the year of referral to TIP could include a program effect and thereby bias estimated effects towards zero, we also matched or checked the balance of the matched samples on three alternative measures of attendance: attendance in the year before referral, the change in attendance between the prior year and the year of referral, and three-year attendance trajectories prior to the year of referral, which were created by performing latent class analysis (results not shown) using Mplus 8.1 software (Muthén, & Muthén, 2017).

**Analytic Strategy**

We analyzed program effects for the years 2006 to 2010. We chose 2006 as the lower bound in order to have attendance information in the two years prior to program referral. We chose 2009 as the upper bound because the neighboring county from which we drew the primary comparison group implemented its own formal multi-step diversion program in 2010. This upper bound also allowed us to examine the attendance trajectory for up to four years after program referral.

A concern for identifying causal effects in non-equivalent group designs is bias from differential selection into treatment and control conditions. Because schools exercised discretion regarding whom they referred to the intervention, it is likely that the referred students differed systematically from the non-referred students on unobserved characteristics, such as achievement motivation. If these unobserved characteristics were related to school attendance, a naïve estimation of program effects would be biased. As an identification strategy, we employed matching and difference-in-difference methods, which have been shown to reduce any bias that remains after cross-sectional matching (Blundell & Costa Dias, 2000; Smith & Todd, 2005).

**Matching.** A common approach in social sciences is to rely on regression to adjust for pre-treatment differences between the treatment and control groups; but, regression is highly limited in its ability to correct for pre-treatment differences, even with sophisticated econometric procedures (LaLonde, 1986). The goal of matching is to reduce imbalance in the distribution of the pre-intervention covariates between the intervention and comparison group so that there are no systematic differences between the groups in their reaction to the intervention. Under the assumption that the imbalance on observed covariates between treatment and control groups is similar to the pattern of imbalance on unobserved covariates, matched sampling approximates the control group of a randomized experiment (Blundell & Costa Dias, 2000; Stuart, 2010; Stuart & Rubin, 2008).

We first limited the potential pool of students in the comparison group to the school districts within the similarly-situated adjacent metropolitan county that did not implement a similar program until 2010. Within the adjacent county, we selected school districts that had similar sociodemographic and academic characteristics as the school districts in the intervention county. We conducted nearest-neighbor matching based on the following district-level characteristics: 1) proportion of minority students, 2) standardized reading/math

test scores, 3) the total number of students, and 4) proportion of students with free-lunch status. Four school districts from Hennepin County were matched to the five districts in Ramsey County.

Matched sampling methods include optimal matching, full matching, nearest-neighbor propensity score matching, nearest-neighbor matching using the Mahalanobis metric within propensity score calipers, weighting by inverse propensity score, and coarsened exact matching (Guo & Fraser, 2010). Specific matched sampling methods have been shown to be superior in different contexts, but no method can determine ahead of time which matched sampling method is ideal for a given problem (Morgan & Winship, 2007). We attempted matched sampling methods until we identified a group of students who did not receive TIP/FTIP that was similar on all identified potential confounders to the non-TIP/FTIP group. Similarity (referred to in the literature as "balance") was measured by statistical tests of central tendency such as the $t$-test (groups that have $p > 0.05$ on all tests are considered to be similar), standardized differences (groups that have standardized differences $< 0.1$ are considered to be similar), and subjective assessment of quantile-quantile plots.

We created matched samples separately for the TIP and FTIP populations. For both populations, we first matched the school districts using district-level variables. The matched school districts then became the sample from which individual-level matches were drawn. Individual-level matching was done within grade and academic year strata, because attendance systematically declines across grades starting in middle school and because overall attendance was depressed during the recession in 2008. Within each grade-year strata, we first randomly pruned observations from the comparison group that had values outside of the area of common support on the key variables of race and free-lunch eligibility (Ho, Imai, King, & Stuart, 2007; King & Zeng, 2007). Within the reduced sample produced by pruning, we implemented nearest-neighbor matching with replacement based on the Mahalanobis

metric. Matching using the Mahalanobis metric can improve covariate balance relative to propensity score matching methods (King & Nielsen, 2016). Matching was conducted within each grade because attendance tends to systematically fluctuate or decline at certain grade levels. We also used exact block match on academic year in our matching model to control for year-specific shocks, but we pooled the matched sample for each grade across the 2006–2009 years in order to increase power for subsequent regression analyses. After matching, we checked whether all covariates of the matched sample had acceptable balance for the grade of referral.

After matching, we pooled all matched samples within the same grade to increase power and improve the clarity of results for subsequent regression analysis. Within each grade, we checked the balance on 15 student characteristics. This matching process was carried out separately for each measure of program participation (referral and attendance at parent meeting).

**Difference-in-differences estimation.** We applied difference-in-differences (DiD) models to the matched samples to further reduce bias. DiD models eliminate selection bias from time-invariant unobserved characteristics under the assumptions that: 1) the magnitude of the selection bias from time-invariant characteristics is constant over time, and 2) the trend over time in the outcome variable, in the absence of treatment, is the same for both treatment and comparison groups (the parallel slopes assumption). We estimated the following generalized DiD basic model:

$$Y_{it} = \alpha + \beta P_{it} + \boldsymbol{X_{it}}\boldsymbol{\delta} + \rho_t + \gamma_i + \varepsilon_{it} \qquad (1)$$

where $P_{it}$ is a binary indicator for pre- and post-program referral for student $i$ where we set t=0 (year of the FTIP referral) as the reference category. $\boldsymbol{X}_{it}$ and the vector of time-varying student characteristics, $\rho_t$, are grade dummies that capture time-trends, and $\gamma_i$ controls for the

time-constant unobserved individual level characteristics. $\varepsilon_{it}$ is an error term that is assumed to be i.i.d.

In addition to the baseline model, we estimated an alternative specification that employs a full set of leads and lags program dummies to examine dynamic program effect (Lovenheim, 2009; Reber, 2005; Sojourner, Mykerezi, & West 2014):

$$Y_{it} = \alpha + \sum_{k=-2}^{4} \beta_k \, 1[t - t^{referral} = k] + X_{it}\delta + \rho_t + \gamma_i + \varepsilon_{it} \quad (2)$$

where $X_{it}$ is the vector of time-varying student characteristics, $\rho_t$ is grade dummies that capture time trends, and $\gamma_i$ controls for the time-invariant unobserved individual-level characteristics. The term $1[t - t^{referral} = k]$ is an indicator that equals 1 if student $i$ is $k$ years relative to its program referral year in grade $t$ and 0 otherwise. The indicator variable is recoded as zero when k=0 for the treated group so that the interpretation of the program effect, $\beta_k$, can be made relative to the year of referral. We limited the analysis window to six years. These six years comprised two years prior and up to four years after the program referral in order to check the pre-intervention parallel trends assumption while keeping the loss of sample members due to censored data to a minimum. All analyses were conducted using Stata 14 software (StataCorp, 2015).

**Sensitivity analyses.** If the two foundational assumptions of the DiD models do not hold—that the magnitude of the selection bias from time-invariant characteristics is constant over time and that the trend over time in the outcome variable, in the absence of treatment, is the same for both treatment and comparison groups—the DiD estimator is still biased (Blundell & Costa Dias, 2009).

To test for this possibility, we examined the short-term effect of referral to the program using daily attendance data. The key advantages of using daily attendance data, rather than annual data, was that we could distinguish the timing of absences relative to the date of referral to TIP. A secondary advantage was that the absences were classified as

excused or unexcused, which enabled us to match based on unexcused absences—the actual criteria for program referral. However, because the matching occurred within school districts for which there were daily absenteeism data available, all students in the pool of non-referred students used to identify matches had the potential to be referred.

## Results

### Research Question 1: Does TIP Improve School Attendance?

**Program implementation.** The linked dataset contained 4,412 students in grades 7–10 who had been referred to TIP by the five school districts between 2006 and 2010. Of the referred students, 61% (n=2,679) had a parent attend the group parent meeting, 28% (n=1,219) had a SART hearing, and 17% (n=749) were eventually petitioned to juvenile court for truancy (61% of those referred to a SART hearing). The first two columns in Table 1 present the individual characteristics of students referred to TIP. The average daily attendance rate was 85%, the equivalent of missing 27 days of school in a full academic year. Three quarters (77%) were eligible for free lunch, and schools moves were frequent, with each student, on average, attending 1.8 schools in the year of referral. Almost half of referred students had been held back a grade (48%) and their average score on standardized reading and math tests was nearly one standard deviation below the mean. In addition, 11% of students had experienced an out-of-home placement, an indicator of family trauma.

A simple but difficult to estimate statistic was the proportion of students with five or more unexcused absences in a year who were referred to the program (having not been previously referred). In two of the five school districts, the daily absenteeism data that indicated whether absences were excused or unexcused was either not of sufficient quality or was not provided. However, we were able to identify students eligible for TIP in three of the five school districts. These three school districts account for 89% of all students in the five school districts who were referred to TIP. Approximately 22% of students with five or more

unexcused absences were referred to the program in these three districts. The proportion of eligible students referred in any given year ranged from 19–24% across schools.

Table 1 reports the individual characteristics for students who were eligible for the program (i.e., had five or more unexcused absences) in the three school districts in which eligibility could be estimated. The large majority (89%) of students referred to TIP or FTIP attended school in the three districts that provided daily attendance data. To examine selection bias in terms of who was referred to the program, we compared means on a range of characteristics among eligible students who were referred to FTIP or TIP and eligible students who were not referred to either FTIP or TIP. This comparison is shown in the right-hand columns in Table 1. Although due to large samples most mean differences were statistically significant, they were not substantively meaningful. There were a few noteworthy differences between groups, however. Among students eligible for TIP, those actually referred were more likely to be eligible for free or reduced lunch (80% vs. 71%), were more likely to have been retained one or more grades (49% vs. 41%) and had substantially lower standardized test scores, particularly in math (-.95 vs. -.77 s.d. units).

Table 1

*Mean differences between referred, eligible but not-referred, and not eligible students (averages across the grade),*

*AY2006–2010*

| | All Referred Students (from 5 districts) | | Eligible and Referred (ER) (from 3 districts) | | Eligible, Not-Referred (ENR) (from 3 districts) | | |
|---|---|---|---|---|---|---|---|
| | Number of Students | Mean (s.d.) | N | Mean (s.d.) | N | Mean (s.d.) | ER vs ENR |
| Attendance rate | 4412 | 0.85 (0.10) | 3947 | 0.84 (0.10) | 9132 | 0.86 (0.12) | *** |
| Number of schools | 4412 | 1.76 (0.76) | 3947 | 1.86 (0.86) | 9132 | 1.75 (0.90) | *** |
| Individualized Education Program | 4412 | 0.24 (0.41) | 3947 | 0.24 (0.42) | 9132 | 0.22 (0.41) | |
| Free-lunch eligible | 4412 | 0.77 (0.38) | 3947 | 0.80 (0.36) | 9132 | 0.71 (0.43) | *** |
| Female | 4412 | 0.48 (0.49) | 3947 | 0.47 (0.50) | 9132 | 0.47 (0.50) | |
| Grade | 4412 | 8.64 (0.80) | 3947 | 8.65 (0.86) | 9132 | 8.83 (0.96) | *** |
| Grade retention (current year) | 4412 | 0.07 (0.21) | 3947 | 0.08 (0.23) | 9132 | 0.06 (0.21) | *** |
| Grade retention (ever) | 4412 | 0.48 (0.49) | 3947 | 0.49 (0.49) | 9132 | 0.41 (0.41) | *** |
| SSIS (year) | 4412 | 0.02 (0.09) | 3947 | 0.02 (0.10) | 9132 | 0.01 (0.10) | |
| OHP (year) | 4412 | 0.11 (0.31) | 3947 | 0.06 (0.19) | 9132 | 0.04 (0.18) | *** |
| English language learner | 4412 | 0.38 (0.48) | 3947 | 0.40 (0.49) | 9132 | 0.36 (0.48) | *** |
| White | 4412 | 0.22 (0.41) | 3947 | 0.15 (0.36) | 9132 | 0.22 (0.41) | *** |
| Black | 4412 | 0.36 (0.48) | 3947 | 0.38 (0.48) | 9132 | 0.39 (0.49) | |
| Hispanic | 4412 | 0.15 (0.35) | 3947 | 0.15 (0.36) | 9132 | 0.13 (0.34) | ** |
| Asian | 4412 | 0.24 (0.42) | 3947 | 0.25 (0.43) | 9132 | 0.21 (0.41) | *** |
| American Indian | 4412 | 0.03 (0.17) | 3947 | 0.03 (0.17) | 9132 | 0.03 (0.17) | |
| Z-score for standardized math test | 2889 | -0.92 (0.91) | 2349 | -0.95 (0.92) | 4316 | -0.77 (0.99) | *** |
| Did not take standardized math test/no score | 4412 | 0.38 (0.25) | 3947 | 0.39 (0.30) | 9111 | 0.34 (0.32) | *** |
| Z-score for standardized reading test | 3923 | -0.89 (0.91) | 3353 | -0.93 (0.91) | 7231 | -0.80 (0.99) | *** |
| Did not take standardized reading test/no score | 4412 | 0.40 (0.29) | 3947 | 0.41 (0.32) | 9132 | 0.41 (0.36) | |

Note: Bonferroni significance test are presented as * $p<.10$; ** $p<.05$; *** $p<.01$

**Matching.** Table 2 presents aggregate K-12 student characteristics of the five school districts from Ramsey County and the four matched school districts from the adjacent Hennepin County. In 2010, the set of intervention districts and the set of comparison districts each had approximately 80,000 students enrolled in grades K-12. On most variables, the two sets of school districts were socio-demographically similar. Both the intervention and comparison school district sets had a large share of students who were eligible for free lunch (47%), and the majority of the student body was minority students (53% and 58%, respectively). Approximately one-quarter of students were English Language Learners (29% and 25%, respectively). The proportion of students who were Asian was noticeably higher in the intervention county (21% vs. 10%). This was likely due to the historical resettlement of Hmong refugees in the intervention county more so than in the comparison county.

Table 2

*Characteristics of the intervention and comparison school districts, 2010*

|  | Intervention Districts (n=5) | Comparison Districts (n=4) |
|---|---|---|
| Attendance rate | 0.93 | 0.92 |
| Mean number of school transfers per student per year | 1.72 | 1.64 |
| Proportion with an Individualized Education Program (IEP) | 0.17 | 0.18 |
| Proportion Limited English Proficiency (LEP) | 0.21 | 0.16 |
| Proportion eligible for free lunch | 0.47 | 0.47 |
| Proportion White | 0.47 | 0.42 |
| Proportion Black | 0.30 | 0.32 |
| Proportion Hispanic | 0.10 | 0.13 |
| Proportion American Indian/Native Alaskan | 0.02 | 0.03 |
| Proportion Asian | 0.21 | 0.10 |
| Proportion Female | 0.48 | 0.49 |
| Proportion speak language other than English at home | 0.29 | 0.25 |
| Number of students | 81,501 | 78,852 |

Individual-level matching was conducted by identifying matches for the intervention districts from the four matched districts from the neighboring county. Covariate balance after matching was assessed by comparing standardized differences in the mean values of observed individual-level characteristics. Standardized difference is calculated using $d = (\overline{X_1} - \overline{X_0})/\sqrt{s_1^2 + s_0^2/2}$ where $\overline{X_1}$ and $\overline{X_0}$ denote the sample mean of treated and comparison group and $s_1^2$ and $s_0^2$ denote the sample variance of two groups. The red dots in Figure 1 represent the standardized mean difference of variables between the intervention and comparison groups before matching for the evaluation of TIP. Red dots to the right of the center line indicate that the treated group had a higher mean than the comparison group before matching, and vice versa. The blue dots represent the standardized mean differences after matching. The darker dotted vertical line indicates the 0.1 standard deviation threshold, and the lighter dotted vertical line indicates the 0.2 standard deviation threshold. After matching, almost all of the standardized mean differences fell within the 0.2 standard deviation threshold recommended by Rosenbaum and Rubin (1985). We also obtained good covariate balance for the matched samples for the analysis of the program effects in the sample of students referred in grades 7–10 whose parents attended the parent meeting (results not shown).

The number of students in the intervention group in the analytic sample was smaller than the total number of students referred to TIP. This is largely due to the fact that we excluded students who did not have attendance data in the two years prior to the referral year and because the matching algorithm dropped students outside of the area of common support, where matches could not be found. Compared to the intervention students retained in the analysis, the excluded students were, on average, less likely to be eligible for free lunch but more likely to have experienced difficulties in school, including lower average daily attendance, lower standardized math scores, and greater numbers of school transfers. The

excluded students also were at least twice as likely to have ever had an out-of-home placement. This pattern held across all grades (results not shown).
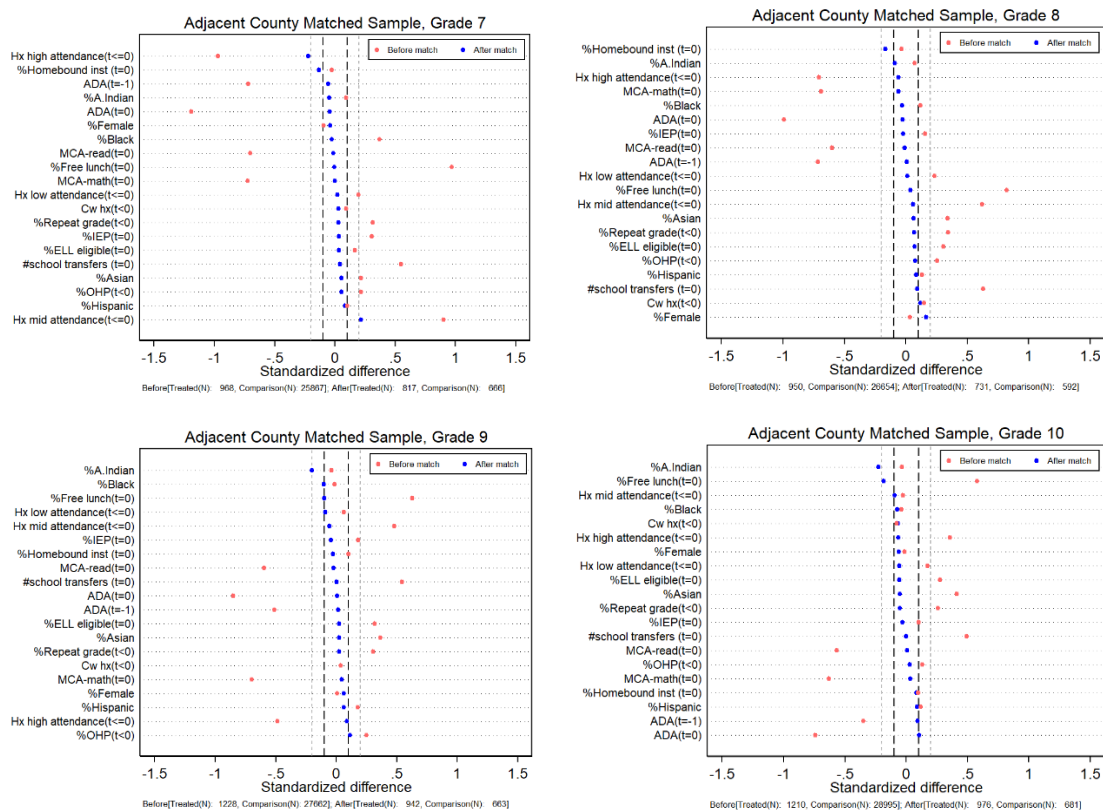


*Figure 1*. Standardized mean difference plots before and after creating matched samples from an adjacent county.

**Attendance trends in the matched samples.** Figures 2 and 3 present the trends in average daily attendance rates for the analyses of program effects among all students referred to the program and those whose parents attended the parent meeting. Students in all grades experienced a fairly sharp decline in attendance in the year prior to the intervention. This suggested that the program effectively targeted students with serious declines in attendance. In bivariate tests of difference, the intervention and matched comparison groups had statistically indistinguishable attendance trends in the years after the intervention—except for

students referred in 7th grade in the first year after the intervention, when students in the intervention group had lower average daily attendance than students in the comparison group.
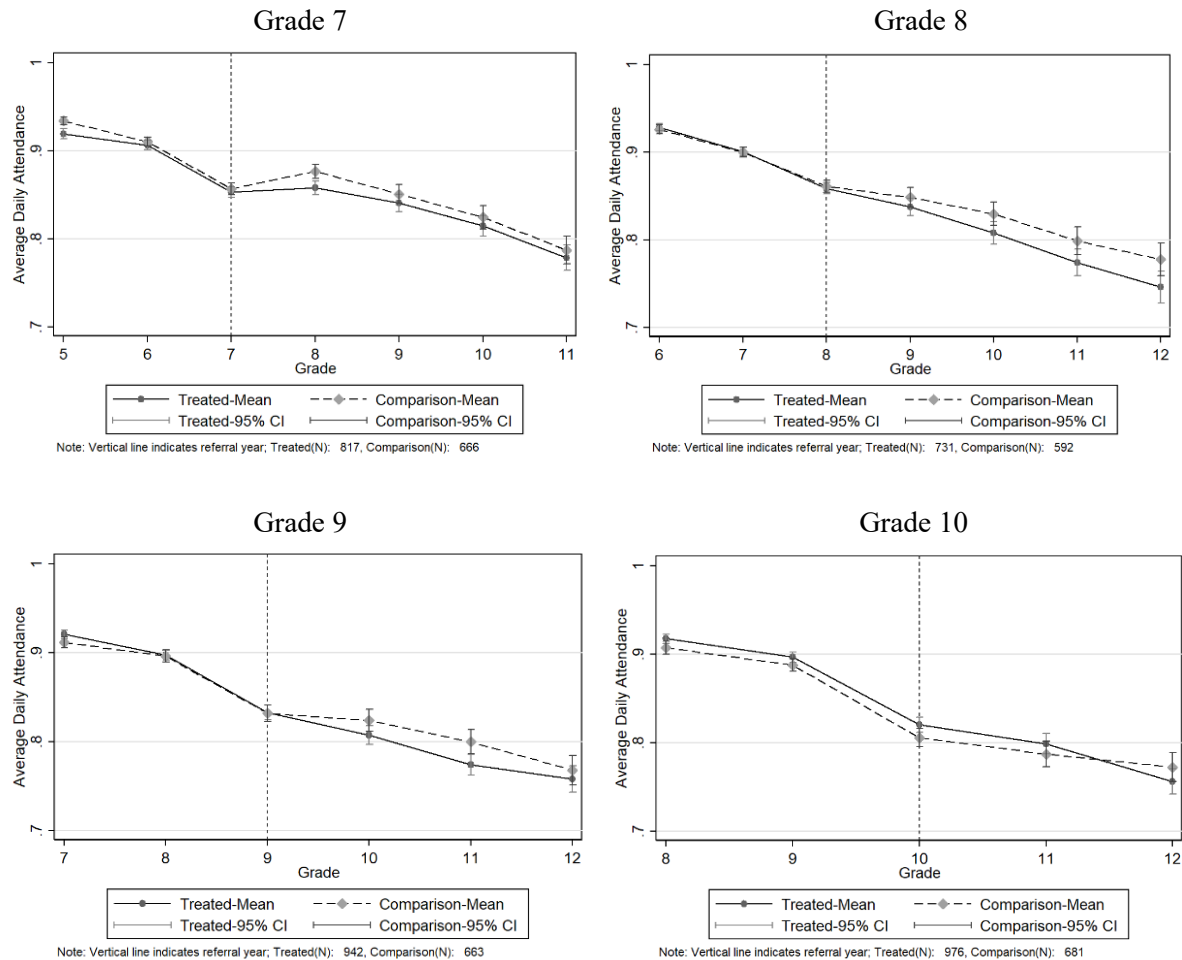


*Figure 2*. Trends in average daily attendance rates for the students referred to TIP and matched comparison groups, by grade.

Attendance trends for the sample of students whose parents attended the parent meeting and their matched comparison groups were not statistically significantly different except for 8th grade students three years after the intervention and 10th grade students two years after the intervention. The overall pattern is one of no differences over time between the intervention and comparison groups.

Grade 7

Grade 8

Grade 9

Grade 10
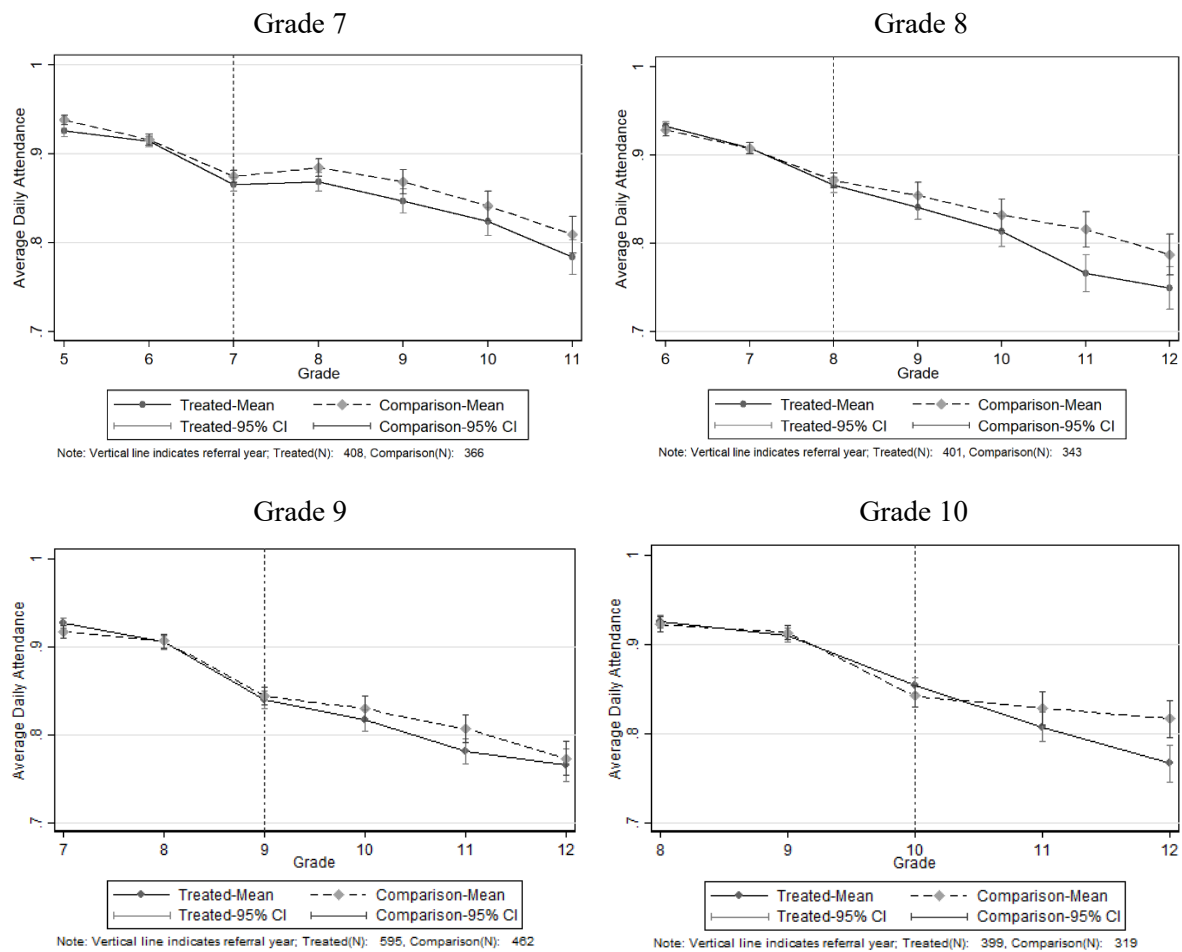
*Figure 3.* Trends in average daily attendance rates for the sample of students whose parents attended the parent meeting and matched comparison groups, by grade.

**Difference-in-differences models.** Table 3 presents the estimated coefficients for the dynamic DiD model that allowed treatment dummies to interact with year-specific time dummies. The coefficients for the two years prior to the intervention test the parallel trends assumption. A few coefficients were statistically significant in some of the grades but the effects sizes were small (approximately a 1-percentage-point difference in the attendance rate

Table 3

*Estimated OLS coefficients of the effect of referral to TIP and participation in the first step of TIP (parent meeting) from dynamic difference-in-differences models*

| | Grade 7 | | Grade 8 | | Grade 9 | | Grade 10 | |
|---|---|---|---|---|---|---|---|---|
| | Referral | Attended Parent Meeting | Referral | Attended Parent Meeting | Referral | Attended Parent Meeting | Referral | Attended Parent Meeting |
| before 2yr+ prior | -0.0125** | -0.00101 | 0.00226 | 0.00483 | 0.00377 | 0.00995 | -0.00189 | -0.00294 |
| | (0.00619) | (0.00676) | (0.00577) | (0.00680) | (0.00687) | (0.00767) | (0.00793) | (0.00808) |
| 1yr prior | 0.00605 | 0.0169*** | 0.00621 | 0.00836 | 0.00261 | 0.00279 | 0.00679 | 0.00494 |
| | (0.00526) | (0.00608) | (0.00493) | (0.00619) | (0.00632) | (0.00726) | (0.00713) | (0.00826) |
| 1yr after | -0.0107* | -0.00170 | -0.00971 | -0.0104 | -0.0185** | -0.00887 | -0.00520 | -0.0112 |
| | (0.00594) | (0.00728) | (0.00764) | (0.00991) | (0.00883) | (0.00937) | (0.00950) | (0.0120) |
| 2yrs after | -0.00345 | -0.00555 | -0.0104 | -0.0159 | -0.0286*** | -0.0248** | -0.0276** | -0.0583*** |
| | (0.00803) | (0.00980) | (0.00987) | (0.0131) | (0.0101) | (0.0118) | (0.0126) | (0.0156) |
| 3yrs after | 0.00121 | 0.000595 | -0.0132 | -0.0411*** | -0.0208* | -0.0114 | | |
| | (0.00945) | (0.0120) | (0.0116) | (0.0151) | (0.0123) | (0.0145) | | |
| 4 years after | -0.00181 | -0.0134 | -0.0286** | -0.0435** | | | | |
| | (0.0112) | (0.0146) | (0.0142) | (0.0178) | | | | |
| Grade-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 9901 | 5184 | 8564 | 4913 | 9210 | 6112 | 7823 | 6043 |
| R2 | 0.481 | 0.497 | 0.488 | 0.490 | 0.503 | 0.508 | 0.521 | 0.478 |

Note: Control variables include Gifted/Talented participation, Free-lunch eligibility, Disability status, Social service involvement, Mobility indicators; * $p<.10$; ** $p<.05$; *** $p<.01$

between intervention and comparison groups) and there was no consistent pattern that would suggest a systematic violation in the parallel trends assumption for either set of analyses.

The coefficients on the dummy variables for 1–4 years post treatment can be interpreted as the average treatment effect of the program on the average daily attendance rate in each year relative to the year of referral. For the analysis of the effect of program referral on attendance, there were small negative intervention effects in most years and most grades but only a few of the differences were statistically significant. The negative program effects were the most consistent and the strongest for students in 9th grade.

In the matched samples for the analyses of the effect of the program on those students whose parents attended the parent meeting, the intervention group in 8th grade had substantially lower attendance rates than the comparison group in the third and fourth year after the intervention, but not in the first two years. The intervention groups in 9th and 10th grade had lower average daily attendance in the second year after the intervention.

**Subgroup analyses.** We repeated the matching and analysis methods described above to create matched samples for the following subgroups: gender (male and female), race/ethnicity (American Indian/Alaskan Native, Asian, Black, Hispanic, and White), English Language Learners (vs. not), and consistent high attenders (vs. not). We pooled all students in each subgroup prior to matching rather than matching within grade due to the difficulty of finding good matches in the smaller samples. Yet, within most of the subgroups, it was difficult to obtain matched samples that were well-balanced on all covariates. We obtained adequate matched samples for students eligible for free lunch, females, males, and Black students. As hypothesized, the findings were similar each of these groups. We found no evidence of enhanced program effectiveness, as defined by referral to the program, for any particular group. We could not test for the effect of the program among those whose parent(s) attended the parent meeting due to the difficulty of obtaining good matches.

**Sensitivity analyses.** As noted above, we matched on the average daily attendance rate in the year of referral. This was necessary because students referred to TIP, on average, experienced a sharp decline in their attendance rate in the year of referral (Figures 2 and 3), and this decline could not be fully predicted by prior attendance or other pre-intervention characteristics of the students. Although this matching strategy ensured that the comparison group experienced an attendance decline similar to the referred students, it also meant that the average daily attendance in the year of referral was determined, in part, by attendance after referral to the program. This could bias inferences about program effects towards zero. If these assumptions do not hold or if unmeasured time-varying covariates are a source of selection bias, the difference-in-differences estimator is still biased (Blundell & Costa Dias, 2009).

For the sensitivity analysis, we pooled the data across all grades and years and then created strata for each month within the academic year. Within each month strata, we identified a matched comparison group for the TIP-referred students. We used the month prior to the month of first parent meeting because the administrative program data did not contain the exact date of program referral. Typically, the referral occurred approximately three weeks before the first parent meeting. Because most students were referred during the second semester of the academic year, we focused on students referred between February and May. We achieved good covariate balance on the same set of student characteristics presented in Tables 2 and 3.

Figure 4 presents the trend in the number of unexcused absences accrued in each month for the months before and after the month of referral to the program. The treatment and control groups had nearly identical trends in the months prior to referral, suggesting that the parallel trends assumption holds. Unexcused absenteeism peaked in the month before the parent meeting, the point in time when schools typically made the program referral. The

intervention and matched comparison groups had statistically indistinguishable attendance trends across all months. We repeated the analysis for excused absences and total absences and found the identical pattern (results not shown).
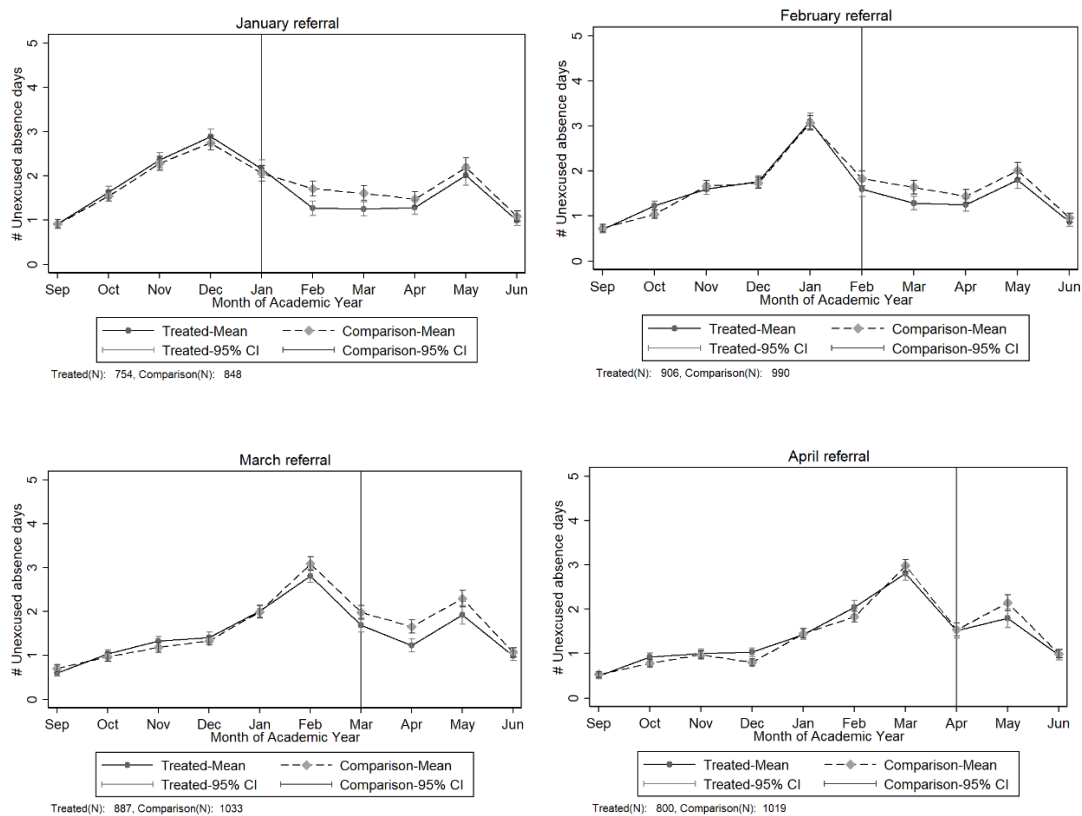


*Figure 4.* Trends in the average number of unexcused absence days in the months following the date the first parent meeting was scheduled: Intervention vs. matched comparison groups, by month of the parent meeting.

## Research Question 2: Does FTIP Improve School Attendance?

**Program implementation.** The linked dataset contained 1,285 students in grades 2–5 who had been referred to FTIP by the five school districts between 2006 and 2010. Of the referred students, 57% (n=736) had a parent attend the group parent meeting, 34% (n=431) had a SART hearing, and 17% (n=221) were eventually petitioned to family court for educational neglect (51% of those referred to a SART hearing).

The first column in Table 4 presents the individual characteristics of students referred to FTIP. The average daily attendance rate was 89%, the equivalent of missing 20 days of school in a full academic year. Most (88%) were eligible for free lunch, and schools moves were frequent, with each student, on average, attending 1.5 schools in the year of referral. About one-fifth of referred students had been held back a grade (21%) and their average score on standardized reading and math tests was nearly one standard deviation below the mean. In addition, 6% of students had experienced an out-of-home placement, an indicator of family trauma.

A simple but difficult to estimate statistic was the proportion of eligible students that were referred to FTIP. Students were eligible for referral to FTIP if they had received multiple tardies as well as if they had five or more unexcused full-day absences in a year. We had no data on tardies. In one of the five school districts, the daily absenteeism data that indicated whether absences were excused or unexcused was either not of sufficient quality or was not provided. However, we were able to identify students eligible for FTIP in four of the five school districts. Approximately 10% of students with five or more unexcused absences were referred to the program in these three districts. We estimated that between 8–12% of students with five or more unexcused absences were referred each year. We do not know what proportion of students who became eligible due to tardies alone were referred to FTIP.

Columns 2 and 3 in Table 4 report the individual characteristics for students who were eligible for the program based on the criteria of five full-day unexcused absences. Student data from one school for which eligibility could not be estimated is excluded. A comparison of means between these two columns served as a test of selection bias regarding which students the schools chose to refer. Most statistically significant differences were not substantively meaningful. It is noteworthy, however, that among students eligible for FTIP, those actually referred were more likely to be eligible for free or reduced lunch (92% vs.

69%), were more likely to be Black (59% vs. 39%), to have been retained one or more grades (23% vs. 14%) in school, and had substantially lower standardized test scores in both math (-1.16 vs. -0.68 s.d. units) and reading (-1.06 vs. -0.63 s.d. units).

**Individual-level matching.** Using the four matched school districts in the neighboring county as a potential comparison pool, we conducted individual-level matching for each grade (i.e., grade 2, 3, 4, and 5) based on observed individual characteristics measured in the grade of FTIP-referral.

Figure 5 shows the comparisons of standardized differences in means before and after matching for each grade level. The purpose of matching was to reduce these large standardized mean differences to be equal to or less than the acceptable threshold. Following Rosenbaum and Rubin (1985), we use a threshold of 0.2 standard deviations. The darker dotted vertical line indicates the 0.1 standard deviation threshold and the 0.2 standard deviation threshold is indicated by the gray dotted vertical line. The red dots represent the standardized mean differences before matching. It is demonstrated, for example, that before matching the free-lunch eligible student share of the referred group was approximately one standard deviation larger than for the pool of potential comparison cases for all grade-levels. As another example, before matching the mean attendance rate in the FTIP-referred group in the year of referral was one standard deviation less than the attendance rate in that same year of the pool of potential comparison cases. The blue dots represent the standardized mean differences after matching. Note that compared to the red dots, which are sparsely distributed over the standard deviation range, the blue dots are mostly contained within the 0.2 standard deviation threshold. This confirmed that we had good covariate balance in the matched samples.

Table 4

*Mean differences between FTIP-referred, eligible but not-referred, and not eligible students (averaged across grades), AY2006–2010*

| | Column 1 | | Column 2 | | Column 3 | | |
|---|---|---|---|---|---|---|---|
| | All Referred Students (from 5 districts) | | Eligible and Referred (ER) (from 4 districts) | | Eligible, Not-Referred (ENR) (from 4 districts) | | |
| | Number of Students | Mean (s.d.) | Number of Students | Mean (s.d.) | Number of Students | Mean (s.d.) | ER vs ENR |
| Attendance rate | 1285 | 0.89 (0.06) | 448 | 0.87 (0.07) | 4150 | 0.91 (0.07) | *** |
| Number of schools | 1285 | 1.54 (0.50) | 448 | 1.54 (0.55) | 4150 | 1.47 (0.53) | *** |
| Individualized Education Program | 1285 | 0.27 (0.42) | 448 | 0.28 (0.43) | 4150 | 0.19 (0.38) | *** |
| Free-lunch eligible | 1285 | 0.88 (0.28) | 448 | 0.92 (0.24) | 4150 | 0.69 (0.43) | *** |
| Female | 1285 | 0.44 (0.50) | 448 | 0.42 (0.49) | 4150 | 0.49 (0.50) | *** |
| Grade | 1285 | 3.37 (0.80) | 448 | 3.26 (0.96) | 4150 | 3.41 (0.89) | |
| Grade retention (current year) | 1285 | 0.03 (0.16) | 448 | 0.03 (0.15) | 4150 | 0.03 (0.15) | *** |
| Grade retention (ever) | 1285 | 0.21 (0.40) | 448 | 0.23 (0.42) | 4150 | 0.14 (0.35) | *** |
| SSIS (year) | 1285 | 0.05 (0.16) | 448 | 0.07 (0.20) | 4150 | 0.02 (0.10) | *** |
| OHP (year) | 1285 | 0.06 (0.17) | 448 | 0.08 (0.15) | 4150 | 0.02 (0.19) | *** |
| English language learner | 1285 | 0.21 (0.41) | 448 | 0.19 (0.39) | 4150 | 0.34 (0.47) | *** |
| White | 1285 | 0.18 (0.38) | 448 | 0.14 (0.34) | 4150 | 0.24 (0.43) | *** |
| Black | 1285 | 0.53 (0.50) | 448 | 0.59 (0.49) | 4150 | 0.39 (0.49) | *** |
| Hispanic | 1285 | 0.13 (0.34) | 448 | 0.10 (0.30) | 4150 | 0.18 (0.38) | *** |
| Asian | 1285 | 0.11 (0.32) | 448 | 0.10 (0.31) | 4150 | 0.17 (0.37) | *** |
| American Indian | 1285 | 0.04 (0.20) | 448 | 0.07 (0.25) | 4150 | 0.02 (0.15) | *** |
| Z-score for standardized math test | 1112 | -1.03 (0.97) | 347 | -1.16 (0.96) | 3254 | -0.68 (1.05) | *** |
| Did not take standardized math test/no score | 1285 | 0.39 (0.31) | 448 | 0.45 (0.35) | 4150 | 0.43 (0.36) | |
| Z-score for standardized reading test | 1131 | -0.95 (0.97) | 351 | -1.06 (0.94) | 3348 | -0.63 (1.06) | *** |
| Did not take standardized reading test/no score | 1285 | 0.35 (0.30) | 448 | 0.43 (0.36) | 4150 | 0.39 (0.35) | ** |

Note: Bonferroni significance tests are presented as * $p<.10$; ** $p<.05$; *** $p<.01$
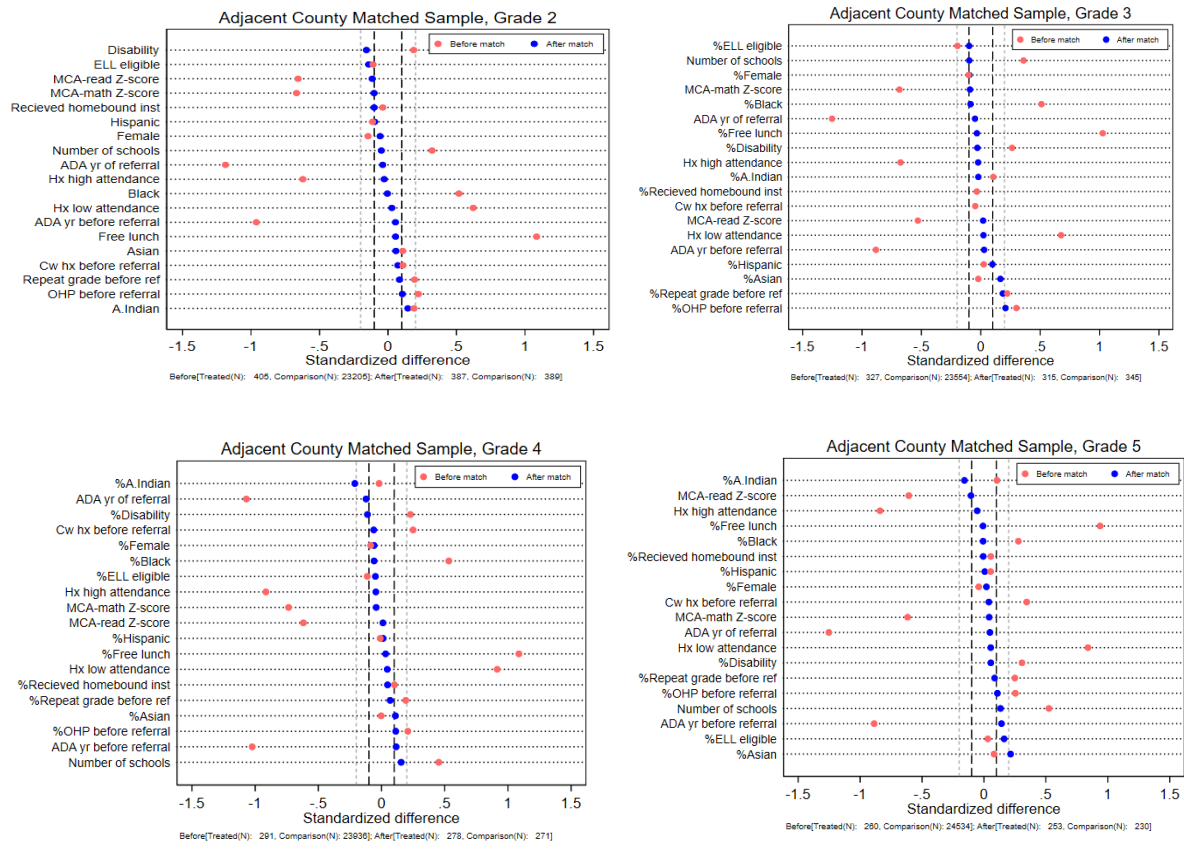
*Figure 5*. Standardized mean difference plots before and after student-level matching.

**Attendance trends in the matched samples.** Figure 6 presents the average

attendance trend before and after the year of FTIP referral for the intervention group and the

matched comparison sample. The intervention and the matched comparison groups showed

similar attendance trends that were statistically indistinguishable. The students referred to

FTIP experienced a decline in attendance in the year of referral (aka. Ashenfelter dip or

preprogram dip; see Ashenfelter & Card, 1985). Depending on whether the absence decline

was transitory or permanent, there is a risk of falsely inferring a positive program effect in the

absence of adequate counterfactuals (Imbens & Wooldridge, 2009). The large sample size of

the comparison pool in our data allowed us to match reasonably well on this decline in

attendance at the year of program referral when constructing the counterfactual comparison

group. The students who were referred to FTIP in earlier grades (grades 2 and 3 and partially

grade 4) showed a clear transitory bounce back in attendance in the year after referral, and so did the students in the comparison group.

Thus a naïve estimator such as a simple pre- and post-mean comparison in attendance without a legitimate counterfactual group would lead to a biased inference that attendance improved due to the intervention. Because the FTIP-referred students, by definition, were those belonging to the low-attendance category in the first place, it is possible that the low-attendance bounce back occurred naturally after a year of low attendance. The bounce back was not so obvious for students who were referred to FTIP in the 5th grade or within their matched comparison groups.
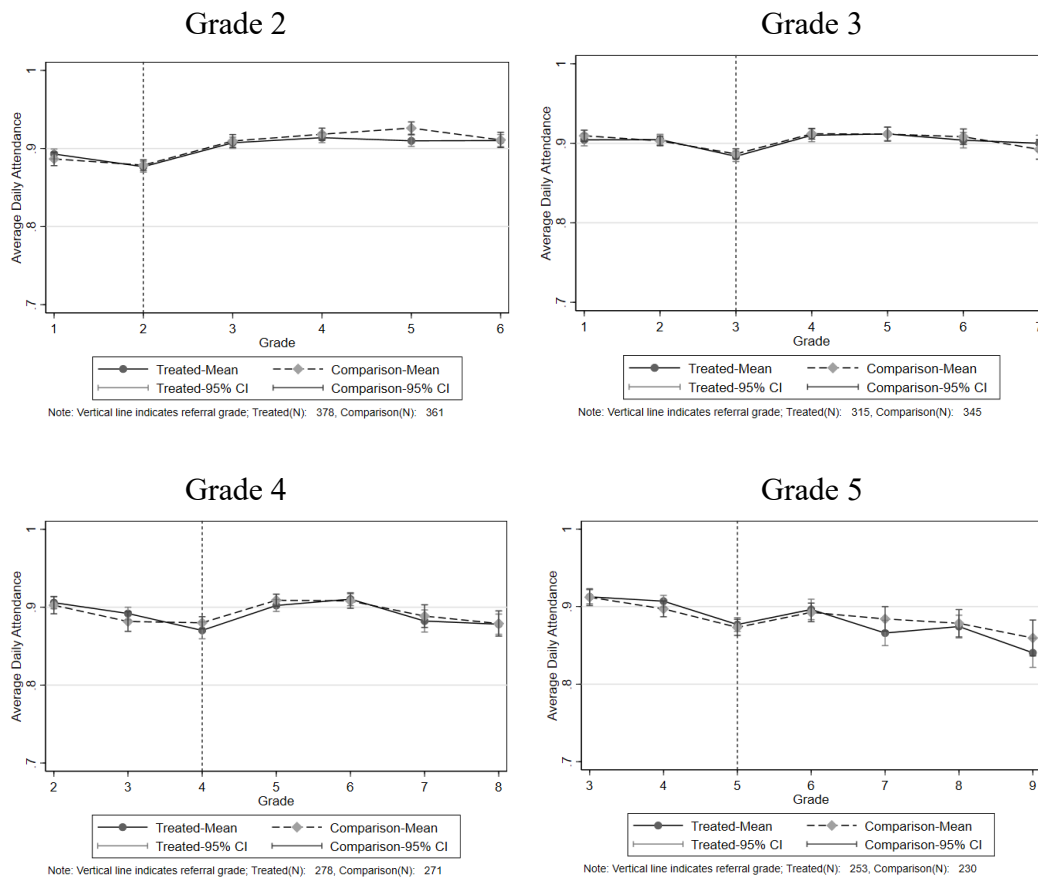


*Figure 6.* Trends in the average yearly attendance rate for the intervention and comparison groups.

**Difference-in-differences models.** Table 5 presents program effects based on the naïve OLS estimates, where the program effect was estimated by simply comparing pre- and post-FTIP referral attendance among the referred group. This yielded a positive and statistically significant estimate of the program effect for the students referred in grades 2 and 3, while a negative estimated effect was obtained for students referred in grade 5. These pre-post comparisons, which are widely used in the literature, showed inconsistent program effects by the grade of referral, unlike the DiD estimates below.

Table 5

*Naive OLS estimates of the effect of referral to FTIP on average daily attendance among students in grades 2-5, AY2006–2010*

|  | Grade at FTIP referral | | | |
|---|---|---|---|---|
|  | Grade 2 | Grade 3 | Grade 4 | Grade 5 |
|  | (1) | (2) | (3) | (4) |
| Treatment Effect | 0.026*** | 0.010*** | 0.006 | -0.028*** |
|  | (0.003) | (0.003) | (0.004) | (0.005) |
| R-squared | 0.101 | 0.050 | 0.067 | 0.075 |
| N | 2188 | 2114 | 1874 | 1695 |

Note: Dependent variable is yearly attendance rate. Standard errors adjusted for clustering at the school-level are presented in parentheses. Control variables include Gifted/Talented participation, free-lunch eligibility, disability status, child welfare involvement, and mobility. * $p<.1$, ** $p<.05$, *** $p<.01$

The estimates of the effect of FTIP on the attendance of referred students, based on the DiD model from equation (1), are reported in Table 6. In contrast to the estimates from the naïve OLS model, the DiD estimate of the program effect was close to zero and statistically insignificant at conventional levels. Thus we failed to reject the null hypothesis of zero effect for all grade levels. Although students referred to FTIP in 2nd grade increased

their attendance rate by approximately 1.3 percentage points (equivalent to 2.27 days) compared to the comparison group and students in 5th grade increased their attendance relative to their comparison group by 0.8 percentage points (equivalent to 1.4 days), these differences were not statistically significant.

The estimated coefficient on the pre-trend dummy suggests that for grades 3, 4 and 5, there was no statistically significant difference in the attendance trends between the intervention and comparison groups prior to the year of referral, confirming the parallel trends identifying assumption for the DiD estimator. The pre-trend coefficient for grade 2 was statistically significant at the 5% level. However, as is more clearly shown in Figure 6, the magnitude of the difference in attendance trends between the intervention and comparison groups was not large.

Table 6

*Estimated effects (OLS coefficients) of referral to FTIP on average daily attendance, from the difference-in-differences baseline model, grades 2–5*

| | Grade at FTIP referral | | | |
| --- | --- | --- | --- | --- |
| | Grade 2 (1) | Grade 3 (2) | Grade 4 (3) | Grade 5 (4) |
| Treatment Effect | 0.002 | 0.008 | 0.008 | -0.012 |
| (s.d.) | (0.005) | (0.005) | (0.008 ) | (0.011) |
| [95% CI] | [-0.008,0.013] | [-0.003,0.018] | [-0.007,0.024] | [-0.033,0.008] |
| Pre-trend dummy | 0.013** | -0.004 | 0.010 | 0.003 |
| (s.d.) | (0.006) | (0.005 ) | (0.007) | (0.008 ) |
| Grade-FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| R-squared | 0.518 | 0.414 | 0.436 | 0.474 |
| N | 4296 | 4419 | 3640 | 3211 |

Note: The dependent variable is the yearly attendance rate. Standard errors were adjusted for clustering at the student-level and are presented in parentheses. 95% confidence intervals are in brackets. Control variables

include Gifted/Talented participation, free-lunch eligibility, disability status, child welfare involvement, and mobility. * $p<.1$, ** $p<.05$, *** $p<.01$

Table 7 presents the estimated OLS coefficients from the dynamic DiD model, which allowed the intervention dummies to interact with year-specific time dummies. These results suggested that the FTIP-referred group had a slightly larger decline in attendance in the year of referral compared to the comparison group. Thus the parallel trends assumption does not hold, although the difference is small. The yearly null effect of the program is somewhat more evident in the graphical representation of the dynamic effects in Figure 7.

**Subgroup analyses.** Due to limited sample size that is required to achieve adequate matching quality, we were not able to conduct subgroup analyses for FTIP group.

**Sensitivity analyses.** We conducted robustness checks to address potential concerns that the analytic strategy could have led to biased estimates. We used slightly different methods for the FTIP evaluation than the TIP evaluation due to sample size considerations. Matching was conducted based on the number of unexcused absences in the month prior to the FTIP parent meeting and free-lunch eligibility status. Because stratifying the FTIP students by month of referral substantially reduced the sample size, we pooled the data across grade. To maximize the sample size, we also included students referred to FTIP in the 1st grade. Because attendance was measured at the monthly level rather than the yearly level for this analysis, we had no prior attendance data available for students in the 1st grade. As shown in Figure 8, the month of referral was not uniformly distributed throughout the year. Most students were referred during the second semester of the academic year.

Table 7

*Estimated coefficients from the difference-in-differences dynamic OLS regression of the*

*effects of referral to FTIP on average daily attendance, grades 2–5*

|  | Grade at FTIP referral | | | |
| --- | --- | --- | --- | --- |
|  | Grade 2 | Grade 3 | Grade 4 | Grade 5 |
|  | (1) | (2) | (3) | (4) |
| 2+ years prior | – | -0.004 | 0.012 | -0.006 |
|  | – | (0.006) | (0.009) | (0.009) |
| 1 year prior | 0.013** | 0.009* | 0.022** | 0.009 |
|  | (0.006) | (0.005) | (0.009) | (0.008) |
| 1 year after | 0.006 | 0.010 | 0.001 | -0.004 |
|  | (0.006) | (0.006) | (0.009) | (0.011) |
| 2 years after | 0.000 | 0.004 | 0.014 | -0.020 |
|  | (0.006) | (0.007) | (0.008) | (0.013) |
| 3 years after | -0.006 | 0.005 | 0.013 | -0.008 |
|  | (0.007) | (0.008) | (0.009) | (0.012) |
| 4+ years after | 0.008 | 0.009 | 0.011 | -0.019 |
|  | 0.007 | (0.009) | (0.012) | (0.016) |
| Grade-FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| R-squared | 0.519 | 0.415 | 0.473 | 0.475 |
| N | 4296 | 4419 | 4219 | 3211 |

Note: Dependent variable is yearly attendance rate. Standard errors adjusted for clustering at the student-level are presented in parentheses. Control variables include Gifted/Talented participation, free-lunch eligibility, disability status, child welfare involvement, and mobility. * $p<.1$, ** $p<.05$, *** $p<.01$

## Grade 2

## Grade 3
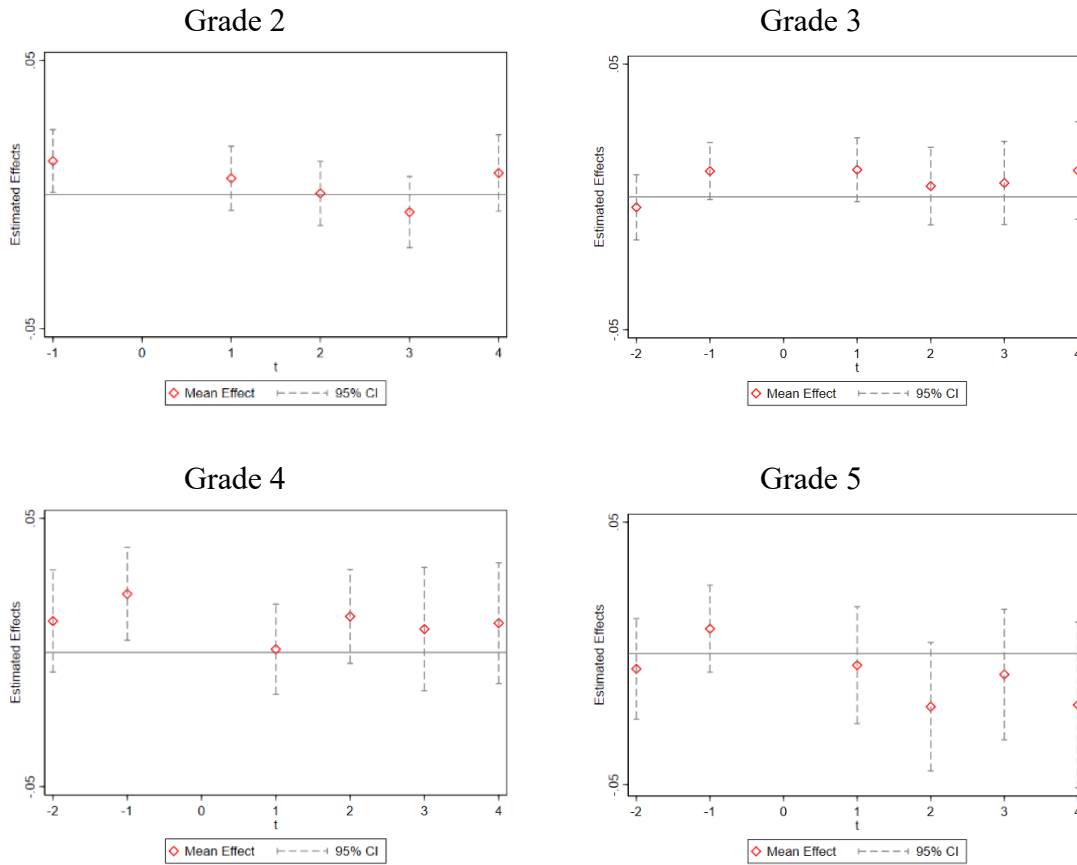
## Grade 4

## Grade 5

*Figure 7*. Dynamic difference-in differences program effect estimates.

Consequently, for the short-term analysis, we present results for students who were referred between February and May.
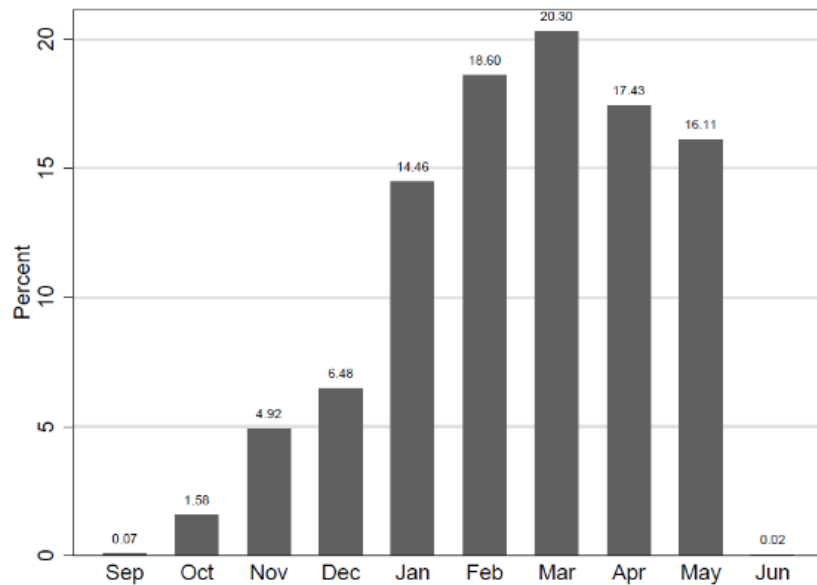
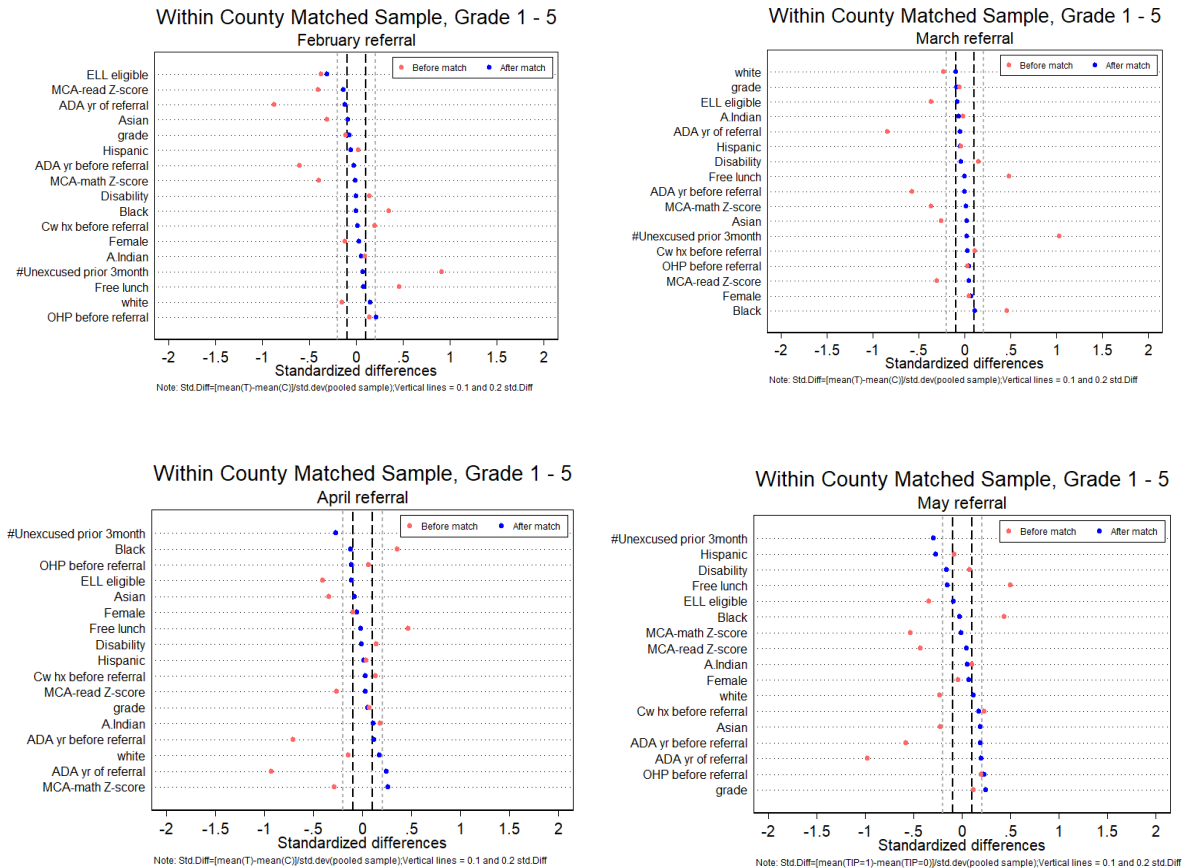*Figure 8*. Distribution of month of referral to FTIP.

*Figure 9*. Standardized mean difference plots before and after individual-level matching.

Figure 9 shows the covariate balance before and after matching on the number of monthly unexcused absences in the month before referral and other socio-demographic variables. Similar to the previous results that examined the effect on the yearly attendance rate up to four years after the referral, in the average absence trends as shown in the Figures 10 and 11, we observed no statistically significant differences between the intervention and comparison groups in the average number of unexcused absences (Figure 10) or excused absences (Figure 11) during the months after FTIP referral. The spike in unexcused absences that occurred right before program referral was transitory for both groups. Absences fell sharply to near zero days of unexcused absences in the months after referral for students in both the intervention and the comparison groups.
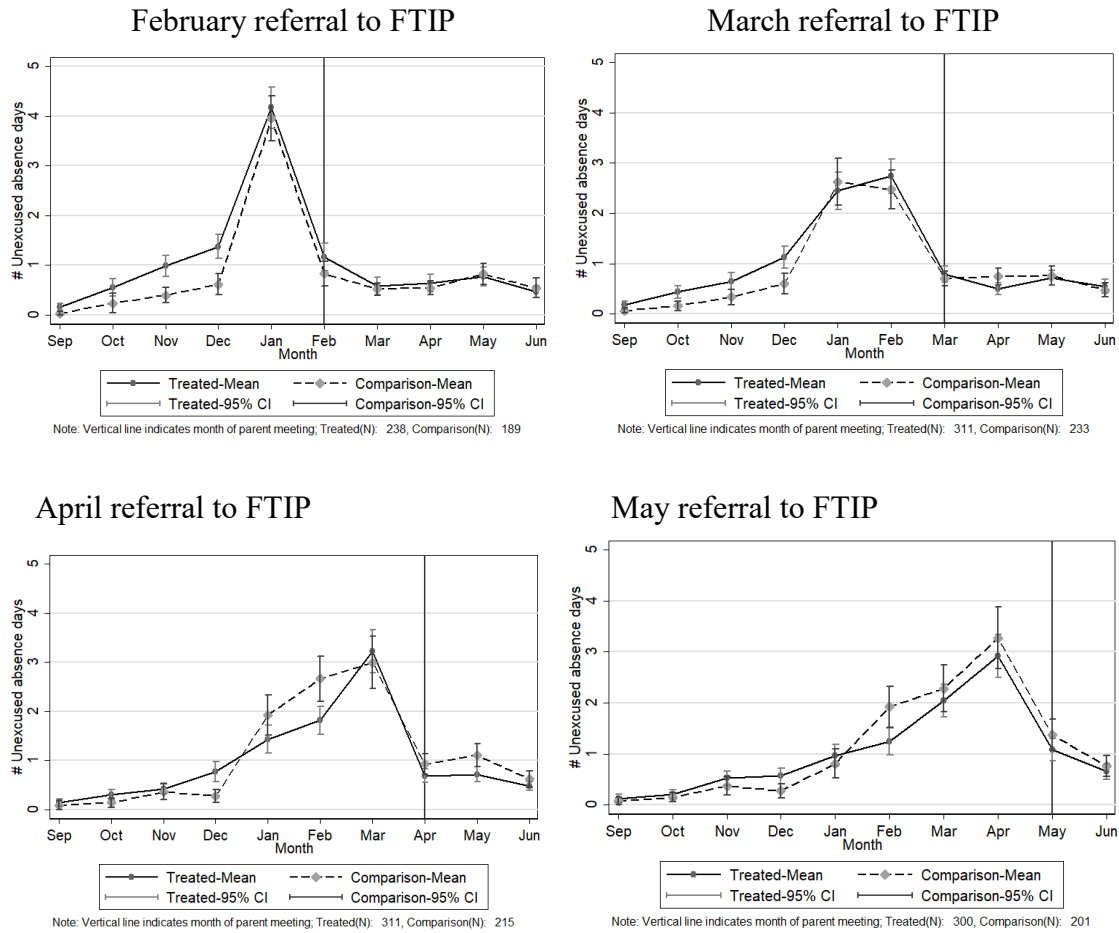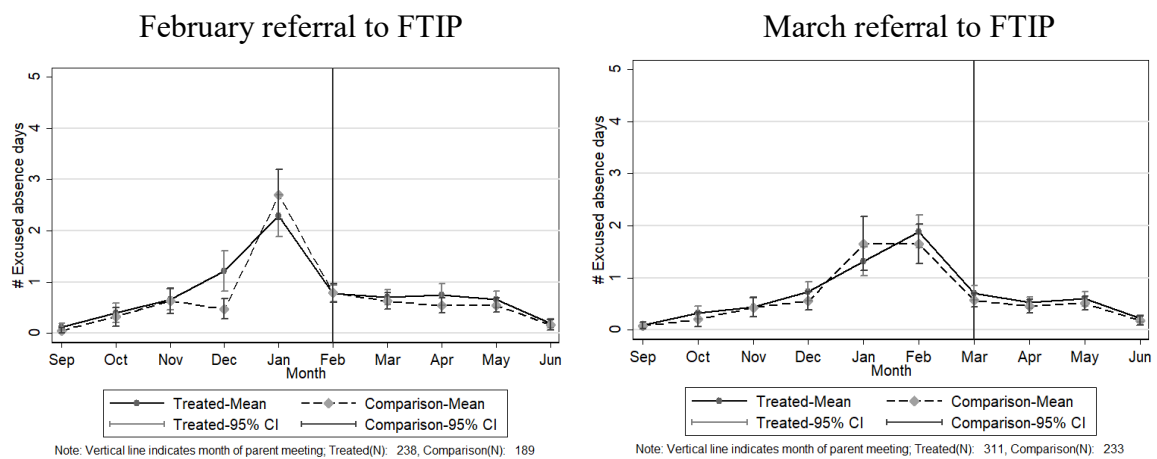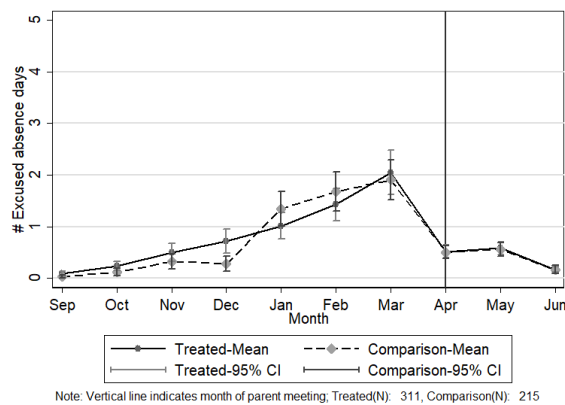
*Figure 10.* Trends in average number of unexcused absence days for elementary students in the FTIP intervention and comparison groups.

April referral to FTIP

May referral to FTIP
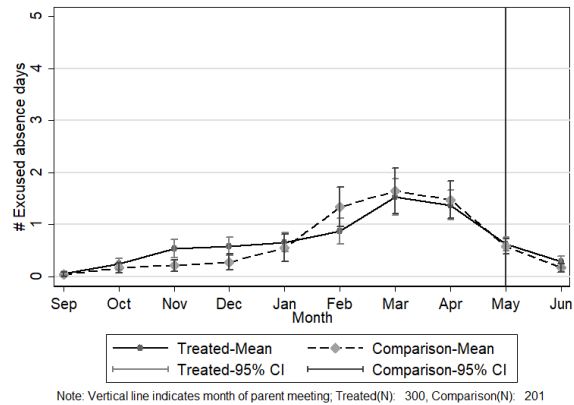


*Figure 11.* Trends in average number of excused absence days for students in the intervention and comparison groups.

**Research Question 3: Are There Racial/Ethnic Disparities in Referral to TIP or FTIP?**

We examined disparities in rates of initial referral to TIP and FTIP for two indicators of student attendance: the attendance rate as reported in the MDE data, and number of unexcused days, as reported in the absenteeism data shared by three school districts in Ramsey County. We converted the MDE measure of attendance rate into the number of days absent by dividing the attendance rate by the number of membership days (n=175 in all public, non-charter schools). This attendance indicator is relevant because the goal of TIP and FTIP was to improve overall student attendance. The Minnesota Department of Education has adopted the goal of consistent attendance, defined as attending 95% of enrolled days attended by 2020 with no racial or ethnic groups achieving below 90% attendance. Unexcused absences remain an important indicator of attendance because this is the key criterion used to refer students to TIP and FTIP.

Using standard procedures, we created post-stratification weights to adjust for differences in the racial compositions of the five school districts. This adjusted for the fact that the racial and ethnic composition of each district differed and some districts referred a higher percentage of students overall to the program.

**Results for TIP.** Between 2006 and 2015, 11,987 students were referred to TIP. Of these students, 36% were identified as Black, 22% as White, 10% as Hispanic, 24% as Asian/Pacific Islander, and 3% as American Indian or Alaskan Native. Figure 12 shows the proportion of students referred to TIP at each level of total absenteeism, calculated from the attendance rate. At each level of absenteeism for any cause, White students were referred to TIP at lower rates than students in all other racial or ethnic groups.
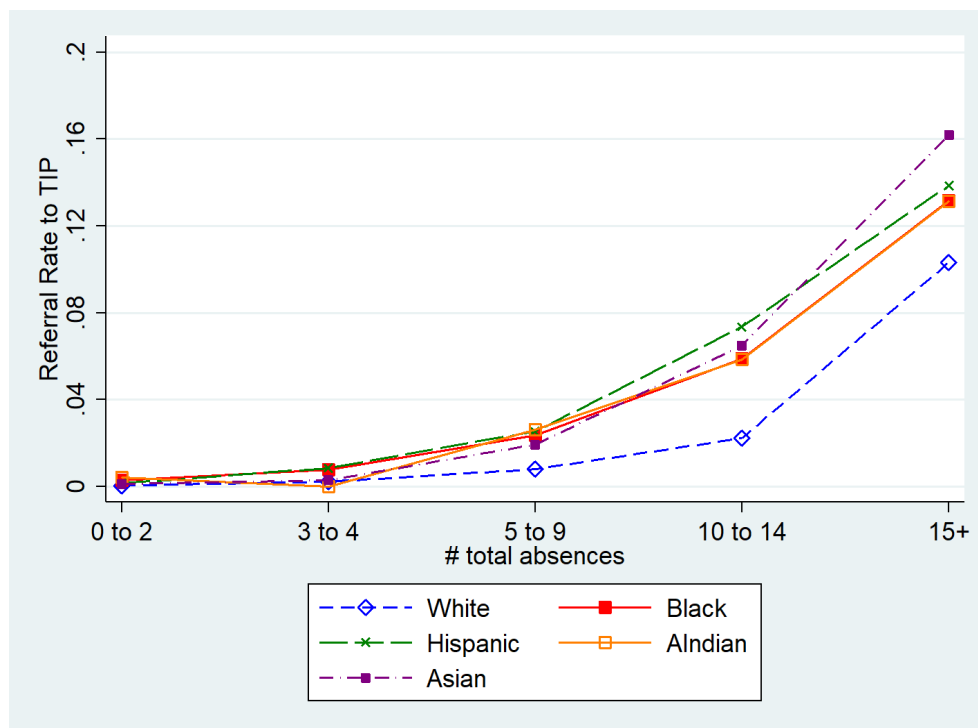


*Figure 12.* The proportion of Ramsey County students in grades 2–10 referred to TIP, by racial/ethnic group and level of absenteeism, 2006–2015.

Figure 13 shows the proportion of students referred to TIP at each level of unexcused absences. None of the differences in rates across groups were statistically significant,

meaning that any observed differences in the referral rate between racial and ethnic groups could be due to chance.
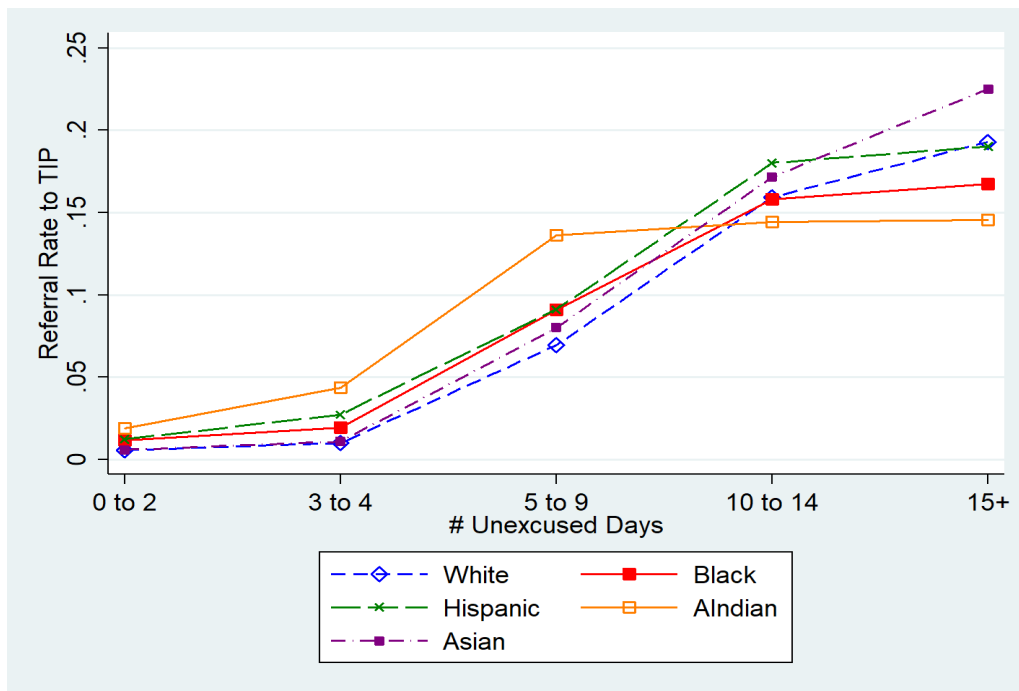


*Figure 13.* The proportion of Ramsey County students from middle and high schools referred to TIP, by racial/ethnic group and number of unexcused absences, 2006–2015.

Figure 14 presents the proportion of absences that were unexcused at each level of absenteeism. In this figure, total absences was simply the count of number of days absent (unexcused plus excused absences) from the school district data and was not adjusted for membership days. At each level of total days absent, White students had a smaller proportion of their absences coded as unexcused, making them less eligible to be referred to TIP. For example, among students with 5–9 total absences in a single year, White students had 21% of their absences coded as unexcused, whereas Black students had 44% of their absences coded as unexcused. In other words, Black students were 2.1 times more likely than White students to have any absence coded as unexcused. Among students with 15 or more absences (defined as chronically absent by MDE), White students had 43% of their absences excused compared

to 57% for Black students, a ratio of 1.3. The disparity was similar between White students and students from other racial/ethnic groups.
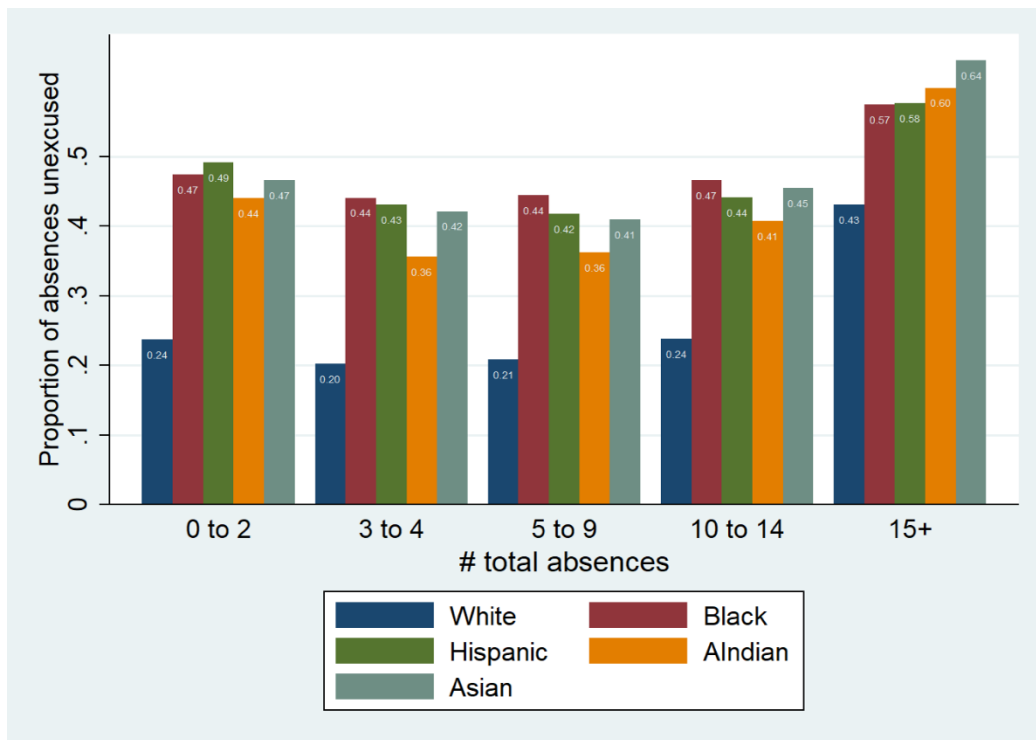


*Figure 14.* The proportion of absences coded as unexcused, by race and total number of absences, as reported by the three largest districts in Ramsey County, 2006–2015.

**Results for FTIP.** Between 2006 and 2015, 5,584 elementary students were referred to FTIP. Of these students, 51% were identified as Black, 18% as White, 15% as Hispanic, 11% as Asian/Pacific Islander, and 4% as American Indian or Alaskan Native. Figure 17 shows the proportion of students in elementary grades[1] referred to FTIP at each level of ADA. The ADA levels are presented as the number of absent days based on a school year of 175 days.[2] As shown in Figure 15, a higher proportion of Black and American Indian/Alaskan Native students were referred to FTIP, compared to all other students, at each level of ADA.

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[1] Elementary grades are defined as grades K-6 before 2014 and K-5 after 2014.
[2] We converted the annual attendance rate into the number of absent days as follows: 175-(attendance rate*175).
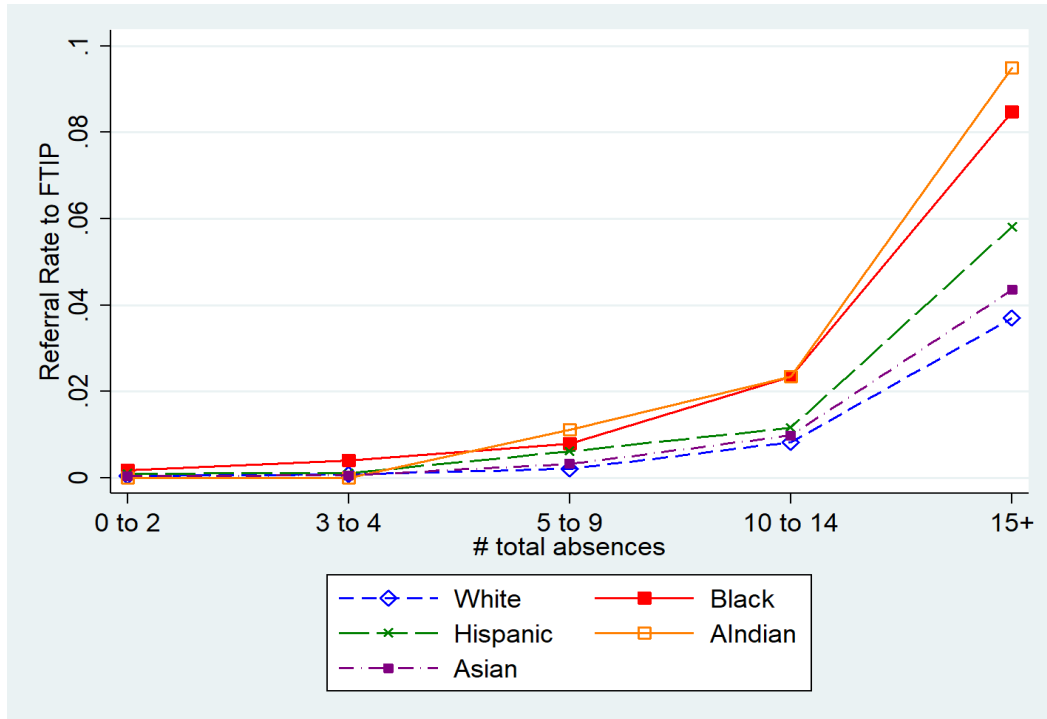
*Figure 15.* The proportion of Ramsey County elementary-grade students referred to FTIP, by racial/ethnic group and level of absenteeism (as measured by average daily attendance), 2006–2015.

Figure 16 shows the proportion of elementary students referred to FTIP at each level of unexcused absences. The level of racial disparity was reduced when attendance was measured as the number of unexcused days, the actual referral criteria for FTIP. There were no statistically significant differences in the rates of referral across racial/ethnic groups.
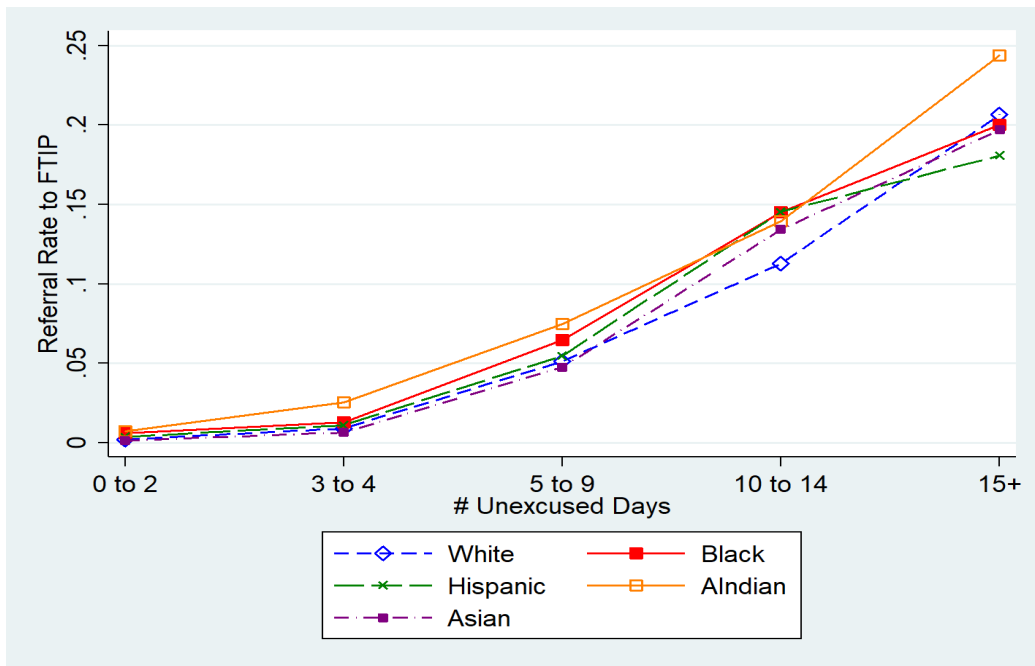
*Figure 16.* The proportion of Ramsey County students referred to FTIP, by racial/ethnic group and number of unexcused absences, 2006–2015.

Figure 17 presents the proportion of total absences that were unexcused. The total number of absences was a simple count of the number of days the student was absent, as reported by the three school districts. Regardless of the total number of absences, White students had a smaller proportion of their absences coded as unexcused, making them less eligible to be referred to FTIP even though they had the same number of total absences as students of color. For example, among students with two or fewer absences, 16% were coded as unexcused for White students, compared to 33% for Black students. This relative difference in referral rate was present across all levels of absenteeism. Among students with 15 or more absences (defined as chronically absent by MDE), 26% of absences were coded as unexcused for White students compared to 50% for Black students, a black-to-white referral ratio of 1.92.
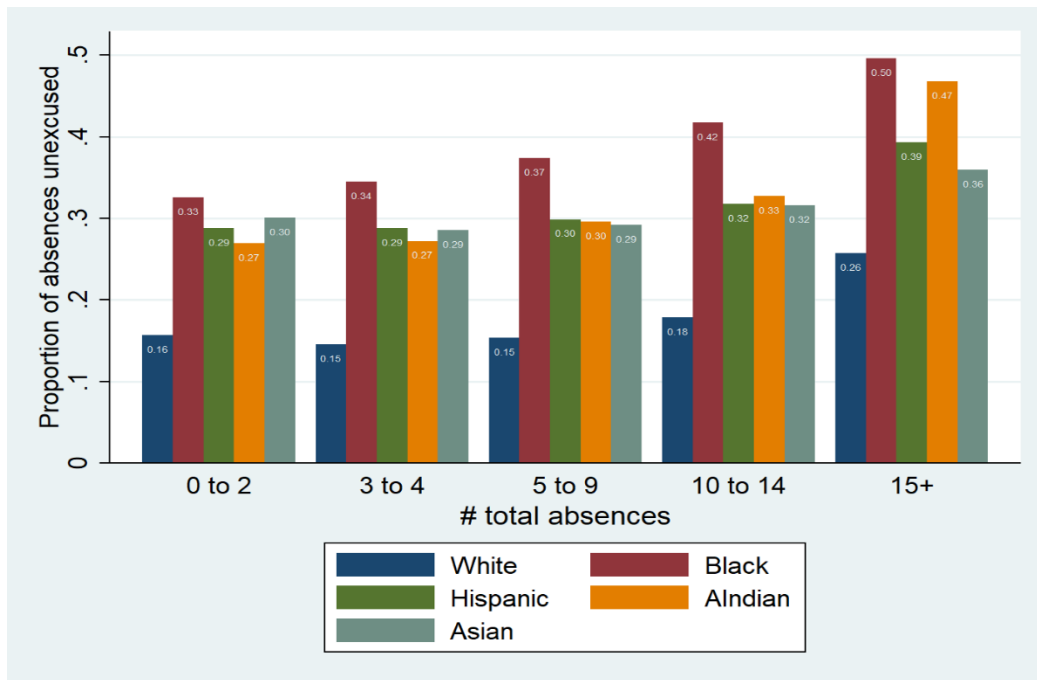
*Figure 17.* The proportion of absences coded as unexcused, by race and total number of absences, Ramsey County elementary students from 2006–2015.

## Conclusions

### Discussion of Findings

Children who are chronically absent often display poor academic outcomes (Gottfried, 2009, 2014; Hernandez, 2011) and negative longer-term consequences, including anti-social behavior (Garry, 1996), substance abuse (Hallfors et al., 2002; Henry & Huizinga, 2007; Vaughn et al., 2013) and criminal justice system involvement (Zara & Farrington, 2010). Chronic absenteeism also has the potential to reduce academic outcomes for other children in the same educational setting (Gottfried, 2019). Although schools, county attorneys, and juvenile justice systems invest substantially in efforts to curb chronic absenteeism, diversion programs have received remarkably little attention by prevention scientists. In this study, we evaluated the effectiveness of a common three-step diversion program to improve attendance among elementary, middle, and high school students.

Using matched sampling and dynamic difference-in-differences models with population data, we found that involvement in TIP did not improve either short-term or long-term attendance among truant students in grades 7–10, relative to the matched comparison group from a contiguous, demographically similar county where the program was not available. Although most estimated coefficients of program effects were negative, only a few of the coefficients were statistically significantly different from zero, and there was no consistent pattern of statistically significant negative effects for either measures of program participation: referral to TIP or parent participation in the parent meeting. This pattern of negative findings was not robust enough to conclude that TIP caused attendance to decline.

We also found that FTIP did not have a noticeable positive effect on improving the attendance of the referred elementary students, even up to four years after the referral, when compared to the matched comparison group of students from a neighboring county. The students in the matched comparison group shared similar characteristics as the intervention students, but the comparison students did not have the opportunity to be referred to the program due to an exogenous reason (their school district). Using monthly-level attendance data that disaggregated attendance into unexcused and excused absences, we found that there was no noticeable short-term effect for FTIP.

We also found that at each level of total or overall absenteeism, measured using the annual absenteeism rate, White students were referred to TIP at significantly lower rates than students in all other racial and ethnic groups. In contrast, there were no statistically significant racial/ethnic differences in the proportion of students referred to TIP at each level of unexcused absences. In the elementary schools, a higher proportion of Black and American Indian/Alaskan Native students were referred to FTIP, compared to all other students, at each level of total absences, as calculated from the average daily attendance rate. In contrast, there was no statistically-significant racial disparity in referral to FTIP when

attendance was measured as the number of unexcused days, the actual referral criteria for FTIP

## Implications for Policy and Practice

Since the National Institute of Justice funded this evaluation study, three-step court diversion programs for chronic absenteeism have become the single-most common truancy intervention in the U.S. In the past few years, for example, Tennessee and Texas have passed state laws requiring the three-step truancy diversion model be implemented in every school district. In a random sample of 90 school districts with more than 5,000 students drawn from the National Center for Education database, 63% of districts implemented some form of multi-step truancy diversion program in 2018 (Carpenter & McNeely, 2018).

Our study is one of only two rigorous studies testing this model, and the only one we know of in the U.S. The other study, conducted in Queensland, Australia, found positive effects of a truancy diversion model. Using a controlled intervention with intensive researcher involvement, Mazerolle and colleagues (2017) found that a diversion strategy incorporating principles of restorative justice increased student attendance by approximately 30% in the three semesters following the intervention. One potential reason for different results is the different comparison groups. The students in the Queensland study received a minimal set of interventions outside of the diversion program (Mazerolle et al., 2017). In the county where our study occurred, students in both the intervention and comparison groups likely benefitted from a wide array of attendance-focused interventions over the years that included evidence-based practices such as Check and Connect, Positive Behavior Interventions and Supports (PBIS), state-mandated letters to parents after three unexcused absences, and informal interventions from teachers, social workers, and guidance counselors. Thus, students in both groups in our study were likely experiencing interventions that were wide-reaching and individualized, although rarely explicit. Program staff anecdotally

reported that only students who were non-responsive to existing interventions were referred to TIP or FTIP; and, the low-resource program might be insufficient to help resolve the complex issues underlying the chronic absenteeism of referred students.

Another potential reason for the different findings is that although the programs both followed a three-step model of increasingly intense interventions that ultimately led to a court petition if attendance did not improve, the programs differed in two key ways. Perhaps the most important difference was the theoretical paradigm on which the intervention was based. Whereas the TIP and FTIP models are grounded in deterrence theory (Pratt, Cullen, Blevins, Daigle, & Madensen, 2008) and used a social work or case management paradigm involving child protective services and linking families to other social services, the court diversion model implemented by Mazerolle and colleagues (2017) was based on principles of procedural and restorative justice. At the family meeting equivalent to the SART hearing, the Queensland program included persons who had a personal relationship with the student and were directly affected by the student's truancy. This could be a teacher or a family member. The person was coached about how to share their story of how the truancy impacted them, without causing shame. A second difference, as mentioned earlier, was the careful attention to implementation fidelity in the Australian study. It is possible that multiple types of truancy interventions that have the level of resources, training, caseloads, and supervision that are afforded by well-funded clinical trials could be effective.

A final implication is that there is no easy solution to chronic absenteeism. Although inexpensive strategies, such as text messaging students and parents, have been shown to produce small improvements in attendance (in the range of a few percentage points; Bergman & Chan, 2017; Kraft & Rogers, 2015; Rogers et al., 2017; Rogers & Feller, 2018), there is no clear evidence that they work for students who are chronically absent. However, neither is it sufficient to make sweeping recommendations for multi-level strategies that involve students,

schools, and communities. A major research investment is needed to identify specific, replicable strategies that effectively reduce truancy, and then to identify how best to adapt them and take them to scale.

The authors of two meta-analyses of diversion efforts found that the most effective programs were efficacy studies in which the intervention was directed and closely monitored by researchers. In practice, however, highly efficacious programs may lose effectiveness once widely implemented (Stuart, Bradshaw, & Leaf, 2015). Over time, as agencies adapt evidence-based programs into complex organizational cultures and address logistical constraints, theoretically critical components may be lost (Beehler, Birman, & Campbell, 2012). In the U.S., there has been a push by experts and funders to disseminate evidence-based interventions developed through tightly controlled efficacy studies (Gottfredson et al., 2015; Institute for Educational Sciences, 2018). Our study demonstrates that it is also important to conduct effectiveness studies of practitioner-initiated, widely-implemented programs and strategies, even when randomized controlled designs are not available. The use of strong quasi-experimental designs is possible, as we have demonstrated here, and effectiveness studies can help address other limitations common to randomized study designs, such as small samples and the lack of long-term follow-up.

Our study findings also differed from the positive effects on attendance reported by two evaluations of truancy diversion programs that used one-sample, pretest-posttest designs (Mueller & Stoddard, 2006; National Center for School Engagement, 2006). As demonstrated by our analysis of short-term attendance trends, spikes in absenteeism are transitory and often naturally resolve. Evaluation studies of truancy programs that sample students based on recent poor attendance and use simple pre- and post-mean comparison of the program-referred students can be misleading due to regression to the mean. Moreover, the overall

declining trend in attendance across the academic years, could also lead to biased estimates of program effects in studies without a legitimate counterfactual group.

**Study Limitations**

Despite the rigorous quasi-experimental design and sensitivity analyses, our study has some limitations. First, schools in both the intervention and comparison counties likely implemented many other strategies to improve attendance in addition to the intervention under study. We do not have careful documentation of absentee prevention strategies other than the formal intervention programs (i.e., TIP/FTIP in the intervention districts and a formal program that began in the comparison districts in 2010) that were implemented by either the intervention or comparison districts. Although a heterogeneous mix of services was available in both counties, it is possible that the comparison county had more effective programming in place; thereby resulting in biased estimates of program effects. If the two counties implemented different types and intensities of truancy prevention strategies that differentially influenced attendance rates over time, the legitimacy of the counterfactual group would be reduced.

Although we collected extensive anecdotal reports about program implementation, including referral decisions, we did not collect this information systematically enough to rigorously provide information on why TIP and FTIP did not have more added value above and beyond other strategies. It is possible, for example, that there was a substitution effect such that schools switched to TIP from something else because it was less resource intensive and equally effective. Future research regarding program implementation and the extent to which program quality and fidelity affects student outcomes is needed.

Finally, because we used yearly attendance data and could not effectively differentiate unexcused from excused absences, it is not clear whether the interventions had any meaningful effect on the long-term trajectory of unexcused absences. Although we

demonstrated that the short-term results were consistent with the null long-term findings, this analysis depended on constructing the matched comparison from students who were eligible to be referred to the intervention but were not. It is more likely that unmeasured selection processes occur in this matched comparison group than in the comparison group consisting of students who were never eligible for the intervention. This makes it more difficult to meet the conditional independence assumption in our matching procedure. It is possible that the counterfactual is non-equivalent to the intervention group on other unobserved characteristics that are related to the outcome. Because schools referred students based on a myriad of factors that are unobservable in the data, it is difficult to be entirely sure that the data met the conditional independence assumption of the matching procedures. Although we conducted sensitivity analyses to overcome the limitation of using annual attendance as a baseline measure, the sensitivity analysis itself was challenged by the fact that students in the matched sample were eligible for the program but were not referred to it for unknown reasons. The consistent findings of null to negative program effects across different measures of program implementation, different measures of attendance, and multiple comparison groups, gives us confidence that the findings are not spurious, as the predicted biases using these different approaches were not all in the same direction.

**Implications for Future Research**

Given the high prevalence of court diversion models (which are increasingly codified in state laws), the limited research on court diversion approaches to chronic absenteeism, and conflicting study findings among the few studies that exist, the clearest implication is the need for more research. Most needed are studies that compare the three-step model as currently implemented with a model of the same three-step structure but which has a focus on restorative and procedural justice. Yet, this work needs to be done thoughtfully and carefully,

as some restorative justice programs have been found to actually increase recidivism (Sherman et al., 2015).

Our analysis suggested that evaluation studies of programs to reduce chronic absenteeism that use pre- and post-mean comparisons of the program-referred students without a legitimate counterfactual group (Mueller & Stoddard, 2006; National Center for School Engagement, 2006) can be misleading due to the transitory nature of absences and the heterogeneity in attendance trends by different grade levels. We demonstrated that spikes in absenteeism are transitory and often naturally resolve. Evaluation studies of truancy programs that sample students based on recent poor attendance and use simple pre- and post-mean comparison of the program-referred students can be misleading due to regression to the mean. Moreover, the overall declining trend in attendance across the academic years, could also lead to biased estimates of program effects in studies without a legitimate counterfactual group.

Our study affirmed how challenging it is to improve attendance among chronically-absent students. Thirty-five states have adopted goals to improve attendance under the federal Every Student Succeed Act, and yet there are few low-cost programs known to effectively reduce absenteeism. More research is need on all programs designed to improve attendance among chronically-absent students who face complex family and academic issues, and additional strategies tailored to their individual situations may be needed. As has been discovered with many other juvenile justice-based interventions, the effectiveness of court diversion programs may depend on the extent to which implementation is guided by sound developmental and educational theory as well as the quality and consistency of program implementation.

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## Dissemination of Research Findings

### Juried Presentations

McNeely, C., Lee, W., Alemu, B., Rosenbaum, J., & Renner, L.M. (2018, April). *"Absent again?" Evaluation of a truancy diversion program using matched sampling.* Paper presented at the 17th Biennial Meeting of the Society for Research on Adolescence. Minneapolis, MN.

Lee, W., Rosenbaum, J., & Renner, L.M. (2018, April). *Can early screening and intervention for educational neglect improve school attendance during adolescence? A quasi-experimental study.* Paper presented at the 17th Biennial Meeting of the Society for Research on Adolescence. Minneapolis, MN.

McNeely, C., & Alemu, B. (2018, April). *Deciding what is excused: How absenteeism policies can produce racial disparities in truancy interventions.* Paper presented at the 17th Biennial Meeting of the Society for Research on Adolescence. Minneapolis, MN.

Alemu, B., McNeely, C., Rosenbaum, J., & Lee, W. (2018, April). *A quasi-experimental analysis of the elementary to middle school transition.* Poster presented at the Population Association of America annual meeting. Denver, CO.

McNeely, C., & Renner, L.M. (2016, May). *Reducing racial/ethnic disparities in school discipline? The Truancy Intervention Program*. Poster presented at the 24th annual meeting of the Society for Prevention Research. San Francisco, CA.

### Peer-Reviewed Manuscripts

McNeely, C., Lee, W.F., Rosenbaum, J.E., Alemu, B., & Renner, L.M. (accepted, pending revisions). Long-term effects of truancy diversion on school attendance: A quasi-experimental study with linked administrative data. *Prevention Science*.