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Long-Term Effects of Truancy Diversion on School Attendance: a Quasi-Experimental Study with Linked Administrative Data

Clea A. McNeely[1] · Won Fy Lee[2] · Janet E. Rosenbaum[3] · Besufekad Alemu[2] · Lynette M. Renner[2]

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Abstract
Over 60% of US school districts implement court diversion programs to address chronic unexcused absenteeism, yet the effectiveness of these programs is not known. We evaluated whether the Truancy Intervention Program (TIP) improved the school attendance of students in grades 7–10 in a metropolitan county in the Midwestern USA. Similar to most truancy court diversion programs, TIP consisted of three increasingly intrusive steps: (1) a parent meeting, (2) a hearing to develop an attendance contract, and (3) a petition to juvenile court. The intervention group consisted of students from the intervention county who had been referred to TIP between 2006 and 2009. The comparison group was drawn from a contiguous, same-sized, and socio-demographically similar county that petitioned truant students directly to court. To construct the comparison group, we applied multi-level matching procedures to linked, individual-level administrative data from eight state and local agencies for all public school students in the state between 2004 and 2015. Using the matched samples, we conducted difference-in-differences analyses to identify program effects for two intervention groups: all students referred to TIP and students whose family participated in the group parent meeting. In the 4 years after the intervention, the intervention groups had similar or slightly lower attendance than the comparison groups. However, most coefficients were not statistically significant, and there was no consistent pattern of effects across different samples and different specifications of the intervention. This pattern of findings was not robust enough to conclude that the program influenced school attendance.

Keywords Truancy · Juvenile court diversion programs · Attendance · Chronic absenteeism

Chronic absenteeism, defined as missing 10% of the school year, is one of the strongest predictors of school dropout and low wages in adulthood (Vaughn et al. 2013). In 2016, 14% of US middle school students and 21% of high school students were chronically absent (U.S. Department of Education 2019). Under the federal Every Student Succeeds Act, 36 states and the District of Columbia have set ambitious goals to reduce chronic absenteeism (Attendance Works 2018).

Primary prevention strategies that mail or text parents specific information about their child's absenteeism, along with behavioral nudges or encouragement to improve attendance, have produced small increases in average daily attendance rates (Rogers et al. 2017). However, there has yet to be a concerted research effort to develop and test secondary and tertiary prevention strategies to improve attendance among chronically absent middle and high school students, who often face complex obstacles to school attendance (Klima et al. 2009; Maynard et al. 2012; Tanner-Smith and Wilson 2013).

In this study, we evaluated the effectiveness of a court diversion program designed to improve school attendance among chronically absent students. The majority of court diversion programs focuses exclusively on truancy, or *unexcused* absences, and does not address the full scope of chronic absenteeism. Nonetheless, they are an important focus of research because they are implemented by over 60% of large school districts in the USA and can result in severe consequences for students (Carpenter and McNeely 2018).

The Truancy Intervention Program (TIP) is a court diversion program for students ages 12–16 in a large Midwestern

✉ Clea A. McNeely
cmcneely@utk.edu

1 University of Tennessee, Knoxville, TN 37996, USA

2 University of Minnesota, St. Paul, MN 55108, USA

3 SUNY Downstate School of Public Health, Brooklyn, NY 11203, USA

county. Like most truancy court diversion programs in the USA, it consists of three increasingly intrusive steps: a parent meeting (step 1), an attendance contract that often includes referrals to services (step 2), and a truancy petition to court (step 3). Students only advance to the next step if attendance does not improve. To assess whether participation in TIP improved school attendance among chronically absent students, we applied difference-in-differences methods to matched samples drawn from 12 years of longitudinal administrative data provided by eight state and local agencies.

Theory and Evidence for the Effectiveness of Court Diversion Programs

In 43 states, unexcused absenteeism, or truancy, is a status offense for which students ages 12–16 can be petitioned to juvenile court (Conry and Richards 2018). During the zero-tolerance movement of the 1990s and early 2000s, schools increasingly requested truant students be petitioned to court. The number of truancy cases formally processed in juvenile courts more than doubled between 1995 and 2003 from 34,900 to 74,600 (Hockenberry and Puzzanchera 2015). This number is a fraction of the total number of students petitioned to court, as most cases were informally processed and did not produce a juvenile record (Hockenberry and Puzzanchera 2018).

The popularity of zero-tolerance approaches waned, however, as evidence emerged that they exacerbated racial disparities in the juvenile justice system (Skiba et al. 2011) and failed to reduce behavior problems (Liberman et al. 2014). Many states and counties replaced direct court petitions with court diversion programs that were less punitive and more equitable than direct court petition (Development Services Group, Inc 2017). The theory underlying court diversion programs was first articulated by Becker (1963) and later termed labelling theory (Akers 2000). According to labelling theory, the stigma of involvement in the juvenile justice system causes a young person to identify with and continue to enact the role of delinquent. Between 2007 and 2016, the number of truancy cases processed by juvenile courts declined by 11% to 54,000 cases (Hockenberry and Puzzanchera 2018).

Evidence is mixed regarding the effectiveness of juvenile court diversion programs at improving behavioral problems. Authors of a meta-analysis of 73 programs, none focused on truancy, reported that juvenile court diversion programs achieved lower recidivism compared to normal court practice (Wilson and Hoge 2013). However, the meta-analysis placed few restrictions on study design, and the studies using experimental or matched sampling methods found no effect. A separate meta-analysis of 28 juvenile court diversion programs for non-violent offenses, including two truancy interventions, analyzed only randomized or quasi-experimental evaluations (Schwalbe et al. 2012). Overall, there were no differences in recidivism rates between diversion programs and standard court practice. However, moderation analyses showed that restorative justice programs with manualized interventions and active involvement by researchers to assure fidelity did reduce recidivism.

We identified five evaluations of truancy court diversion programs that followed a three-step model similar to TIP. Four of the studies used a pre-test/post-test single-group design and reported positive effects on school attendance (Fain and Turner 2014; Mueller and Stoddard 2006; National Center for School Engagement 2006; Richtman 2007). However, these studies lacked an equivalent comparison group and reported only short-term results. Spikes in absenteeism are transitory and often resolve naturally. Evaluation studies that simply compare pre-test and post-test attendance rates cannot disentangle true program effects from the natural bounce back in attendance that inevitably occurs when a sample is drawn from the extreme tail of the attendance distribution (i.e., regression to the mean).

One study was a carefully conducted randomized controlled trial of a police-school partnership in low-resource schools in Queensland, Australia, for students ages 10–16 years who were absent 15% or more days for unexcused reasons (Mazerolle et al. 2017). The intervention in this study was similar to the three-step diversion model in that it added a conference bringing together parents, school staff, and law enforcement to the traditional practice of direct petitions to court. Key to the intervention was the scripted participation in the family conference of a caring teacher or school staff person known to the student to help the student understand how their behavior was negatively affecting people who cared about them. The aim of this restorative justice approach was to improve attendance by legitimizing the authority of law enforcement. Unlike other three-step court diversion models, the program did not refer families and students to educational or social supports. 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. This was compared to a decline from 25 days to 23 days for the same-sized comparison group that received a direct referral to court. Based on the findings of this efficacy study, the authors concluded that the intervention has promise for reducing chronic truancy and recommended future evaluation research.

Our study makes three contributions to understanding the potential of the three-step court diversion model to reduce truancy. First, we conduct an effectiveness study of implementation of the three-step court diversion model in “real world” conditions where program staff were not trained in the theoretical underpinnings of the model and implementation fidelity was not precisely defined. Second, we assess the effectiveness of the three-step diversion model in a population exposed to a relatively rich array of other programs and strategies to improve attendance. Students in both the intervention and comparison groups in our study received an unknown and varied mix of interventions that included formal programs targeted at increasing school engagement (e.g., Positive Behavioral Interventions & Supports, Check & Connect), school policies regarding unexcused absences (e.g., denial of credit for work missed, Saturday school), and individual staff and teachers' responses to absenteeism. As schools across the USA strive to achieve new state and federal targets for reducing chronic absenteeism (U.S. Department of Education 2015), the number and variety of approaches dedicated to reducing absenteeism will increase, making the evaluation in this resource-rich setting highly relevant. Third, we examine the sustainability of program effects by examining attendance trajectories up to 4 years after referral to the program.

If effective, TIP could be a lower-cost, already-in-place alternative to restorative justice approaches that require a unique school staff member to be trained to participate in the intervention for each truant student. If, however, TIP is not effective, our findings would support a greater investment in and study of restorative-justice based models in the USA.

Method

Truancy Intervention Program

TIP was developed by the county attorney's office in partnership with the county's five largest public school districts. All public and private middle and high schools in the county can participate in the program. Similar to other court diversion truancy interventions, the program has three steps. After a student has three full-day unexcused absences (defined as three or more periods), school staff send a letter to the student and parent(s) stating that failure to improve attendance will result in a referral to TIP. After two more full-day unexcused absences, for a total of five, school personnel may refer the student to TIP. Schools exercise substantial discretion regarding whom to refer and when to make referrals. Upon receipt of a TIP referral, a county attorney sends a letter to the parent(s) stating that they and their child must attend a group parent meeting at the school (step 1). At the meeting, an assistant county attorney explains the legal consequences of continued truancy and the importance of school attendance to each student's future.

Students who accrue five or more additional unexcused absences after the parent meeting, for a total of 10, may be referred to the second step of the program, the student attendance review team (SART) hearing. Prior to the SART hearing, a school or district staff person conducts an informal inquiry into the reasons for the ongoing unexcused absences. In the SART hearing, the parent(s) and the student meet with a school or district representative and a county attorney to create a contractual agreement to improve attendance. The attendance contract formulated at the SART meeting typically involves specific actions by the student and parent(s), enhanced supervision of attendance by school personnel, and referrals to social service agencies to address the complex issues underlying poor attendance, such as homelessness, family or student mental health issues, substance use disorders, bullying, or lack of safe transportation to school. Unlike many other diversion programs, TIP does not give students a fresh start each year, meaning the unexcused absences from prior years count towards students' eligibility to be referred to the second and third steps. The program follows students across schools as long as they remain in a public or private school in the county.

If another five unexcused absences accumulate after step 2, for a total of 15 or more, school personnel may request a petition to juvenile court (step 3). Juvenile court judges typically impose conditions that, if met, result in the dismissal of charges. For example, the student may receive a court order for substance use treatment.

Data

We created a panel dataset containing all students enrolled in a public school in the state between 2004 and 2015 using linked, individual-level administrative data from eight state and local agencies. The core dataset came from the state's Department of Education, which maintains individual-level data on all students in public schools, including charter and alternative schools. To this dataset, we linked TIP data provided by the county attorney's office, information on child welfare involvement from the state's Department of Human Services, and daily absenteeism data from five large public school districts in the county.

We assessed the program effect for students referred to TIP between the 2005–2006 and 2008–2009 academic years. We chose 2006 as the lower bound to have attendance information for the 2 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 three-step diversion program in 2010. This upper bound also allowed us to examine the attendance trajectory for up to 4 years after program referral.

Measures

Program Participation

A student was defined as having participated in the program if they or their family received a letter from the county attorney's office stating that more than five unexcused absences were accrued, and they were required to attend the parent meeting on a specific date (step 1). To understand whether the program improved attendance for students whose parents actually attended a parent meeting, we created an indicator of whether a parent attended the scheduled parent meeting.

School Attendance

The primary outcome was the student's annual daily attendance rate, defined as the proportion of days enrolled in any public school in the state (called "membership days") that the student attended. This variable was provided by the Department of Education. Even though program referral is based on unexcused absences, our outcome measure does not distinguish between excused and unexcused absences because the goal of the program is to improve overall attendance. As a secondary outcome variable, 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, students were considered absent for a full day if they had three or more full-period absences. We did not have daily absenteeism data for students in the comparison county.

Matched Sampling

Under the assumption that the distributions of measured and unmeasured student characteristics are similar between the intervention and control groups, matching can approximate a randomized experiment (Blundell and Costa Dias 2000; Stuart 2010). We matched students in the intervention to students from a contiguous county that did not implement a court diversion program until after the study period. Both counties contained equivalent-sized cities and suburbs, were demographically and geographically similar, and experienced similar macro-economic and demographic shifts during the study period.

Variables for Matching We used several variables from the administrative datasets to create matched samples and to check whether variables associated with absenteeism had similar distributions in the intervention and matched control sample. From the Department of Education data, we drew the following variables measured in the year of program referral: gender, race/ethnicity, eligibility for free lunch, and indicators of whether the student had an individualized education program (an indicator of an emotional, behavioral, learning or physical disability). From the same dataset, we also measured whether the student was classified as an English language learner, received homebound instruction (available for students unable to attend school for more than 5 days for any valid reason, which could range from hospitalization to suspension), had ever been retained for one or more grades, and was classified as gifted or talented. Additional variables included the number of school transfers in the current school year (i.e., mobility), math and reading standardized test scores for available grades (proficient vs. not proficient and Z scores), and average daily attendance in the year of referral.

Average daily attendance in the year of TIP referral reflected attendance both before and after program referral, thereby potentially incorporating a program effect, which could bias results towards zero. Thus, we also matched or checked the balance of the matched samples on attendance in the year before referral, the change in attendance between the prior year and the year of referral, and 3-year attendance trajectories prior to the year of referral created using latent class analysis of the average daily attendance rate.

From the Department of Human Services data, we used the following variables for matching and checking covariate balance: whether anyone in the student's home had been involved in a child maltreatment report to the county child protection services and whether the student had ever had an out-of-home placement prior to the year of referral.

Matching Procedure We followed a two-stage matching procedure due to the large size of the dataset and challenges with computational tractability: school districts and then individual students. For the district-level matching, we pooled student-level data from 2007 to 2014 to create four district-level variables: (1) the proportion of students who were non-White, (2) mean Z scores on standardized math and reading tests for the years the tests were given, (3) number of students, and (4) the proportion of students eligible to receive free lunch. We selected comparison school districts in the adjacent county using nearest-neighbor matching with Mahalanobis metrics with replacement on the four district-level variables.

Because attendance systematically declines between grades 7 and 10 (U.S. Department of Education 2019), individual-level matching was done within grade and academic year strata in the subset of matched school districts. Within each grade-year strata, we first randomly pruned observations from the comparison group for computational tractability (Ho et al. 2007; King and Zeng 2007) and then 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 and Nielsen 2019). After matching, we pooled matched samples by grade and within each grade checked the balance on 15 student characteristics. This matching process was carried out separately for two distinct measures of program participation: referral and attendance at parent meeting. In each grade, between 15.6 and 23.3% of students (20.3% overall) were dropped due to pruning cases outside the area of common support.

Difference-in-Differences Estimation

The primary concern for identifying causal effects in non-equivalent group designs is bias from differential selection into treatment and control conditions. For example, if the students referred to TIP had lower mean levels of achievement motivation (an unmeasured characteristic) than students in the matched comparison group, and achievement motivation was correlated with attendance, a naïve estimation of program effects would be biased towards zero. As an identification strategy, we employed difference-in-differences methods on the matched samples, an approach shown to reduce bias that remains after matching (Blundell and Costa Dias 2000; Smith and Todd 2005).

Difference-in-differences (DD) models minimize selection bias caused by differential selection into the intervention and comparison groups on unmeasured, time-invariant characteristics such as race/ethnicity. DD models additionally overcome selection bias from time-varying unobserved characteristics (e.g., achievement motivation) if, in the period before the intervention is implemented, the trend in the outcome variable is parallel in the intervention and comparison groups (i.e., the attendance trend has the same slope in both groups). Parallel trends in the outcome pre-intervention indicate that differences in the distribution of unobservable characteristics between the intervention and comparison groups do not differentially affect attendance in the two groups. If the parallel trends assumption holds, a statistically significant change in slope for the intervention group relative to the comparison group after the intervention can more confidently be interpreted as a real program effect.

We estimated dynamic DD models with a full set of leads and lags of program dummies to examine dynamic program effects:

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

where $\boldsymbol{X}_{it}$ was the vector of time-varying student characteristics, ρ_t was grade dummies that capture time trends, and γ_i controlled for the time-invariant unobserved individual-level characteristics. The term $1[t - t^{\text{referral}} = k]$ was an indicator equal to 1 if student i was k years relative to the program referral year in grade t and 0 otherwise. The indicator variable was recoded as 0 when $k = 0$ for the intervention group so that the interpretation of the program effect, β_k, could be made relative to the year of referral. We limited the analysis window to a 6-year period spanning from 2 years prior to program referral until 4 years after referral. This allowed us to check the pre-intervention parallel trend assumption while keeping the loss of sample members due to censoring to a minimum. We conducted all analyses using Stata15-MP.

Results

Program Implementation

In both the intervention and comparison school districts, school staff referred the student to the county attorney's office for truancy. In the intervention county, all documented truancy cases participated in the truancy diversion program; no direct petition to court was allowed. In the comparison county, by contrast, all documented truancy cases were petitioned to court directly as no diversion program existed.

The linked dataset contained 4412 students in 7th–10th grades referred to TIP between 2006 and 2009. Of the referred students, 61% ($n = 2679$) had a parent attend the group parent meeting, 28% ($n = 1219$) had a SART hearing, and 17% ($n = 749$; 61% of those referred to a SART hearing) were eventually petitioned to juvenile court for truancy. These program-referred youth had an average daily attendance rate of 85%, the equivalent of missing 26 days of school in a full academic year (Table 1). Three-quarters (77%) were eligible for free lunch; and referred students moved frequently, with each student attending an average of 1.8 schools in the year of referral. Referred students also faced academic challenges. Almost half (48%) had been held back a grade, and their average scores on standardized reading and math tests were nearly one standard deviation below the mean. Five percent of these students experienced an out-of-home placement, which may be an indicator of family trauma.

A straightforward but difficult-to-estimate statistic was the proportion of eligible students referred to the program. We estimated this statistic for the schools with more than ten eligible students in the three districts that had reliable daily attendance data (89% of the sample). In this subgroup of schools, staff referred 18.6% of eligible students to TIP (SD = 11.3). Table 1 presents the characteristics for students eligible for TIP from the three school districts in which eligibility was estimated. Among students eligible for TIP, those referred were more likely to receive free or reduced lunch (80% vs. 71%), to have been retained one or more grades (49% vs. 41%), and to have lower standardized test scores, particularly in math (− 0.95 vs. − 0.77 SD units).

Matching

The five intervention and four comparison school districts selected from the district-level matching strategy were socio-demographically similar. The two sets of school districts each contained one urban district and three or four suburban districts and approximately 30,000 students in grades 7–10 annually. In the 2010 academic year in both the intervention and comparison school district sets, 47% of students were eligible for free lunch, the majority of students were racial/ethnic minorities (51% and 55%, respectively), and approximately one-quarter of students were English language learners (28% and 23%, respectively).

Table 1 Mean differences between students who were referred, eligible but not-referred, and not eligible for TIP (averages across the grade)

	All referred students		Eligible and referred (R)		Eligible, not-referred (ENR)		
	Number of students	Mean (SD)/ proportion	Number of students	Mean (SD)/ proportion	Number of students	Mean (SD)/ proportion	R vs ENR
Attendance rate	4412	0.85 (0.10)	3947	0.84 (0.10)	9111	0.86 (0.12)	***
Number of schools	4412	1.76 (0.76)	3947	1.86 (0.86)	9111	1.75 (0.90)	***
Individual education program	4412	0.24 (0.41)	3947	0.24 (0.42)	9111	0.22 (0.41)	
Free-lunch eligible	4412	0.77 (0.38)	3947	0.80 (0.36)	9111	0.71 (0.43)	***
Female	4412	0.48 (0.49)	3947	0.47 (0.50)	9111	0.47 (0.50)	
Grade	4412	8.64 (0.80)	3947	8.65 (0.86)	9111	8.83 (0.96)	***
Grade retention (current year)	4412	0.07 (0.21)	3947	0.08 (0.23)	9111	0.06 (0.21)	***
Grade retention (ever)	4412	0.48 (0.49)	3947	0.49 (0.49)	9111	0.41 (0.41)	***
Child maltreatment report in home (year)	4412	0.02 (0.09)	3947	0.02 (0.10)	9111	0.01 (0.10)	
Out-of-home placement (year)	4412	0.05 (0.17)	3947	0.06 (0.19)	9111	0.04 (0.18)	***
English language learner	4412	0.38 (0.48)	3947	0.40 (0.49)	9111	0.36 (0.48)	***
White	4412	0.22 (0.41)	3947	0.18(0.36)	9111	0.24 (0.41)	***
Black	4412	0.36 (0.48)	3947	0.38 (0.48)	9111	0.39 (0.49)	
Hispanic	4412	0.15 (0.35)	3947	0.15 (0.36)	9111	0.13 (0.34)	**
Asian	4412	0.24 (0.42)	3947	0.25 (0.43)	9111	0.21 (0.41)	***
American Indian	4412	0.03 (0.17)	3947	0.03 (0.17)	9111	0.03 (0.17)	
Z-score for standardized math test	2889	− 0.92 (0.91)	2349	− 0.95 (0.92)	4307	− 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)	7213	− 0.80 (0.99)	***
Did not take standardized reading test/no score	4412	0.40 (0.29)	3947	0.41 (0.32)	9111	0.41 (0.36)	

Bonferroni significance test are presented by asterisks: *significant at 5%; **significant at 1%; ***significant at 0.1%

Before and after the individual-level matching, we assessed whether the intervention and matched comparison groups had similar means on 15 observed covariates. Following the recommendation of Ho et al. (2007), we converted the difference in the means into standard deviation units and plotted these standardized mean differences to ease interpretation. The black triangles in Fig. 1 represent the standardized mean differences before matching. Triangles to the right of the center line indicate that the intervention group had a higher mean than the comparison group before matching, and vice versa. The gray dots represent the standardized mean differences after matching.

Rosenbaum and Rubin (1985) recommended that after matching, the standardized mean differences should be less than ± 0.2 standard deviations. In Fig. 1, 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. We also obtained good covariate balance for the analysis of the effect of TIP on students who attended the parent meeting.

The matching algorithm dropped intervention students for whom matches could not be found (i.e., were outside the area of common support). We also excluded students who did not have attendance data in the 2 years prior to the referral year because they were not enrolled in a public school in the state. Compared to intervention students retained in the analytic sample, the excluded students were less likely to be eligible for free lunch but more likely to have lower average daily attendance, lower standardized math scores, and greater numbers of school transfers. The excluded students also were over twice as likely to have had an out-of-home placement.

Attendance Trends in the Matched Samples

Figure 2 shows the trends in average daily attendance for students referred to TIP and the matched comparison groups from the adjacent county. The follow-up period was 4 years for the students referred to TIP in 7th and 8th grades, 3 years for students referred in 9th grade, and 2 years for students referred in 10th grade. Due to right censoring, the actual length of follow-up was 3.8 and 3.7 years, respectively, for students referred to TIP in 7th and 8th grades. For intervention students referred in 9th and 10th grades, the average length of follow-up was 2.9 years and 1.8 years, respectively. There were no differences in average length of follow-up between the intervention and comparison samples.

There were no statistically significant differences in the attendance trends in the 2 years prior to the interventions,

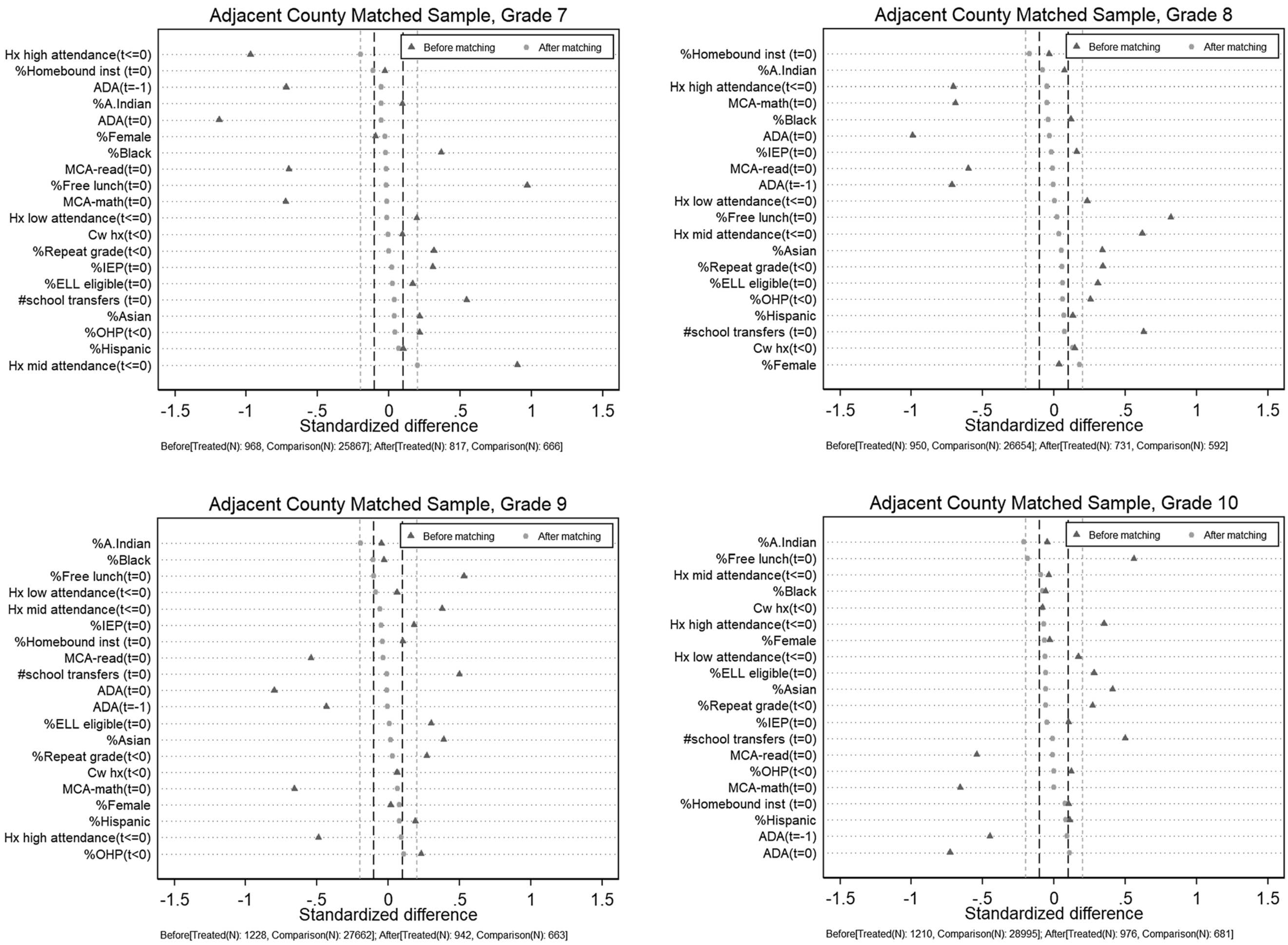

Fig. 1 Standardized mean difference plot before and after creating matched samples from an adjacent county for the analysis of the effect of program referral on average daily attendance rates. *ADA* = average daily attendance, *MCA*–read = Z-score for standardized reading test, *MCA*–math, = Z-score for standardized math test, *Cw hx* = ever any report of child maltreatment in home, *IEP* = student has individualized education plan, *OHP* = ever out-of-home placement

suggesting that the parallel trends assumption was not violated. Students in all grades experienced a relatively sharp decline in attendance in the year prior to the intervention. There were also no statistical differences in attendance trends in the years after the referral to TIP. Figure 3 presents the trends in average daily attendance for the subset of students who attended the parent meeting and their matched comparison counterparts. There were no statistically significant differences in these trends, with two exceptions: students referred to the intervention in 8th grade had lower attendance than their matched comparison counterparts in 11th grade, and students referred in 10th grade had lower attendance than their matched counterparts in 12th grade.

Difference-in-Differences Models

Table 2 presents the estimated coefficients for the dynamic DD models that allowed treatment dummies to interact with year-specific time dummies. The coefficients for the 2 years prior to the intervention can be interpreted as a test of the parallel trend assumption. A few of the coefficients were statistically significant in a few of the grades, but the effect sizes were small (approximately a 1-percentage-point difference in the attendance rate between intervention and comparison groups). There was no consistent pattern that would suggest a systematic violation in the parallel trend assumption.

The coefficients for the 1 to 4 years post-treatment variables can be interpreted as the average treatment effect in that year relative to the year of referral. The coefficients in the model estimating program effects on all referred students indicated small reductions in attendance in most years and grades, but few of the differences were statistically significant. The coefficients in the model estimating program effects for students whose parent(s) attended the parent meeting indicated the program had no effect on attendance in the year after referral. In the second year after referral, 9th and 10th grade students had more absences than the comparison group, as did 8th grade students in the third and fourth years after program referral.

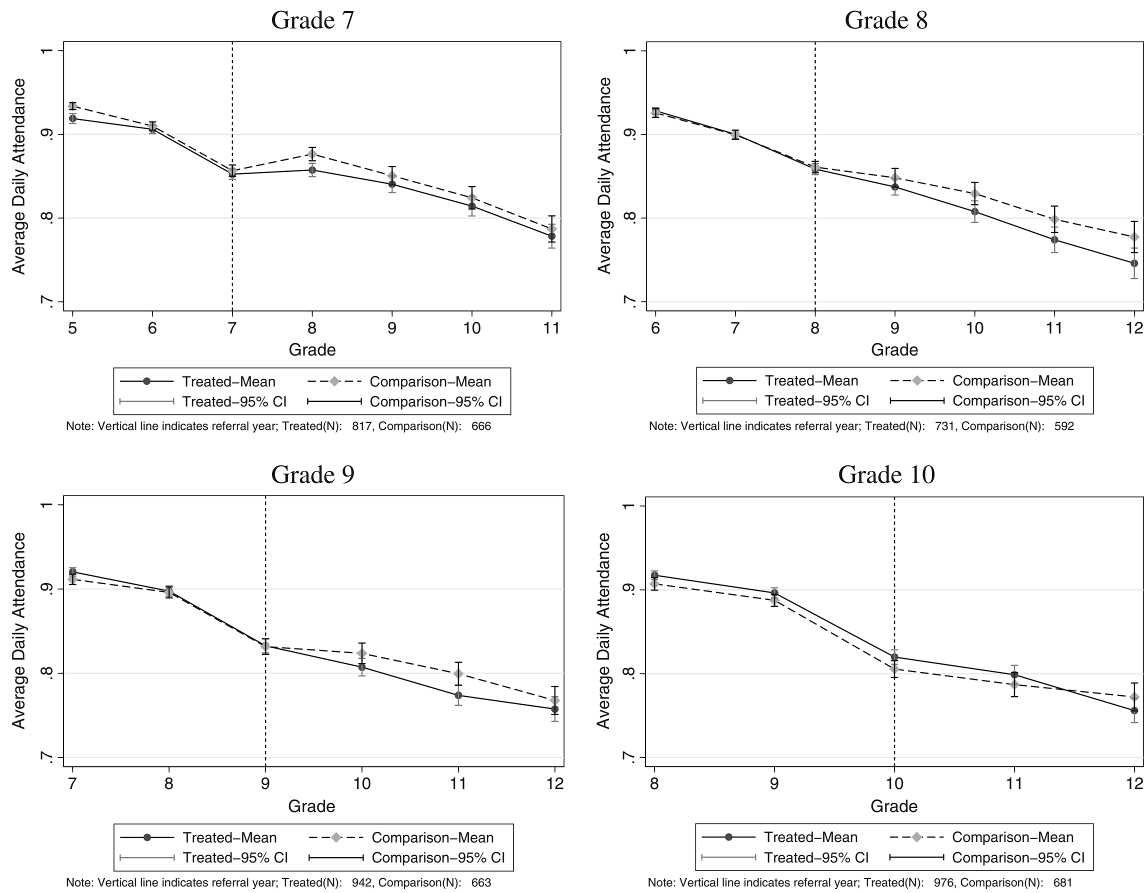

Fig. 2 Trends in average daily attendance rates in the intervention and matched sample created for the analysis of the effect of program referral on attendance

Analyses of Demographic Subgroups

We repeated the matching methods described above to create matched samples for several 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 reduced sample sizes. Even still, it was difficult to obtain matched samples that were well balanced on all covariates. We obtained adequately matched samples for students eligible for free lunch, females, males, and Black students. The findings for each subgroup were similar to the findings for the full sample.

Sensitivity Analyses

Matching on attendance in the year of referral was necessary because students referred to TIP, on average, experienced a sharp decline in their attendance rate in the year of referral (see Figs. 2 and 3), and this attendance decline could not be fully predicted by prior attendance or other pre-intervention characteristics of the students. However, average attendance in the year of referral was a function, in part, of attendance after referral to the program. Thus, matching on attendance in the year of referral risked biasing inferences about program effects.

To test for this possibility, we used daily (rather than annual) attendance data to estimate the short-term effect of referral to TIP in the months following referral. Daily attendance data allowed us to distinguish the timing of absences relative to the date of referral. A secondary advantage was that the daily absences were classified as excused or unexcused, which enabled us to match based on unexcused absences—the actual criteria for TIP referral. Unfortunately, the daily absenteeism data were available only for intervention school districts so we had to draw our matched samples from the same districts, and all students in the pool available for matching had the opportunity to be referred to TIP. Thus, unmeasured variables may have influenced selection to the program.

For the sensitivity analysis, we pooled the data across all grades and years and then identified matched comparison groups within each month within the academic year (September through May). Because most students were referred during the second semester of the academic year, we present results for students referred between February and

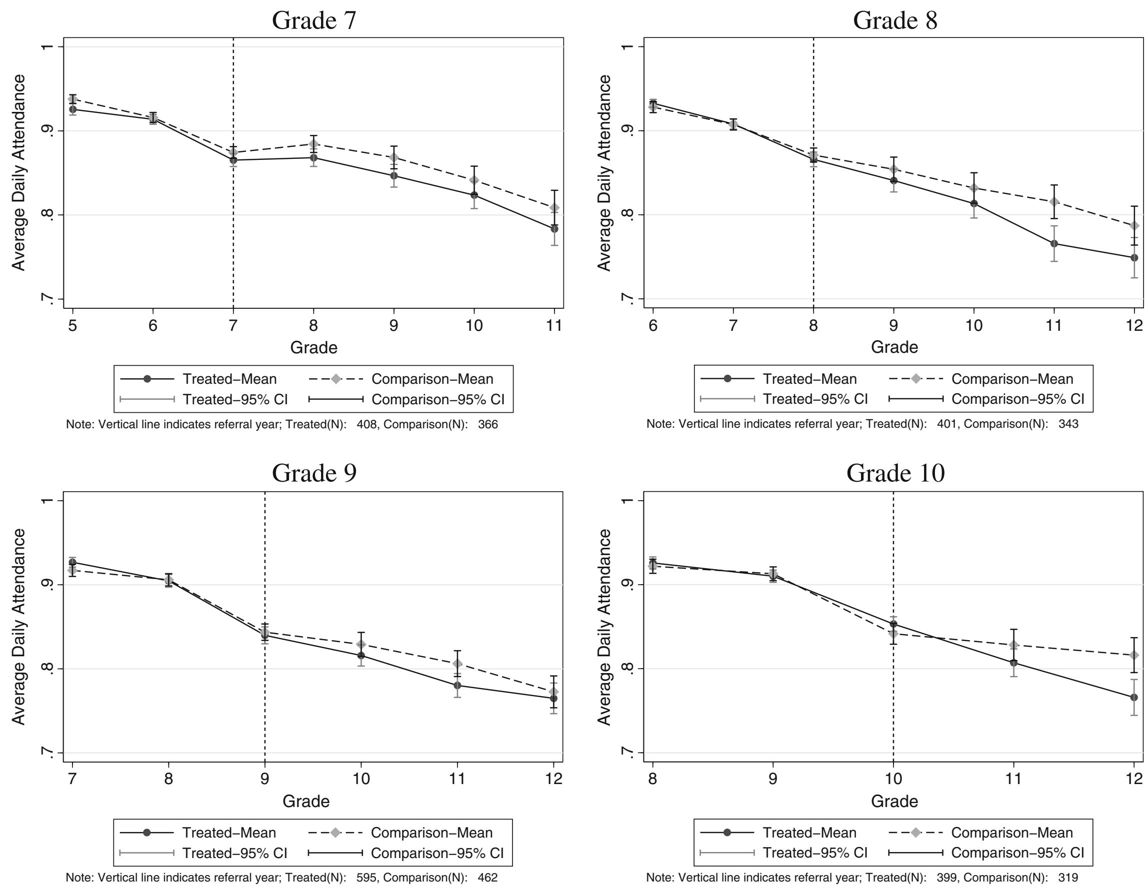

Fig. 3 Trends in average daily attendance rates in the intervention and matched sample for the analysis of the effect of program referral on attendance among the subset of students who attended the group parent meeting (step 1 of TIP)

April. We achieved good covariate balance on the same set of student characteristics presented in Figs. 2 and 3. Figure 4 presents the trend in the number of unexcused absences accrued each month for the months before and after the month of program referral. The intervention and control groups had nearly identical trends in the months prior to referral, suggesting that the parallel trends assumptions held. Unexcused absenteeism peaked in the month before the parent meeting, the point in time when school personnel typically made the TIP referral. The intervention and matched comparison groups had statistically indistinguishable attendance trends across all months after the TIP referral.

We repeated the analysis for excused absences and total absences and found the same pattern (i.e., no differences in attendance in the months following referral for either intervention or comparison groups). It was also possible that the findings were biased towards zero because some members of the control groups were potentially referred to the diversion program adopted in the comparison county in 2010. To test for this possibility, we repeated the analysis on the subgroup of students referred to TIP before 2008. The results were similar to the full sample.

Discussion

In this study, we evaluated the effectiveness of a common three-step court diversion strategy to improve attendance among chronically absent middle and high school students. Using matched sampling and dynamic DD models with population data, we found that involvement in TIP did not improve either short-term or long-term attendance among truant students in 7th–10th grades, relative to the comparison group of students from a neighboring county where the program was not available. Although in most years after the intervention, the intervention group had slightly lower attendance than the comparison group, most of the differences were statistically insignificant and there was no pattern of statistically significant negative effects across the different samples and different specifications of the intervention. This pattern of greater absenteeism in the intervention group was not robust enough to conclude that TIP caused attendance to decline.

Our findings differed from the findings of a recent randomized control trial studying the efficacy of a diversion program in Queensland, Australia (Mazerolle et al. 2017). Using a tightly controlled intervention with intensive researcher

Table 2 Estimated coefficients from dynamic difference-in differences models regressing attendance on program referral for the analysis of the effect of program referral on all students referred and on the subset of students whose parents attended the group parent meeting

	Grade 7		Grade 8		Grade 9		Grade 10	
	Referred	Attended Parent Meeting	Referred	Attended Parent Meeting	Referred	Attended Parent Meeting	Referred	Attended Parent Meeting
before 2 yr. + 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)
1 yr. 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)
1 yr. 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)
2 yrs. 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)
3 yrs. 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

Control variables: gifted/galented participation, eligible for free lunch, has an individualized educaion plan, and any child welfare involvement in the family, school mobility indicators

* significant at 10%; ** significant at 5%; *** significant at 1%

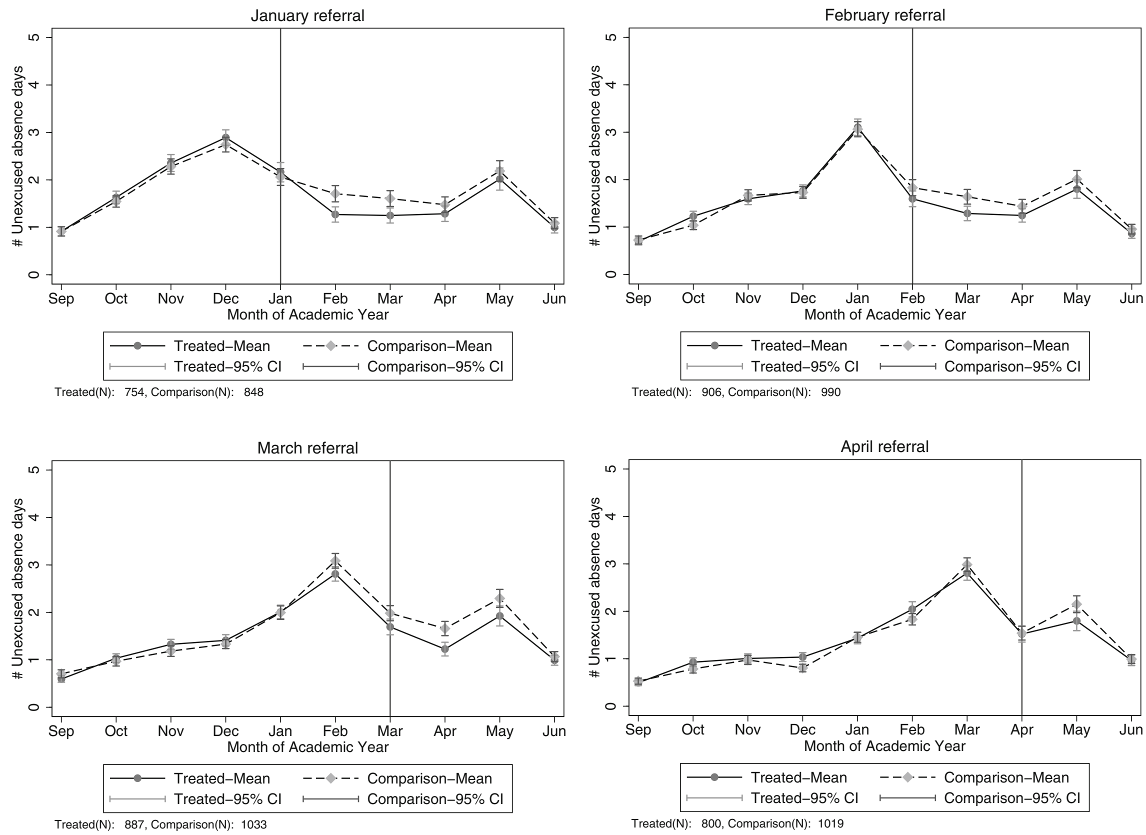

Fig. 4 Trends in the average number of unexcused absence days in the months following the parent meeting: intervention vs. matched comparison groups, by month of the parent meeting

involvement, Mazerolle and colleagues found that a diversion strategy incorporating principles of restorative justice increased attendance by approximately 30% in the three semesters following the intervention among students with similar rates of absenteeism as students referred to TIP (Mazerolle et al. 2017). One reason for differing results may be the utilization of restorative justice conferences and procedural justice protocols in the Australian study (Acutt 2017). The meta-analysis by Schwalbe et al. (2012) found that restorative justice was the only successful diversion approach to reducing absenteeism. In contrast to TIP, the restorative justice conferences included an adult in an authority position at the school, often a teacher, who had a relationship with the student and was personally affected by the students' absenteeism. This person followed a manualized protocol to describe how the student's chronic absenteeism personally affected their life (e.g., "I worried about you when you didn't come to class."), as well as the legal consequences of not attending school, and their belief that the student could improve attendance. Their approach incorporated developmental science by placing behavioral regulation within the context of a caring relationship with an adult who respects the young person's autonomy (Barber and Olsen 1997).

Another potential reason for differing findings may lie in the consistency of implementation. The authors of meta-analyses of juvenile diversion efforts found that the most effective programs were efficacy studies in which program implementation was closely monitored by researchers (Schwalbe et al. 2012; Wilson and Hoge 2013). The authors of two meta-analyses of truancy interventions implemented as part of tightly controlled randomized or quasi-experimental studies found multiple strategies to be modestly effective (Klima et al. 2009; Maynard et al. 2012). It is possible that any consistently implemented strategy that provides personal attention to truant students reduces absenteeism. The lack of data on implementation fidelity in our study makes it difficult to disentangle whether the null findings were due to theory, implementation fidelity, or both.

Our study had several limitations. First, other than TIP, we did not have documentation of truancy prevention strategies implemented by either the intervention or the comparison districts. Although a heterogeneous mix of services was offered in both counties, it is possible that the comparison county had more effective programming, thereby biasing estimates of program effects towards the null. Second, although we collected extensive anecdotal reports about program implementation, including referral decisions, we did not collect this information systematically enough to rigorously inform why the program 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 for the schools and deemed equally effective. In addition, about one in five eligible students were referred to the program. This non-representative group of students may be less responsive to the program relative to the typical eligible student. Future research regarding program implementation and the extent to which program quality and fidelity affects outcomes is needed.

Finally, it was possible that the counterfactual was non-equivalent to the intervention group on unobserved, time-varying characteristics related to the outcome, such as suspensions and expulsions. The consistent findings of null to negative program effects across two measures of program implementation, multiple subgroups, multiple comparison groups, and multiple matching procedures, gave us confidence that the findings were not spurious.

Implications for Practice and Research

Juvenile justice-based interventions were developed in response to compulsory education laws that distinguish between sanctioned and non-sanctioned reasons for missing school. Court diversion programs, as currently conceptualized in the USA, were implemented despite the lack of a strong theoretical base because they were less punitive alternatives to juvenile courts that still upheld the laws making non-sanctioned absences illegal. As long as state education laws define truancy as a status offense, justice-based responses are required. Thus, more research should be undertaken to determine whether our null finding for this common diversion model is the norm or an anomaly. Also needed are long-term efficacy and effectiveness studies of diversion models that incorporate theory-based principles, as did the intervention developed by Mazerolle et al. (2017). Finally, working with school districts to find systematic ways to track implementation fidelity, document other ongoing efforts to improve attendance, and collect information from parents and students about their experiences with the truancy intervention programs would be beneficial for future evaluations.

Even if effective, justice-based strategies, which often focus solely on unexcused absences, are not sufficient. One promising universal prevention strategy is offering behavioral nudges to parents and students to improve attendance through brief communications (Kraft & Rogers, 2015; Rogers et al. 2017). Effects of such nudges on attendance appear to be sensitive to the content, timing, and frequency of the communication (Balu et al. 2016), to whom the communication is sent (child, parent, or both) and the format (text, mailer, or phone). It is possible that adolescents who are chronically absent are responsive to well-crafted and timed messages, consistent with positive findings for elementary students (Robinson et al. 2018; Smythe-Leistico and Page 2018). Reanalysis of data from studies of nudging interventions to study program effects specifically for chronically absent students could help determine the potential of these interventions in middle and high schools.

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Compliance with Ethical Standards

Conflict of Interest The authors declare that they have no conflicts of interest.

Ethical Approval The institutional review boards of the University of Minnesota and the University of Tennessee, Knoxville, approved the study. In addition, the state and local agencies that shared the data approved the research. All analyses involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

Informed Consent This study used secondary administrative data linked and de-identified by the Minn-LInK project at the University of Minnesota School of Social Work. A waiver of informed consent was obtained by the appropriate institutional review boards.

Research Data Transparency Due to data sharing agreements with each state and local agency, the data used in this study are not permitted to be made available for public use. Individual researchers may send separate data inquiries to all parties involved.

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