Substance Use Disorder Treatment and Human Capital: Evidence from At-Risk Youth*

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Abstract

This paper studies the short- and long-run impacts of substance use disorder (SUD) treatment on human capital accumulation and labor market outcomes among at-risk adolescents. Specifically, I study the effect of one of the most common types of SUD treatment programs for adolescents—residential treatment center schools, which provide clinical SUD treatment and have a school on site. Using administrative data that link individual-level records across multiple government agencies in Texas, I examine within-individual changes in outcomes around the time of SUD treatment with a difference-in-differences design. I find that treated students experience declines in chronic absenteeism, disciplinary action, and course failure in the first two years following SUD treatment relative to a matched comparison group. I also find positive long-term impacts on college enrollment by age 20 and employment at ages 17–20. Heterogeneity analysis reveals that the positive impacts of SUD treatment center schools are nearly universal across demographic characteristics. My results suggest that this treatment option may be a promising tool to address SUDs and promote human capital development among at-risk youth.

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1 Introduction

Substance use disorders (SUDs) are a major and growing public health concern in the United States, and the rate of severe events associated with substance use has been dramatically rising. From 1999 through 2020, drug overdose death rates more than quadrupled; in 2020, about 91,800 people died from drug overdose deaths, translating to an average of more than 250 deaths each day (CDC, 2021). Beyond contributing to overdose deaths, SUDs may have profound effects on all aspects of individuals' lives and may have particularly far-reaching effects on adolescents suffering from these disorders. Adolescence is a critical period for brain development and for investments in health and human capital. It is also a time when many individuals initiate and increase alcohol and other substance use, and untreated SUDs in adolescence often persist into adulthood and may last decades (Kessler et al., 2005).

However, despite the numerous potential benefits from SUD treatment receipt during adolescence, it is estimated that less than 1 in 10 adolescents with a SUD have access to treatment (SAMHSA, 2019). As policymakers weigh expanding access to SUD treatments, it is critical to understand the effectiveness of these treatments among adolescents. However, little is known about how SUD treatment affects adolescents, in part due to data limitations and empirical challenges. This is a particularly important gap in knowledge given the large potential for SUDs in adolescence to derail an individual's life and the large prevalence of SUDs among adolescents.[1]

This paper aims to fill this gap by estimating the short- and long-run impacts of SUD treatment on adolescents' educational and labor market outcomes, focusing on a common type of SUD treatment program for adolescents—treatment center schools. These schools are residential centers that provide clinical treatment for SUDs and have a school on site. Using administrative data from Texas, I estimate the impacts of SUD treatment center schools among at-risk adolescents—specifically, youths aged 12–16 years who have previously been detained in a juvenile detention center. This population is of particular interest for two key reasons. First, SUDs are highly prevalent among

[1] In 2020, 6.3% of youth aged 12 to 17 met the criteria for a SUD (SAMHSA, 2021). The rate of substance abuse is even higher among at-risk youth. For example, a third of youth aged 10 to 17 in the juvenile justice system meet the criteria for a SUD (Aarons et al., 2001; Wasserman et al., 2010).

juvenile detainees.[2] Second, juvenile detainees represent a large share of the adolescents served by SUD treatment center schools.[3] More generally, the juvenile justice system is a leading source of referrals to both residential and non-residential SUD treatment services for adolescents. Between 2000 and 2018, about half of all admissions to SUD treatment for adolescents nationwide were referred by the justice system.[4] Therefore, a first-order question for understanding the impacts of SUD treatment programs for adolescents is to study their effects on the adolescent population involved in the juvenile justice system.

This paper uses longitudinal administrative data that link individual-level records across three state government agencies in Texas. These data cover the universe of all individuals ever enrolled in K–12 public schools in Texas, and the data include comprehensive information from individual K–12 educational records as well as linked information on juvenile detention records, SUD treatment center school attendance, college enrollment, and labor market outcomes in young adulthood. One of the key challenges in identifying the long-run impact of interventions in adolescence is the lack of data that follow individuals from adolescence to young adulthood. I overcome this challenge by using the linked data that allow me to provide a comprehensive analysis of the impacts of SUD treatment center schools. In my analysis, I include the universe of SUD treatment center schools in Texas between 1999 and 2018 and estimate the impact of these schools on short-run outcomes (e.g., attendance, course failure) and longer-run outcomes (e.g., completed secondary school education, college enrollment, and employment measured by age 20).

To estimate causal effects of attending a SUD treatment center school on short-run educational outcomes, I examine changes in outcomes around the timing of SUD treatment initiation by taking advantage of the high-frequency nature of the data. Specifically, I use a difference-in-differences approach in which I compare within-individual changes in outcomes for adolescents who entered a SUD treatment center school within six months following detention to those experienced by a matched

[2]About half of juvenile detainees meet the criteria for a SUD (Teplin et al., 2002; McClelland et al., 2004; Islam et al., 2020).

[3]Roughly a third of all adolescents who attended a SUD treatment center school in Texas between 2000 and 2018 were previously detained in a juvenile detention center.

[4]The Treatment Episode Data Set - Admissions, 2000–2018.

comparison group. To form a matched comparison group for each treated individual, I focus on individuals who were detained at the same time as the treated individual and who also suffered from a SUD, but were not enrolled in a treatment center school after detention. Among these matched controls, I use "exact" and "fuzzy" matching techniques to restrict attention to matches with the same basic demographic characteristics as the treated individual (e.g., age, gender, race/ethnicity, and socioeconomic status) and to exclude those with very different values of key measures at baseline (e.g., absence rate and juvenile detention history). The assumption underlying this research design is that, in the absence of SUD treatment center school attendance, outcomes among treated individuals would have trended similarly to those among the matched comparison group. I use the high-frequency data on educational outcomes to provide support for this assumption. Specifically, I illustrate that outcomes across the two groups evolved similarly in the months prior to detention and continued to evolve similarly after detention but before placement in the SUD treatment center school. The outcomes for the treatment group only diverge after enrollment in the treatment center school.

I find that attending a SUD treatment center school has a positive impact in the short run. Attending a SUD treatment center school reduces the share of school days that an individual is absent by 5.1 percentage points (or 28% relative to the control group mean); reduces chronic absenteeism by 12 percentage points (23%); and reduces the likelihood of not being observed within Texas public school system by 4.9 percentage points (11%) in the first two years following SUD treatment initiation. In addition, attending a SUD treatment center school leads to a 7.5 percentage point decrease in the likelihood of being disciplined in school (28%) and a 5.5 percentage point reduction in the course fail rate (16%) in the first two years following SUD treatment initiation.

I also analyze the impact of attending a SUD treatment center school on long-run outcomes—such as completed secondary school education, college enrollment, and labor market outcomes—through age 20. Since long-term outcomes are only observed once for each individual, it is not possible to analyze within-individual changes in these outcomes. Instead, I analyze long-run impacts by including match group fixed effects to compare outcomes between treated individuals and matched control individuals. In this specification, I also include additional controls—such as county fixed effects and other pre-detention characteristics—to account for any further potential differences between the treatment and control individuals.

The findings indicate that attending a SUD treatment center school has lasting positive impacts on educational and labor market outcomes through age 20. Attending a SUD treatment center school leads to a 4.4 percentage point increase in the likelihood of completing grade 10 (15.4% relative to the control group mean) and a 1.7 percentage point increase in the likelihood of grade 11 completion (10.2%). I find no statistically significant effect on high school graduation. To summarize the effects of treatment center schools on completed secondary education, I analyze the effect on the maximum grade level completed in secondary school. These results indicate that attending a SUD treatment center school leads to 0.11 additional years of schooling in secondary school.

I also find that treatment center school attendance substantially increases college attendance and employment through age 20. Attending a SUD center school leads to a 1.3 percentage point (12%) increase in the likelihood of enrolling in college by age 20. This increase is almost entirely explained by an increase in two-year college attendance among youth who would not have attended college. SUD treatment center attendance also leads to a 2 percentage point (2.7%) increase in the likelihood of being employed at ages 17–20.[5] This estimated increase in employment is large. It is roughly third the size of the estimated effect of moving children who live in distressed public housing to lower-poverty neighborhoods (Chyn, 2018) and twice the size of the estimated effect of a paid summer employment program (Gelber et al., 2016).

My study contributes to a growing literature quantifying the returns to SUD treatment services. Recent studies document that access to SUD treatment facilities leads to reductions in local crime, emergency visits, and drug overdose deaths (e.g., Bondurant et al., 2018; Corredor-Waldron and Currie, 2022; Swensen, 2015; Wen et al., 2017).[6] Prior work has focused on examining the short-run impacts of SUD treatment

[5] I find no evidence that SUD treatment center school attendance increases earnings at ages 17–20, though an important limitation is that earnings are only measured through late adolescence; any increases in lifetime earnings because of the estimated increases in educational attainment may not appear until later ages.

[6] Horn et al. (2021) investigate the impacts of SUD treatment centers on local property values and find no evidence that SUD treatment centers negatively affect local property values.

using aggregate data and has focused on SUD treatment available in the community at large—rather than SUD treatments aimed specifically at the adolescent population suffering from SUDs. This paper advances this literature in two key ways. First, this study provides the first causal estimates of the impacts of SUD treatment programs on adolescents. It is particularly important to understand the impacts of interventions targeted toward adolescents, given that interventions earlier in life tend to have larger impacts and are often more cost-effective than interventions targeting adults (Hendren and Sprung-Keyser, 2020).

Second, to the best of my knowledge, this paper also provides the first causal estimates of the long-run effects of SUD treatments more generally. My findings illustrate that SUD treatment among at-risk adolescents increases educational attainment in the short- and long-run, and back-of-the-envelope calculations suggest that projected increases in lifetime earnings based on these increases in educational attainment alone may be large enough to offset a substantial share of the costs of this treatment.

More broadly, my work contributes to a wider literature investigating the effect of interventions for at-risk children or children from disadvantaged backgrounds. Prior studies have investigated the impacts of interventions such as summer youth employment programs (e.g., Gelber et al., 2016), placement in disciplinary schools (Meiselman and Verma, 2021), moving children to lower-poverty neighborhoods (e.g., Chyn, 2018), and placing children who are abused or neglected into foster care (e.g., Bald et al., 2022; Doyle, 2007). Because SUDs are prevalent among disadvantaged youth, understanding returns to SUD treatment among this population is of particular interest to policymakers. The findings of this paper demonstrate that increasing access to SUD treatment center schools could be a promising tool to promote human capital development among at-risk youth.

Finally, my paper directly addresses issues in an active policy landscape. In response to the worsening substance use epidemic, President Biden's budget for fiscal year 2023 proposes historic investments to address the growth in SUDs, including large increases in funding for treatment services for adults and adolescents (White House, 2022). While it is increasingly important to understand the returns to SUD treatment programs, little is known about the impacts of SUD treatments—especially the impacts of these programs on adolescents. By providing causal estimates of the short- and long-run impact of SUD treatment center schools—a resource that is critical to at-risk adolescents with severe SUDs—this paper illustrates that SUD treatment services for adolescents may not only positively impact human capital development but also provide far-reaching benefits to the affected individuals and society more broadly.

2 Background

2.1 Substance Use Disorder Treatment Center Schools

SUD occurs when "the recurrent use of alcohol and/or drugs causes clinically significant impairment, including health problems, disability, and failure to meet major responsibilities at work, school, or home." (SAMHSA, 2022). SUD treatment for adolescents is delivered in many different settings, which fall within two categories: non-residential (e.g., early intervention services, outpatient treatment) and residential (residential/inpatient treatment, medically managed intensive inpatient treatment). Roughly a third of youth admissions to SUD treatment are for residential services,[7] which are aimed at individuals with severe SUDs.

One of the most common types of residential SUD treatment for adolescents is a SUD treatment center school. As of 2020, SUD treatment center schools represent 83% of all residential beds for SUD treatment among adolescents in Texas. SUD treatment center schools are nonhospital, licensed residential treatment centers that have a school on site. While there is some variation in the specific services provided by residential SUD treatment centers, all centers provide safe housing and medical care in a 24-hour supervised setting. These centers offer intensive care and support, including comprehensive evaluations, therapy, and an individualized treatment plan to meet individuals' specific behavioral and mental health needs (Somers et al., 2021). Beyond standard residential SUD residential treatment services, SUD treatment center schools

[7] Treatment Episode Data Set, 2000–2018.

also provide educational services based on standard, age-appropriate curriculum.[8,9] Compared with regular public schools, classrooms in these treatment center schools have low student-to-teacher ratios, allowing teachers to provide as much one-to-one assistance as possible (Letourneau, 2014).

2.2 Juvenile Detention and Assignment to a Treatment Center School

In this paper, I estimate the impacts of SUD treatment center schools among at-risk adolescents who have previously been detained in a juvenile detention center. Juvenile detention centers are primarily used to temporarily hold juveniles while they await a court hearing, disposition, or placement in a different facility.[10] In 2019, about 1 in 4 (26%) delinquency cases were referred to juvenile court involved detention, with the average length of stay of 27 days (Puzzanchera et al., 2017). Youth in juvenile detention have the right to education, and juvenile detention centers provide youth with access to educational programs (Umpierre, 2014).

Assignment to a SUD Treatment Center School In all states, juvenile detention facilities use mental health screening tools to identify youths with mental health and/or substance use disorders and youths who need further assessment.[11] Among juvenile detainees with an identified need for SUD treatment, some individuals are assigned to SUD treatment programs, including SUD treatment center schools. Juvenile detainees can be assigned a SUD treatment center school either by court order or by referral.[12]

[8] Coursework completed within a treatment center school leads to credits awarded by the associated school or school district, and some treatment center schools have authority to grant diplomas as well. For more details, see: `https://www.transformingyouthrecovery.org/wp-content/uploads/2017/09/ARS_The_State_of_Recovery_High_Schools_2016_Biennial_Report..pdf` (accessed October 2022).

[9] Youth in residential facilities have the right to education, regardless of the length of stay, and most residential treatment facilities have an on-site school with a standard age-appropriate education curriculum (Umpierre, 2014).

[10] For some cases (roughly 5-10%), juvenile detention centers are also used for longer-term, court-mandated treatment programs following post-trial sentencing (Baron et al., 2022). Although my analysis includes all detention cases, the results are qualitatively similar if I exclude the long-term detention episodes that are in the top 10% of the distribution of the detention length.

[11] One of the most commonly implemented screening measures in juvenile justice settings is Massachusetts Youth Screening Instrument-Second Version (MAYSI-2), which is a standardized screening tool for mental health needs of detained youths. The MAYSI-2 is a 52 yes/no item screening tool and only takes 15 minutes to administer and thus can be easily used in detention facilities.

[12] Among adolescents who entered a SUD treatment center school following detention in Texas between 2000 and 2018, about 40% entered the school by court order.

First, judges can order placement into a SUD treatment center school at the time of disposition. Second, juvenile detainees can be referred to a SUD treatment center school (either during detention or after release from detention) by other referral sources, including probation officers, healthcare providers, and schools. Among these sources, the major source of referral to youth residential SUD treatment programs is probation officers.[13,14] About 60–65% of justice-involved youths are mandated to probation on release, providing the juvenile probation officer a unique opportunity to connect youths to SUD treatment (Holloway et al., 2013; Hockenberry and Puzzanchera, 2020).

Access to SUD Treatment Services Judges (and probation officers) take into account the severity of an individual's SUD when deciding whether to require (or refer) the individual to attend a SUD treatment center school. However, beyond the severity of a SUD, several factors may influence judges' decisions to order (and probation officers' decisions to refer) juvenile detainees to SUD treatment. A key factor that may influence these decisions is the availability of services. While about half of juvenile detainees meet the criteria for a SUD, which is more than eight times higher than among the general adolescent population, only 10% of youth in the juvenile justice system who need SUD treatment services are connected to appropriate care (McClelland et al., 2004; Kelly et al., 2005; Teplin et al., 2002; McClelland et al., 2004; Islam et al., 2020). Aside from the current availability in local treatment center schools, other factors cause variation in the judges' (and probation officers') decisions, such as (i) their attitudes towards youth substance use and SUD treatment services, (ii) their perceptions about the availability and quality of services, (iii) established networks between service providers and the court, and (iv) clinical backgrounds of the decision-maker in SUD treatment services (Breda, 2001; Yurasek et al., 2021).[15]

[13] Between 2000 and 2018, referrals from the court/justice represent the largest share (47.5%) of all admissions to youth residential treatment programs, followed by an individual (17.7%), alcohol/drug use care provider (15.4%), other community referrals (11.3%), other healthcare providers (6.4%), school (1.5%), and employer (0.14%); among referrals from the court/justice system, the largest share (47.6%) are from probation/parole, implying that probation officers is the major source of referral to residential SUD treatment among youths in the juvenile justice system (Treatment Episode Data Set-Admissions, 2000–2018).

[14] It is important to note that SUD treatment services for adolescents are very costly and funding from the juvenile justice system is crucial for accessing these services (Ebener and Kilmer, 2003). It is much harder for adolescents outside of the juvenile justice system to receive SUD treatment services, partially explaining that the juvenile justice system is the major source of referral to youth SUD treatment.

[15] For instance, a probation officer may be unsure of where to refer youth for further evaluation and "ultimately just refer for mental health services or do not refer at all" (Yurasek et al., 2021).

As a consequence, there may be a large variation in the rates of referral to a SUD treatment center school across judges/probation officers and across time, even conditional on youths' severity of SUDs. This institutional feature is helpful for identification because there may be substantial overlap in the support of individuals who do and do not access SUD treatment center schools. My matched difference-in-differences approach builds on this institutional feature by identifying matched control individuals who did not attend a treatment center school but who appear otherwise comparable to individuals who did attend a treatment center school.

Timing of Treatment After Detention As noted above, treatment center schools are capacity constrained. These capacity constraints may impact both whether an individual is referred to a treatment center school and the timing of enrollment in the treatment center school after being released from detention. For example, some individuals may enter the SUD treatment center school immediately after release, but it can take several weeks to several months for other individuals to enter.[16] Figure 1 provides a graphical illustration of the timeline from the pre-detention period to the post-SUD treatment period among adolescents who attend a SUD treatment center school after detention. I define the intermediate pre-period as the period between the timing of placement into a detention center and the timing of enrollment in a SUD treatment center school. As discussed above, the length of the intermediate pre-period varies across individuals and can be as long as several months.[17]

3 Data

To estimate the impact of attending residential SUD treatment centers on educational and labor market outcomes among juvenile detainees, I use individual-level administrative data from several sources. The data cover the universe of public school records in Texas. These data are then linked to information on education (K–12

[16] Not only capacity constraints but also other systemic barriers can affect the timing of enrollment in the treatment center school. For example, probation officers sometimes "shop" for programs, making the adolescent available for multiple interviews by multiple programs (Ebener and Kilmer, 2003). This may delay the actual SUD treatment among adolescents after their release from detention.

[17] The length of the intermediate pre-period reflects both the length of detention and the time lag between release from detention and enrollment in a SUD treatment center. As described in Section 3.3, individuals in my analysis sample spend 17.3 days in a detention center on average.

education, college education), juvenile detention, residential SUD treatment center school attendance, and employment/earnings. This section describes each of the administrative data sources and outcome variables I construct for the analysis.

3.1 Individual Educational and Labor Market Outcomes

Educational Outcomes. I use administrative microdata on educational outcomes that are obtained from two sources. First, I use data from the Texas Education Agency (TEA) that cover all students in all public K–12 schools in Texas over the academic years 1996–1997 through 2019–2020. The TEA data contain information on students' attendance, graduation, type and reason for disciplinary actions, course completion and pass/fail results, standardized test scores, and a reason for leaving Texas public school system. The data further contain information on student characteristics, including age, gender, race/ethnicity, disability, and eligibility for free or reduced-price lunch.

TEA data on enrollment and disciplinary actions are available for each student and for 6 six-week grading periods in a given academic year.[18] Using these records, I construct the following five outcomes: (1) a continuous absence rate, which I measure as the ratio of the number of days absent to the number of days a student is enrolled in any school in Texas public school system; (2) an indicator for chronic absenteeism, which I define as a continuous absence rate being equal to or larger than ten percent; (3) an indicator denoting being enrolled in any school in Texas public school system; (4) an indicator for whether a student is chronically absent or not in Texas public school system (which is a combined measure of (2) and (3)); and (5) an indicator for whether any disciplinary action is taken against a student in school. Note that the outcomes (1), (2), and (5) are defined by conditioning on being observed in Texas public school system, while the outcomes (3) and (4) are not. TEA data on course completion and course pass/fail results are only available at the academic year level. Using these data, I construct an additional outcome: course pass rate, which I define as the ratio of the number of courses passed relative to the number of courses completed. I also construct an indicator for graduating high school by age 20, using TEA data on whether and at

[18] If a student switches schools within a given six-week grading period and within the Texas public school system, the TEA data contain separate enrollment and disciplinary action records for each student and for each school.

what age a student graduated.

Second, I use the Texas Higher Education Coordinating Board (THECB) data that cover all students in all public and most private institutions of higher education in the state of Texas. The THECB data are linked to the TEA data at the individual level. Using the THECB data, I construct the following outcomes measured through age 20: (1) an indicator for ever having enrolled in any college; (2) an indicator for ever having enrolled in a two-year college but not in a four-year college; and (3) an indicator for ever having enrolled in a four-year college. THECB data do not contain information on out-of-state college enrollment or enrollment at some private institutions in Texas.

Labor Market Outcomes. I use administrative, quarterly microdata on employment and wage for all workers covered by Unemployment Insurance (UI) obtained from the Texas Workforce Commission (TWC).[19] The TWC data are linked to the TEA and THECB data at the individual level. Using the TWC data, I construct the following outcomes at ages 17–20: (1) an indicator for being employed, measured as having positive wage in any quarter; (2) average annual earnings (including zeros), measured in 2020 dollars. When an individual is not identified as being employed in a given year, her annual earnings are coded as zero. I do not have information on out-of-state employment.

3.2 Residential Substance Use Disorder Treatment Centers

Since SUD treatment center schools have a school on site, all students enrolled in these schools are included in the TEA data. I identify residential SUD treatment centers using data from the Texas Department of State Health Services (DSHS). The Texas DSHS provides a document listing all licensed SUD treatment facilities in Texas (DSHS, 2019). For each facility, the data report license number, county, the name of the provider, address, the number of residential beds, the number of outpatient slots, setting(s) provided (outpatient, detoxification ambulatory/outpatient, residential detoxification, intensive residential, supportive residential), and gender and age group(s) served (adolescent, adult) for each setting. In addition, I use the Health Treatment Services Locator database provided by the Substance Abuse and Mental Health Services

[19] For more details, see `https://www.twc.texas.gov/tax-law-manual-chapter-3-employer-0`.

Administration (SAMHSA) to further obtain information on facility operation (e.g., private, public), payment/insurance/funding accepted, treatment approaches (e.g., anger management), and other service details.

During my sample period, there were 14 residential SUD treatment center schools in Texas with a total of 428 residential beds.[20] As described above, the TEA K–12 education data contain detailed information on enrollment (e.g., days enrolled, days absent) for each student and for 6 six-week grading periods in a given academic year. This allows me to identify whether and for how long a student is enrolled in a school at a SUD treatment center. Appendix Figure A3 presents the distribution of length of stay within a SUD treatment center school.[21] On average, individuals stay 49 days in a SUD treatment center school.

3.3 Juvenile Detention Centers

In this paper, I focus on estimating the impact of SUD treatment schools among at-risk adolescents who were previously detained in a juvenile detention center; I compare changes in outcomes between adolescents who did and did not attend a SUD treatment center school after detention. As mentioned above, juvenile detention centers are required to provide youth with access to education. Most juvenile detention centers have a school within their facilities, and thus are included in the TEA data. I identify 37 juvenile detention facilities in the TEA data using data from the Texas Juvenile Justice Department (TJJD) that list all registered pre-adjudication juvenile detention facilities in Texas.[22] Since the TJJD data only list currently active facilities, I use data from several additional sources, including county websites, and identify 11 additional facilities that were ever active during my sample period. The final analysis sample includes 48 juvenile detention facilities in Texas. The TEA data on enrollment allows me to identify whether

[20] In fact, there were 20 residential SUD treatment centers during my sample period (i.e., 2000–2018). Six out of 20 residential SUD centers for adolescents do not have an on-site school accredited by the TEA and thus are not included in the TEA data. Most of these six facilities are either small in size or specifically designed for adolescents under exceptional circumstances. One facility is specifically designed for pregnant or newly parenting adolescents. Four facilities are small in size—the number of residential beds ranges from 12 to 16. Small facilities may choose to provide formal education through partnerships with local schools in the community rather than to provide education by an on-site school.

[21] The length of stay is winsorized at 180 days (i.e., about one academic year).

[22] For more details, see: `https://www2.tjjd.texas.gov/publications/other/searchfacilityregistry.aspx` (accessed July 2022).

and for how long a student is enrolled in a school within a juvenile detention facility in a given period. Appendix Figure A4 shows the distribution of length of detention for 48 juvenile detention centers in my sample. The average length of detention is 17.3 days and half of the sample are detained for less than 13 days.

4 Empirical Design

To estimate the causal effects of attending a SUD treatment center school on educational and labor market outcomes, I employ a difference-in-differences research design in which I compare within-individual changes in outcomes following SUD treatment to those experienced by adolescents who have the same basic demographic characteristics and suffer from a SUD but did not enroll in a SUD treatment center school after detention. In this section, I begin by describing my sample and the treatment group. I then discuss how I form a matched comparison group for each treatment individual. Finally, I explain my empirical strategies for the short- and long-run analyses.

Sample and the treatment group In my analyses, I focus on adolescents who are detained in a juvenile detention center at any point between ages 12 and 16 over the academic years 1999–2000 to 2017–2018.[23] I make two more sample restrictions. I exclude adolescents who are detained for more than 95 days (i.e., greater than value at 99th percentile in terms of length of detention). And, I restrict attention to individuals those who are observed in the TEA data for at least 3 six-week grading periods—about half of an academic year—during the last year prior to detention.[24] Within this sample, I define the treatment group as individuals who enter a SUD treatment center school within three grading periods (about six months) after being placed into a juvenile detention center.[25]

[23] Although the TEA data cover years 1992–2020, I restrict attention to detention episodes between 1999 and 2018 for two reasons. First, data on disciplinary actions are only available from the academic year 1998–1999 onward. Given that these data are used to construct key measures in my analysis, I focus on detention episodes in or after the academic year 1999–2000. Second, I restrict the sample to detention episodes through 2018 to follow individuals over time in the first two years following SUD treatment.

[24] About 8% of the sample are observed for two or fewer grading periods and thus are dropped by this restriction.

[25] Among adolescents who attended a SUD treatment center school within six grading periods (i.e., a year) following juvenile detention between 2000–2018, 71.2% entered the center within three grading periods. Since I do not have individual-level data on referral sources or the timing of referrals, I focus on

4.1 Control Group Construction

Individuals who attended a SUD treatment center school are likely to differ from those who did not attend in many aspects. For example, some of the individuals who did not attend a SUD treatment center school may not suffer from a SUD and thus not need SUD treatment. In Appendix Table A1, I investigate how these individuals differ in observable characteristics. The table presents average individual characteristics and academic performance measured at baseline for individuals who enroll in a SUD treatment center school following detention (column (1)) and all individuals who were detained between my sample period (column (3)). The sixth column presents the differences between mean characteristics between these two groups, and the seventh column presents $p-$values from tests of these differences.

A comparison of columns (1) and (3) indicates that individuals who enter a SUD treatment center school differ from the average juvenile detainee in a number of observable characteristics, including demographic characteristics (e.g., gender and race/ethnicity) and academic performance (course pass rate and standardized test scores in reading and math) at baseline. Compared to the average juvenile detainee in Texas, individuals who enter a SUD treatment center school after detention are less likely to be female, Black, eligible for free/reduced-price lunch, and in a special education program; and more likely to be White, Hispanic, and in a large central metro; and slightly older. In addition, Panels B and C show that treatment individuals have higher absenteeism, longer detention history, and lower academic performance at baseline, which may reflect that treatment individuals have a severe SUD, while some of the others may not suffer from a SUD prior to detention.

The key assumption underlying my difference-in-differences design is the parallel trends assumption. However, the substantial differences between detainees who did and did not attend a SUD treatment center school in a number of observable characteristics and academic performance at baseline raise concerns about differential trends between these individuals. To reduce these concerns, I match each treatment individual with a set

adolescents who enter a SUD treatment center school within three grading periods after being placed into a detention center to mitigate concerns about unobserved shocks other than detention around the timing of SUD treatment.

of individuals who suffer from a SUD and are similar in the basic demographic characteristics but did not attend a SUD treatment center school during my sample period (1997–2020). For this match, I first restrict attention to control individuals with a SUD. I then use both exact and fuzzy matching methods together to identify individuals who are similar in observable characteristics. In the baseline analysis, I focus on the control sample that I obtain using both exact and fuzzy matching methods, in Section 5.3, I discuss the robustness of my results to omitting the fuzzy matching procedure or to using an alternative, non-matching based approach. Below, I explain each step I conduct to form a matched comparison group.

Step 1. Identifying control individuals with a SUD Not all detainees need SUD treatment services. To identify control individuals who are likely to meet the criteria for a SUD and be eligible for SUD treatment services, I use an indicator for being *disciplined for substance-related problems* in school during the two years prior to detention as a proxy for having a SUD.[26] As a result, my control group consists of individual who did not attend a SUD treatment center school but who were disciplined for substance-related problems during the two years prior to detention. Note that I do not make this restriction for the treatment group because I consider all treatment individuals as having a SUD regardless of their substance-related disciplinary action history.[27] However, I show that the results are very similar in magnitude if I make this restriction for the treatment group as well, but the confidence intervals are slightly wider (see Section 5.3).

Appendix Table A1 shows that how treatment individuals (column (1)) differ from control individuals with substance-related discipline history (column (2)) in a wide range of observable characteristics. The fourth column presents the differences between

[26]The TEA data on disciplinary actions contain information on the type of and the reason for each individual and for each disciplinary action. Disciplinary actions for substance-related problems are defined by combining the following discipline action reason codes: (1) marijuana or controlled substance or dangerous drug, (2) alcohol, and (iii) abuse of a volatile chemical. For more details, see: `http://ritter.tea.state.tx.us/peims/standards/1314/app_additional_information_related_to_discipline_actio.html`.

[27]Another reason is that if I make the same restriction for the treatment group, it substantially cuts the sample size and reduces statistical power. About 39% of treatment individuals are disciplined for substance-related problems in the pre-detention period. This means that 61% of them will be dropped if I make the same sample restriction for the treatment group. Given that the results are robust to making the same restriction for the treatment group (see Section 5.3), my baseline analysis focuses on including the full treatment sample to increase the sample size.

mean characteristics between these two groups, and the fifth column presents $p-$values from tests of these differences. A comparison of columns (1) and (2) for characteristics in Panel C suggests that my proxy for a SUD is successful at identifying individuals who have similar academic achievement at baseline. However, even after focusing on control individuals who are likely to be eligible for SUD treatment, I still see large differences in demographic characteristics (Panel A). Compared to control individuals with substance-related discipline history, individuals who enter a SUD treatment center school after detention are more likely to be female and White; less likely to be Hispanic, Black, eligible for free/reduced-price lunch, and in a special education program. These large differences in demographic characteristics motivates me to use the matching methods to further restrict the control sample to individuals who are similar in these characteristics. As noted above, I also discuss the robustness of my results to omitting the fuzzy matching or to using an alternative, non-matching based approach in Section 5.3.

Step 2. Exact matching on basic demographic characteristics I use the exact matching on the following matching variables: (1) the timing of detention (e.g., a treated and the matched control units are detained in the same six-week grading period in the same academic year), (2) gender, (3) race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other), (4) age (no more than one year gap), (5) eligibility for free/reduced price lunch (measured in the last two years prior to detention), (6) indicator for being enrolled in a special education program (measured in the last two years prior to detention) and (7) urbanicity category based on county of detention center.[28,29]

Step 3. Refinement using the fuzzy matching Finally, I do the fuzzy matching to exclude exact matches with very different values in terms of key measures at baseline. Specifically, I use the following three variables: (1) mean absence rate in the year prior to detention,[30] (2) the number of six-week grading periods in which an individual is ever

[28]Counties are categorized using 2013 National Center for Health Statistics (NCHS) Urban-Rural Classification Scheme for Counties. This county-level scheme consists of four metropolitan (large central metro, large fringe metro, medium metro, and small metro) and two nonmetropolitan (micropolitan and noncore). For more details, see: `https://www.cdc.gov/nchs/data_access/urban_rural.htm` (accessed August 2022).

[29]The TEA data do not contain information on home address. I use county of a detention center as proxy for home address.

[30]More exactly, this is measured in the 6 six-week grading periods prior to detention.

detained, measured two years prior to detention, and (3) the number of six-week grading periods in which an individual is ever detained that is measured one year prior to detention. There are several different ways to calculate distance between each treated individual and a control individual. For example, a single matching variable can be used to measure the level of similarity in terms of that variable, while all fuzzy matching variables can be used together to measure overall similarity. I use the former approach to construct a comparable control group for each treated individual, but my results are qualitatively similar if I use the latter approach instead. My fuzzy matching takes the following steps. First, I calculate the distance (i.e., the absolute value of difference) between a treated individual and a control individual, separately for each of the three fuzzy matching variables. Then, for each matching variable, I drop control units with outlier distance values, defined as values greater than 90 percentile.[31]

I define a "qualified" match as an exact match with non-outlier distance values in terms of the three fuzzy matching variables. Out of 5,182 treatment individuals, 863 individuals do not have any exact matches; and 285 individuals have at least one exact match but do not have any qualified matches; and 4,034 individuals have at least one qualified match. The final analysis sample consists of the 4,034 treatment individuals and 35,714 matched control individuals.[32]

Table 1 reports average individual characteristics and academic performance measured at baseline for the 4,034 treatment individuals included in the final analysis sample (column (1)) and the matched control individuals (column (2)). The third column presents the differences between mean characteristics between these two groups, and the fourth column presents $p-$values from tests of these differences. The two groups are identical in characteristics used in the exact matching (except for age, for which I allow for a one-year difference), and similar in characteristics used in the fuzzy matching as well as academic performance measured in the pre-detention period, which I do not use for the matching. Note that my difference-in-differences research design is not based on the assumption that the two groups are identical in all dimensions. The key

[31] About 19% of the exact matches are dropped.

[32] Appendix Table A2 reports average individual characteristics across (i) the baseline treatment sample (those with at least one qualified match), (ii) treatment individuals with at least one exact match but no qualified matches and (iii) treatment individuals with no exact matches.

identification assumption of my research design is the parallel trends assumption, and I will discuss the validity of this assumption in Section 5. In all regressions throughout my analyses, each control unit within a given match group is given equal weight, and these weights are summed up to one. Treated individuals are assigned a weight of one.

One might have concerns about using absence rate, which is itself used as an outcome variable, as a matching variable. There are several things to note that assuage these concerns. First, in Section 5.3, I show that the results are very similar if absence rate is excluded from the fuzzy matching procedure. Second, I only match on the level of mean absence rate measured in the year prior to detention, not the trend in absence rate during the entire pre-detention period. This allows me to assess whether absence rate among the treated and matched control group evolves similarly prior to juvenile detention. Third, in Section 5.3, I show that outcomes that are not used for the matching—including course fail rate and disciplinary action history—are very similar across the treated and control groups both in level and trend before SUD treatment.

4.2 Short-Run Analysis

In the short-run analysis, I examine how residential SUD treatment impacts academic outcomes in the short run, focusing on outcomes that can be measured both before and after SUD treatment for each individual (e.g., attendance and course pass rate). For the short-run analysis, I restrict attention to individuals who were in or below grade 10 at the time of detention in order to follow adolescents two years before and after SUD treatment (i.e., 12 six-week grading periods before and after SUD treatment). As described above, I do not require that students stay in the Texas public school system before or after detention, as long as they are observed for at least 3 six-week grading periods in the last pre-detention year. I use this sample to estimate difference-in-difference models in which I measure within-individual changes in outcomes following SUD treatment, relative to the matched control individuals. My difference-in-differences specification is:

$$Y_{igt} = \rho Treatment_i \times Post_{gt} + \alpha_{gt} + \delta_i + \varepsilon_{igt}, \tag{1}$$

where Y_{igt} is an outcome in period t for adolescent i who is in match group g. $treatment_i$ is an indicator for individuals who attended a SUD treatment center. For each match group, $Post_{gt}$ is defined as an indicator for periods during or after the period of the treated individual's SUD treatment initiation. Note that this indicator turns on not only for a treated individual but also for control individuals within the same match group during the post-SUD treatment period. I include match group–by–time fixed effects, α_{gt}, which flexibly account for time trends in the outcomes within each match group. I also include individual fixed effects, δ_i, which account for time-invariant differences between treatment and control individuals. Standard errors are clustered at the individual level. The key coefficient of interest is ρ, which summarizes the difference in the change in outcomes following SUD treatment between treatment and control individuals within each match group.

The key identifying assumption of the difference-in-differences approach is that in the absence of residential SUD treatment, outcomes would have evolved similarly for treated and control individuals in each match group. To assess the validity of this assumption, I plot raw trends in outcomes between treated and control individuals and conduct an event study analysis. My event study regressions take the following form:

$$Y_{igt} = \sum_{k=-12, k\neq -6}^{12} \gamma_k Treatment_i \times \mathbf{1}\{t - E_i = k\} + \sigma_{gk} + \mu_i + \nu_{igt}, \tag{2}$$

where E_i is the period when individual i initially received SUD treatment. $k = t - E_i$ are periods relative to the time of a treated individual's SUD treatment initiation. A negative k denotes $|k|$ periods prior to SUD treatment initiation. Again, note that every control individual has non-missing values for E_i and $k = t - E_i$, which are defined based on the period in which the treated individual in their match group initially received SUD treatment. I also include a full set of match group–by–relative time fixed effects, σ_{gk}, to flexibly account for match group–specific trends in outcomes, as well as individual fixed effects, μ_i.[33] The four periods before SUD treatment initiation is used as the reference period.[34] Any observations outside the +/- 12 event time window are dropped. The key

[33]Time fixed effects will be absorbed since I include a full set of match group–by–relative time fixed effects.

[34]As described above, individuals receive SUD treatment 0 to 3 grading periods after detention. This means that they were placed in a detention center in relative period -3 (i.e., three grading periods prior to

coefficients of interest are γ_k, which summarize the within-individual changes in outcomes relative to the reference period among adolescents who received residential SUD treatment, compared to the matched control individuals who did not receive residential SUD treatment. Standard errors are clustered at the individual level as in equation (1).

4.3 Long-Run Analysis

In my long-run analysis, I estimate the impact of residential SUD treatment on educational and labor market outcomes that are measured in early adulthood. For the long run analysis, I focus on adolescents in my sample who are aged 20 or older as of 2020, the last year of my sample period.[35,36,37] The latter requirement on age leads me to focus on the treated individuals who received residential SUD treatment during the academic years 1999–2000 and 2016–2017. My final long-run analysis sample consists of 3,252 treated individuals and 28,723 matched control individuals (10,841 unique individuals). Again, each control individual in the same match group is given equal weight and weights are summed up to one. Treated individuals are assigned a weight of one.

Since an individual's long-run outcomes can only be observed after the SUD treatment, I cannot include individual fixed effects. Instead, my econometric model includes fixed effects for match group so that I can measure the difference in the outcomes between a treated individual and the matched controls. My long-run analysis specification takes the following form:

$$Y_{igc} = \lambda Treatment_i + \pi_g + \omega_c + \kappa' X_i + \xi_{igc}, \tag{3}$$

detention). I choose relative period -6 as the reference period, because this period is a pre-detention period for all individuals in my sample.

[35]To be specific, I include adolescents who are detained at some point between ages 12–16 during the academic years 1999–2000 and 2017–2018 (i.e., the same restriction used in the short-run analysis) and aged 20 or older as of 2020.

[36]I only look at outcomes through age 20 because the sample size significantly decreases as I extend to later ages. For example, some treatment center schools opened around 2015–2016, and I am only able to observe ages through 16–20 for individuals who attended those centers. To include all treatment center schools in my analysis, I focus on outcomes measured through age 20 in the baseline analysis.

[37]The TEA provides annual data on an individual's age as of September 1st. Since I do not have information on the date of birth, I assume that each individual was born on September 1st. In my long-run analysis, I limit my sample to those who are aged 20 or older as of September 1st, 2019.

where Y_{igd} is an outcome for individual i in match group g who was detained in juvenile detention center d at the time of detention. $Treatment_i$ is an indicator for individuals who attended a residential SUD treatment school. I include match-group fixed effects, π_g, to account for differences between match groups. I also include a vector of individual-level controls, X_i, including length of detention and an indicator for ever being employed in the pre-detention period. X_i also includes three pre-detention measures (absence rate, juvenile detention history one year prior to detention, juvenile dentention history two years prior to detention) that were used for the fuzzy matching procedure, which account for potential differences between the treatment and control individuals in the pre-detention period. To understand the difference between my short- and long-run analysis models, I estimate the impact of SUD treatment center schools on my short-run educational outcomes using both models and compare the estimates (see Section 5).

5 Results

5.1 Short-Run Effects on Educational Outcomes

Raw Trends in Outcomes In Panels (a) in Figures 3–8, I present raw trends in my short-run outcomes from 12 six-week grading periods before (i.e., about two academic years) to 13 six-week grading periods after the time of SUD treatment initiation (event time 0), separately for treated and matched control individuals. All individuals in the sample are placed into a juvenile detention center at some point between event time -3 and 0, the shaded area, and the average length of detention is 17.3 days. Therefore, I define event time periods between -12 and -4 as the pre-detention period. Event time periods 0 to +12 are defined as the post-treatment period. Raw data trends indicate that for all the short-run analysis outcomes, the treated individuals are trending similarly to the matched control individuals during the last 12 grading periods prior to SUD treatment initiation, providing evidence in support of the parallel trends assumption. It is also important to note that not only the trends but also the levels are also very similar across the two groups in the entire pre-detention period.

Panel (a) in Figure 3 shows raw trends in enrollment in a SUD treatment center school over time. The outcome variable is an indicator denoting whether an individual is enrolled in a SUD center or not. In event time zero, all treated individuals are enrolled in a SUD treatment center school, and then the fraction of treatment individuals staying in the center goes down over time as they leave the center. The average length of stay is about 49 school days (or about one and a half six-week grading periods). During the periods in or after event time +4, the share of treated units who are in a SUD treatment center school is 10.8%, which means that less than 10.8% spend more than 4 six-week grading periods (about two-thirds of an academic year) in a center.

Raw trends in the continuous absence rate and an indicator for chronic absenteeism are presented in Panels (a) and (c) in Figure 5, respectively. Absences are increasing in event time prior to detention for both groups. This may reflect the fact that absenteeism increases with age as well as the fact that an adolescent's risk for delinquency is likely to increase over time prior to detention. However, starting with the period of SUD treatment initiation, I see a divergence in these trends, with individuals who enter a SUD treatment center having a large drop in absences and chronic absenteeism. I see the largest drop in absenteeism in the first two grading periods following event time zero.

While absence rate and chronic absenteeism are measured by conditioning on being observed within the Texas public schools system, outcome variables used in Figure 6 are measured for all individuals and for all event time periods. In Panel (a), the outcome is an indicator for not being observed in the public school system, and in Panel (c), the outcome is an indicator denoting whether an individual is either chronically absent from school or not observed in the public school system. In Panel (a) in Figure 6, I see that the share of adolescents who are not observed in the public school system begins to increase around the time of detention among both the treated and control groups.[38] Then, in event time zero, this outcome becomes zero among the treated group as they enter a SUD center school. Similarly, Panel (c) shows that there is a sudden, large drop in the likelihood of chronic absenteeism or not being in the public school system in event time zero, and the magnitude of the effect becomes smaller over time.

[38] As noted above, I restrict my sample to individuals who are observed for at least three grading periods during the six grading periods prior to detention. I do not make any further restrictions on enrollment in the public school system.

For the analyses using disciplinary action outcomes throughout the paper, I only include match groups where the treated individuals were ever disciplined for a substance-related problem in the pre-detention period—the same restriction made for the control individuals—to make both groups more comparable, though I also present the results using the full sample in Appendix Figure A7. Panel (a) in Figure 7 indicates that the treated individuals are less likely to be disciplined in school following SUD treatment. In fact, these raw trends in any disciplinary action are very similar when I use the full sample—individuals with and without substance-related disciplinary action history (see Panel (a) in Appendix Figure A7). This implies that treatment individuals who were never disciplined specifically for substance-related problems were often disciplined for other reasons, making the overall likelihood of any disciplinary action very similar across these individuals and the matched control individuals.

Next, in Panel (c) in Figure 7, I show that the likelihood of being disciplined for substance-related reasons is also trending similarly across the treated and control groups in the pre-detention treatment period. One concern with using substance-related disciplinary action as a proxy for SUD among control individuals is that the severity of SUD may trend differentially over time across the treated and control groups. Panel (c) in Figure 7 indicates that the likelihood of being disciplined for substance-related problems was trending very similarly in the entire pre-treatment period, providing supportive evidence that the risk of substance use and delinquency evolves similarly over time across the two groups prior to the time of SUD treatment initiation.

Finally, Figure 8 plots the trends in course fail rate from three years before to two years after the SUD treatment initiation. Note that I use yearly-level data for the course fail rate outcome because the data on course completion is only available at the academic year level. Panel (a) in I see that the average course fail rate for the treated group decreases beginning in the year of treatment relative to the matched control group, and this effect persists over the first two academic years.

Event Study Results Panels (b) of Figures 3–8 plot the regression analogue to raw trends presented in Panels (a). I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2). Event study estimates indicate that the interpretations from raw data trends are robust to the regression adjustment (i.e., inclusion of individual and match group–by–time fixed effects). For all the outcomes examined in the short-run analysis, there are no statistically significant differences between the treated and matched control individuals in the pre-treatment period, providing evidence in support of the parallel trends assumption.

Panels (b) and (d) in Figure 5 indicate that treated individuals experience declines in absenteeism following SUD treatment.[39] Although a drop in absence rate would be partially mechanical, given that individuals in a treatment center school attend a school within a residential facility, understanding the impact of SUD treatment attendance on absenteeism is particularly important for several reasons. First, treated individuals experience a large drop in absenteeism in the first two years after SUD treatment. This could be a key channel through which SUD treatment center schools can have a longer-term impact on the treated individuals. For instance, decreased absence rate can affect the likelihood of graduating high school or the maximum grade level completed in secondary school, which I will investigate in Section 5.2. Second, it is important to understand whether SUD treatment center school attendance has a persistent impact on absenteeism in the post-discharge period. Although it is difficult to isolate the impact of treatment center school attendance in the post-discharge period because how long an individual stay in a treatment center school is endogenously determined (i.e., it reflects treated individuals' behaviors and choices), a simple analysis suggests that treated individuals experience a persistent drop in absenteeism even after they leave SUD treatment and attend other public schools (see Appendix Figure A8).[40]

[39] To understand how the numerator and denominator of the absence rate change, I present in Panel (a) and (b) in Appendix Figure A6 raw trends in the number of days absent and the number of days enrolled, respectively, which are measured while an individual is enrolled in either public school system or school within a juvenile detention center. Changes in absence rate for the treated and control groups mostly reflect the changes in the number of days absent. Note that the total number of days enrolled in a given six-week period decreases over time for both groups, partially driving an increasing trend in absence rate in the post-treatment period.

[40] Note that some individuals are absent from school while they are enrolled in a SUD treatment center school, though the absent rate within SUD treatment center schools is relatively low on average. Appendix Figure A5 shows the distribution of absence rate within a treatment center school measured as the total days absent from a SUD treatment center school relative to the days enrolled in the same center school. For almost 70 percent of the treated individuals in my sample, the absence rate within a SUD treatment center school is zero, but the other 30 percent are absent from a SUD center school for at least one school day. Therefore, the drop in absenteeism while individuals are in a SUD treatment center school is not purely mechanical.

Panel (b) in Figure 8 shows that the effect of SUD treatment center school attendance on the course fail rate is visually more pronounced with the event study regression. Following SUD treatment, treated individuals are less likely to fail a course relative to the matched control individuals, and this effect is persistent in the first two post-treatment years. As mentioned above, a large fraction of individuals leave the public school system in the first two years. As a result, only 69.5% of treated individuals have non-missing course completion records in the following academic year of SUD treatment initiation. To address concerns about compositional changes, I show that my results for the course fail rate are robust if I use a balanced sample instead, confirming that the course fail rate estimates are not driven by student compositional changes (see Section 5.3).

Figure 9 presents the coefficients and 95% confidence intervals from equation (1), in which I pool the post-treatment periods to capture the average effects of attending a SUD treatment center on each of my short-run outcomes. Table 2 reports the corresponding regression estimates. As shown in columns (1) and (2), attending a SUD treatment center school leads to an average decrease in the absence rate of 5.1 percentage points (or 27.5% relative to the pre-detention treated group mean, $p-$value$<$0.001) and a 11.9 percentage point decrease in chronic absenteeism (23.5%, $p-$value$<$0.001) in the first 13 grading periods following SUD treatment. Moreover, as reported in column (3), the likelihood of not being observed within the Texas public school system decreases by 4.93 percentage points (10.9%, $p-$value$<$0.001). In column (4), I combine the measure of chronic absenteeism and the measure of not being observed in the public school system and find that SUD treatment center school attendance reduces the likelihood of chronic absenteeism or not being observed in the public school system by 10.4 percentage point (13.7%, $p-$value$<$0.001) in the first 13 periods. Finally, as reported in the last column, the course fail rate decreases by 5.5 percentage points (16.1 percent, $p-$value$<$ 0.001) in the first two academic years following SUD treatment.

Heterogeneity Analyses I investigate heterogeneity in the estimated effects of SUD treatment center schools on the short-run outcomes across a number of individual and treatment characteristics. Specifically, I define the following sub-groups based on individual characteristics: (1) Non-Hispanic White, (2) Non-White (Hispanic or Non-Hispanic Black), (3) female, (4) male, (5) economically disadvantaged (measured using free/reduced-price lunch receipt in the two years prior to detention), (6) not economically disadvantaged, (7) in a special education program (measured in the last two years prior to detention), and (8) not in a special education program. In addition, I investigate heterogeneity by treatment characteristics (including age at the time of SUD treatment) using the following sub-groups: (1) treatment at ages 13–14, (2) treatment at age 15, (3) treatment at age 16, (4) by court order, and (5) not by court order.[41]

To fully understand the heterogeneity in the treatment effect, it is important to explore heterogeneity in the length of stay—a measure of treatment intensity—across the sub-groups. In the box plots in Figures 10–11, the whiskers show the lower and upper extreme values (excluding outliers); and vertical lines show the 25th percentile, median, 75th percentile of the length of stay, expressed as a percentage of one academic year.[42] The box plots in Figure 10 indicate that females have the longest length of stay among the sub-groups, while adolescents who are in a special education program prior to detention spend the shortest period of time in a SUD treatment center school on average. The distribution of the length of stay is similar across the other sub-groups. Figure 11 shows that the length of stay is similar across different ages at the time of SUD treatment initiation. Adolescents who are court-ordered into the programs are enrolled in the program for a substantially longer period of time than those who are not.

In Figures 12–17, I show the results for my heterogeneity analysis for each sub-group and for each of my short-run outcomes. Panel (a) shows the heterogeneity by demographic characteristics, and Panel (b) presents the heterogeneity estimates by treatment characteristics. I present the coefficients and associated 95% confidence intervals from equation (1). In these analyses, I interact the indicators for sub-groups

[41] For each enrollment record, the TEA data provide data on "attribution code", which indicate several circumstances including whether the student attends an open enrollment charter school; the student is in a residential treatment facility and was court-ordered into the facility; and the student is in a residential treatment facility and the student was not court-ordered into the facility. I identify students who are court-ordered into a SUD treatment center school using the codes indicating that a student is court ordered into a residential treatment facility. I consider a student is not court-ordered into the facility if the student has any other attribution codes. Since data on court order status are only available from 2009–2010 onward, my heterogeneity analyses by court order status only include adolescents who enter a SUD treatment center in or after the academic year 2009–2010 and have non-missing attribution code. Roughly 40 percent of the final sample in the heterogeneity analysis by court order status are court-ordered into a SUD center school.

[42] Specifically, I calculate the length of stay by the following steps. First, I assume that every academic year has 180 days. Second, I winsorized the length of stay at 180 days and divided it by 180 days to express the length of stay as a percentage of one academic year.

with the $Treatment_i \times Post_{gt}$ term, and report the estimates on these interaction terms.[43] My heterogeneity analysis for the short-term outcomes suggests that the impacts of SUD treatment center schools are nearly universal but with the following differences. First, for all the outcomes, I find the largest effects on females among my demographic sub-groups. One potential explanation is that females spend the longest time in a SUD treatment center school among the demographic sub-groups. Second, although SUD treatment center school attendance reduces the likelihood of not being observed in the public school in the post-treatment period among almost all sub-groups, I do not see any effect among those who were in a special education program in the pre-detention period.

5.2 Long-Run Effects on Educational and Labor Market Outcomes

Figure 18 shows the estimates of the effects of attending a SUD treatment center school on adolescents' educational and labor market outcomes by age 20. The figure presents the coefficients and 95% confidence intervals from equation (3) for each of my long-run outcomes. Tables 3 and 4 present the corresponding regression results, where I report coefficients, standard errors that are clustered at the individual level in parentheses, and $p-$values in brackets.

The estimates in Tables 3 and 4 indicate that attending a SUD treatment center school leads to a 4.4 percentage point (15.4% relative to the control group outcome mean, $p-$value$<$0.001) increase in the likelihood of completing grade 10 and a 1.7 percentage point (10.2%, $p-$value$=$0.006) increase in the likelihood of grade 11 completion by age 20.[44,45] In my sample, a relatively small number of individuals graduate high school (control group mean$=$13.2%). I find no statistically significant effect of attending a SUD treatment center school on high school graduation ($p-$value$=$0.561). To summarize the effects of treatment center schools on completed secondary education, I investigate the effect on the maximum grade level completed. As reported in column 4, the estimates indicate that attending a SUD treatment center school leads to 0.11 additional years of schooling.

[43] When investigating the heterogeneity by gender, I use indicators for females and males.

[44] I define grade 10 completion as ever being enrolled in Texas public school system in grade 11. I define grade 11 completion similarly.

[45] Although not reported, I do not find evidence that attending a SUD treatment center is systematically associated with completing grade 10 (or 11) prior to detention.

I find that attending a SUD center school leads adolescents to be 1.34 percentage point (11.5 percent, $p-$value=0.018) more likely to enroll in any college by age 20. This increase is almost entirely driven by an increase in two-year college attendance. Note that only a tiny number of individuals in my sample attended a four-year college by age 20 (control group mean=0.6%), implying that SUD treatment center school attendance leads to an increased two-year college enrollment among youth who would not have attended college. I also find that treated individuals experience an increased likelihood of being employed at ages 17–20 by 2 percentage points (2.7 percent). I combine the measures of college enrollment and employment and find that attending a SUD center school leads to a 2.1 percentage point (2.7%, $p-$value=0.003) increase in the likelihood of being enrolled in any college by age 20 or employed between 17–20 or both.

Heterogeneity analyses by demographic characteristics I examine heterogeneity in long-run impacts by student demographic and treatment characteristics. In Figures 19–20, I present the coefficients and 95% confidence intervals from equation (3) for each sub-group.[46] Heterogeneity in long-run impacts by gender, race/ethnicity, receipt of free or reduced-price lunch, and participation in a special education program are presented in Panels (a), (b), (c), and (d) in Figure 19, respectively. Heterogeneity in long-run impacts by age at the time of SUD treatment initiation is presented in Figure 20. Panels (a) and (b) in Figure 21 show heterogeneity by court order status.

The estimates reported in Figure 19 indicate that the impact of SUD treatment center schools on college enrollment and employment is much larger among females (14.5% of the long-run analysis sample) and adolescents who were not eligible for free/reduced price lunch in the pre-detention period (24% of the long-run analysis sample). I also find that the size of the effect on college enrollment is larger for Whites, and the effect on employment is mostly driven by non-Whites (22.8% of the long-run analysis sample). Among adolescents who were in a special education program prior to detention, the effect of SUD treatment schools is indistinguishable from zero for almost all outcomes and is significant and negative for high school graduation. The estimates presented in Appendix Figure 20 indicate that the effect of SUD treatment schools on grade 10 and

[46]In these analyses, I interact indicators for sub-groups with an indicator for the treated individuals ($Treatment$) and report the estimates for the interaction terms.

grade 11 completion is largest among those who were aged 16 (oldest among my sample) at the time of SUD treatment initiation, reflecting the fact that their grades were closer to grade 10 or 11. For the college and employment outcomes, I see a larger effect of SUD treatment center schools on college enrollment among adolescents who were 13–15 years old at the time of SUD treatment, while I see a larger effect on employment among those who were 16 years old at the time of SUD treatment.

Finally, Panel (b) in Appendix Figure 21 reveals important heterogeneity in the impact of SUD treatment center school attendance on my long-run outcomes by court order status. I find positive impacts of SUD treatment center schools on both treated individuals who are court-ordered into the program and those who are not. In particular, I see a larger impact on grade 10 completion, grade 11 completion, and high school graduation among those who enter a center by court order, while I observe a larger effect on college enrollment and employment among those who do not enter a program by court order. Although this analysis is conducted using 37.7% of my long-run analysis sample,[47] the results provide important evidence that SUD treatment center schools improve student outcomes regardless of whether they are court-ordered into the program or not.

5.3 Robustness Analysis

As described in Section 4, I use different empirical models in the short- and long-run analyses. To investigate the difference between the two models, I estimate the long-run analysis model (equation (3)) using my short-run analysis outcomes and compare those estimates with the baseline estimates. For this exercise, I first construct a version of the short-run analysis outcomes by taking the average of the outcome values between relative periods 0 and +12 (i.e., the first two years after residential SUD treatment). Using this sample, I then run both short- and long-run analysis models. Figure 22 presents coefficients and 95% confidence intervals from these estimations separately for each econometric model. Importantly, the results are robust across the two models, suggesting that the difference between short- and long-run analysis models does not

[47] As noted before, the data on whether an individual is court-ordered into a residential facility is only available from the academic year 2009–2010 onward.

drive my results.

Alternative explanation: difference in underlying ability In Section 5.2, I show that SUD treatment center school attendance leads to an increase in college enrollment and employment by age 20. However, there could be a concern that my results are driven by the difference in underlying ability or academic performance in the baseline period. To address this concern, I examine the impact of SUD treatment center school on academic performance measured in the pre-treatment period, which is similar to a falsification test. In Appendix Figure 23, I report the coefficients and 95% confidence interval from equation 3 with the following outcomes as the dependent variable: (1) average past course pass rate, (2) average past Z-scores for standardized reading tests, and (3) average past Z-scores for standardized math tests, all measured in the two academic years prior to SUD treatment initiation. The coefficients are close to zero and statistically insignificant, providing evidence that my results are not driven by the difference in academic performance in the pre-treatment period.

Alternative explanation: the effect of detention or mean reversion In Figure 5, I show that the treated group experienced a sudden and large drop in absenteeism, disciplinary action, and course pass rate beginning in the period of SUD treatment initiation. However, there could be a concern that reduction in absenteeism or improvements in academic performance following SUD treatment initiation may be driven by the differential impacts of detention across the treatment and control groups and/or differential mean reversion effects—a return to the individual's typical performance—rather than positive causal effects of treatment center school attendance. To address this concern, I investigate whether SUD treatment center attendance leads to a reduction in absenteeism among those who enter a SUD treatment center school not within three grading periods but after five or six grading periods (about a year).[48] If a reduction in absence rate is solely driven by the differential detention effects or differential mean reversion effects across the two groups, I would not expect to see any substantial changes in absenteeism at the time of SUD treatment initiation for those who enter a treatment center school after five or six grading periods. To perform this test, I

[48]Note that in my baseline analysis, I restrict attention to adolescents who enter a SUD treatment center school within three grading periods.

assign individuals into seven groups based on the length of the intermediate pre-period (i.e., the period between detention and SUD treatment initiation), ranging from zero to six. In Appendix Figure A9, I plot raw trends in chronic absenteeism separately for each group (for brevity, I omit the group who enter a SUD treatment center school four periods after detention). In the top left panel, I show the trends in chronic absenteeism for those who enter the treatment school in the same period of detention. Then in the remaining panels, I show the trends for sub-groups with 1, 2, 3, 5, and 6 period-long intermediate pre-period, respectively. The solid gray vertical line denotes the time of detention, and the red dashed vertical line denotes the time of SUD treatment initiation.

In all panels, I observe a certain level of drop in absence during the intermediate pre-period, which may reflect factors such as the deterrence effect during detention and the mean reversion pattern. However, in all panels, the largest drop in absenteeism coincides with the exact time of SUD treatment initiation, suggesting that the reduction in absenteeism measured in event time zero is not driven by the alternative explanations mentioned above.

Robustness analysis: unbalanced vs. balanced panel My short-run analysis uses an unbalanced panel of adolescents who are observed in the TEA data in each of the 25 grading periods surrounding SUD treatment initiation (12 periods before to 13 periods after). In Appendix Figure A10, I explore the sensitivity of my estimates using a balanced panel instead. However, as shown in Figure 6, a large fraction of adolescents leave the Texas public school system in the first two years following SUD treatment initiation, implying that only a small fraction of the sample will be consistently observed for all 25 periods. To relax this balanced sample restriction, I use semester-year-level data for absenteeism and disciplinary action outcomes instead of six-week grading period-level data for this analysis.[49] In particular, I overlay my event study estimates that are obtained using an unbalanced sample (i.e., baseline sample) with results obtained from a sample in which I only include individuals that are consistently observed from four semesters before to three semesters after SUD treatment initiation. The results across the two samples are very similar, indicating that my baseline estimates are not

[49]Empirical specification is the same as equation (2). One semester prior to SUD treatment initiation is the reference period.

driven by compositional changes in the sample.[50]

Robustness analysis: limiting the sample to youth with prior substance-related discipline history As described in Section 4.2, to be eligible for the control group, individuals should have been disciplined for substance-related problems in the last 12 grading periods prior to detention, but I do not make the restriction for the treated group. In Appendix Figures A12–A14, I show that both short- and long-run analyses results are very similar if I only include match groups where both the treated and matched control individuals were ever disciplined for substance-related reasons prior to detention, confirming that this sample restriction does not drive my results.

Alternative matching: exact matching only I also test the robustness of my estimates using two alternative ways of matching. First, I exclude the absence rate from the set of matching variables. Second, I only do the exact matching omitting the fuzzy matching. Using these two alternative ways of matching, I show the following sets of results: (1) raw plot and event study results from equation (2) for each of my short-run analysis outcomes, (2) the difference-in-difference estimates for my short-run analysis outcomes from equation (1), and (3) the estimates for my long-run analysis outcomes (from equation (3)). In Appendix Figures A15–A20, I show the sets of results derived from the alternative matching approach in which I drop the absence rate from the fuzzy matching. The sets of results using exact matching only are presented in Appendix Figures A21–A26. For all the regression results, I overlay the baseline estimates and the estimates derived from an alternative matching approach. The results from this analysis indicate that my results are qualitatively similar when I exclude absence from the set of matching variables or when I do an exact match only.

6 Discussion and Comparison with Previous Studies

Comparison to Other Interventions in Disadvantaged Populations To contextualize my estimates of the effects of attending a SUD treatment center school on college enrollment and employment, I discuss how my findings compare to results from

[50] Although not reported, the event study results for course fail rate are also robust to only including individuals who have non-missing course fail rate record from two years before to one year after SUD treatment initiation.

papers that examine the impacts of interventions for at-risk youth or youth from disadvantaged backgrounds. Although I focus on justice-involved youth and other interventions target youth from disadvantaged backgrounds more generally, these comparisons could be helpful given substantial overlap between these two populations. Using administrative data from Illinois, Chyn (2018) finds that moving children who lived in severely distressed public housing to lower-poverty neighborhoods between ages 7 and 18 leads to a 9 percent (or 4 percentage point) increase in employment at ages 19–26. I find a 2.7 percent (or 2 percentage point) increase in employment at ages 17–20, which is nearly a third the size of the estimated impact of moving to less-disadvantaged neighborhoods. My estimate of the impact of SUD treatment schools on employment is relatively large given that youths spend on average 49 days in a SUD treatment center school and thus the duration of the treatment is much shorter than that of moving to lower-poverty neighborhoods. This relatively large magnitude could reflect the fact that untreated SUDs can have adverse impacts on all aspects of a young person's life, and thus even access to SUD treatment for a relatively short-term period can have large positive impacts that can persist into adulthood.

My estimates can also be compared to Gelber et al. (2016)'s study on the impact of the New York City (NYC) Summer Youth Employment Program (SYEP) on college enrollment and employment.[51] They find that providing youths aged 14–21 with paid summer employment for up to seven weeks increases employment by 1 percentage point in 1–4 years after the program but has no impact on college enrollment. My estimates of a 2 percentage point increase in employment at ages 17–20 and 1.3 percentage point increase in college enrollment are larger in magnitude. While the intervention in their setting is of a similar duration as the average treatment duration in my setting, the intervention I analyze is more targeted both in the treatments provided (health and education services) and the population served (those who are suffering from severe SUDs). These differences in targeting may contribute to the difference in estimated effects.

Benchmarking Benefits Against Costs My estimates suggest that attending a

[51] Youths who participated in this program on average came from disadvantaged family backgrounds and were disproportionately minorities.

SUD treatment center school has positive impacts on the maximum grade completed in secondary school, college enrollment, and employment at ages 17–20. To understand the cost effectiveness of providing access to residential SUD treatment, I compare the benefits of attending a SUD treatment center school with the associated costs. First, I take the estimated cost per adolescent residential treatment episode of $13,643.1 (in 2020 dollars) from French et al. (2008). Second, I conduct a back-of-the-envelope calculation based on my estimates of the impact of attending a treatment center school on years of schooling in secondary school. Assuming that an additional year of schooling leads to 10% increase in earnings (Card, 1999) and that this effect on earnings is constant through age 64, I find that my estimated 0.11 increase in years of education in secondary school leads to $5,008.12 (in 2020 dollars) increase in the present discounted value of lifetime earnings for each individual, indicating that the benefits from increased schooling in secondary school alone can cover 36.7% of the total costs of providing access to residential SUD treatment.

7 Conclusion

Substance use and SUDs are significant public health challenges in the United States. SUDs can have particularly profound effects on adolescents, given that adolescence is a critical period for developing healthy behaviors and accumulating human capital. Despite the urgent need for greater implementation of effective SUD treatments, there is a lack of causal evidence of the effect of SUD treatment programs on individuals, especially on adolescents. Quantifying the causal effects of SUD treatment programs on affected individuals and understanding the heterogeneity in the effect of such programs across individuals are critical for policy design, as the policymakers must decide how to allocate limited resources across different types of programs and across individuals.

Using individual-level administrative panel data from Texas, this paper provides evidence on the causal impact of attending a SUD treatment center school on later educational and employment outcomes through age 20 among at-risk youth—youths who were previously detained in a juvenile detention center at some point between ages 12 and 16. By taking advantage of the high-frequency nature of the data, I investigate within-individual changes in outcomes. I show that attending a SUD treatment center school reduces absenteeism, course fail rate, and disciplinary action, and increases the likelihood of being in the public school system in the following two years, relative to matched control individuals. By comparing outcomes across the treatment and matched control individuals while controlling for county fixed effects and pre-detention characteristics, this paper also establishes for the first time that these schools have long-lasting positive impacts on completed secondary education, college enrollment, and employment at ages 17–20.

This paper demonstrates that providing access to a SUD treatment center school—an increasingly popular type of SUD treatment programs for adolescents—has substantial benefits for justice-involved youth, who represent about half of all SUD treatment admissions among youth aged 12–17 years. My back-of-the-envelope calculations suggest that projected increases in lifetime earnings based on the increases in educational attainment in secondary school alone can offset roughly a third of the costs of this treatment. These estimated benefits may understate the total benefits, if attending a treatment center school also leads to unmeasured improvements in health (e.g., reductions in mortality or health care spending) and reductions in crime (e.g, reductions in costs incurred by the justice system or victims). Interventions during adolescence have particularly important implications for substance use policies because many individuals begin their use of addictive substances during this period. Policymakers might consider improving access to SUD treatment among adolescents as one important way to address SUD problems as well as increase human capital accumulation among at-risk youth.

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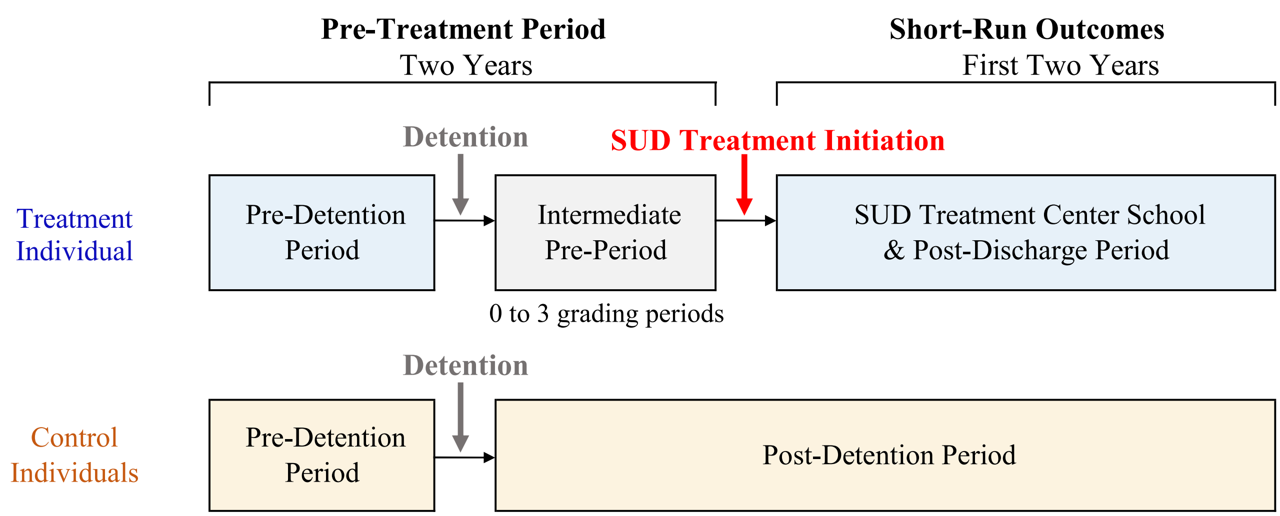
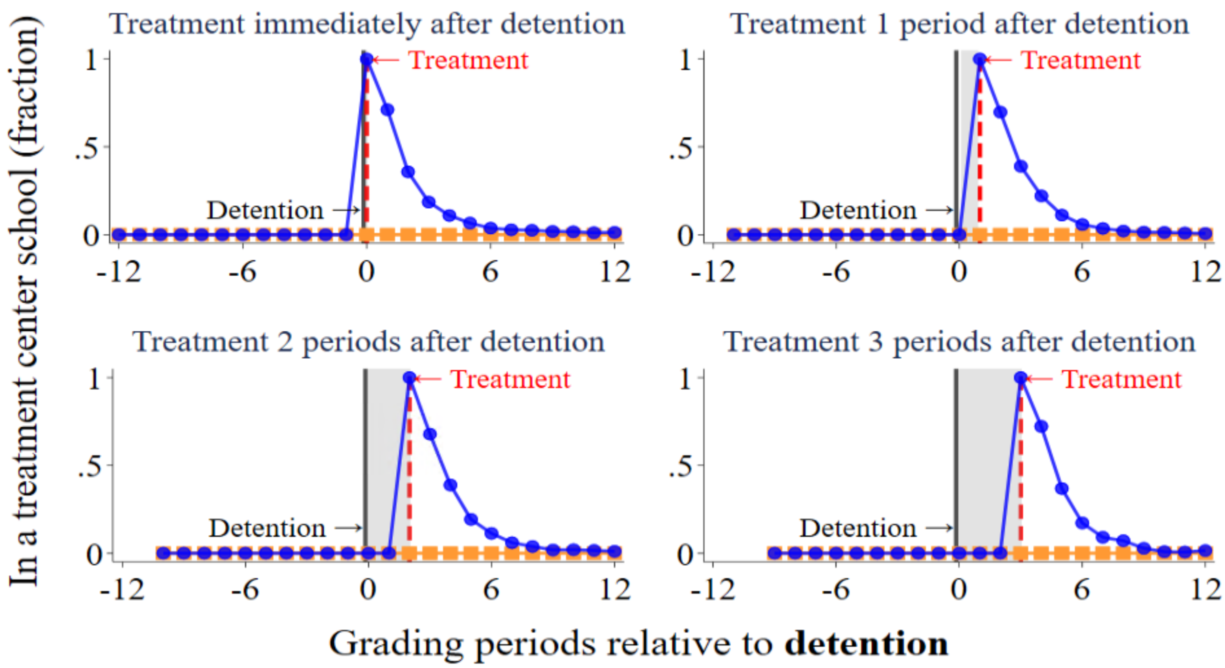
8 Figures and Tables

Figure 1: Description of the Short-Run Analysis Design



Notes: For each treated individual, I identify control individuals based on observable characteristics measured in the pre-detention period. For each match group, the study post-period is define as the periods during or after which the treatment individual enters a residential SUD treatment center school.

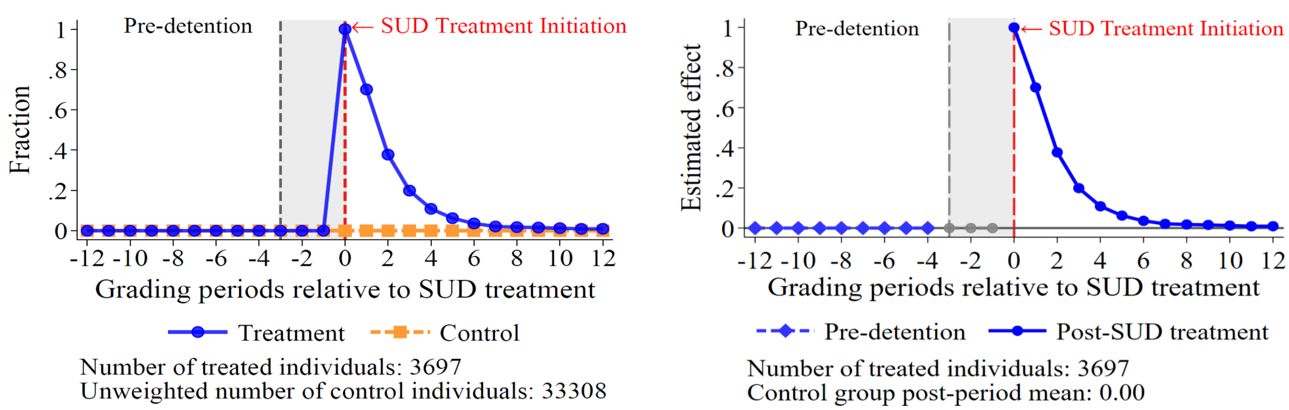
Figure 2: Treatment Center School Enrollment: Raw Trends and Event Studies



Notes: These figures plot raw data trends in enrollment in a SUD treatment center school separately for four sub-groups that are defined based on the length of the intermediate pre-period.

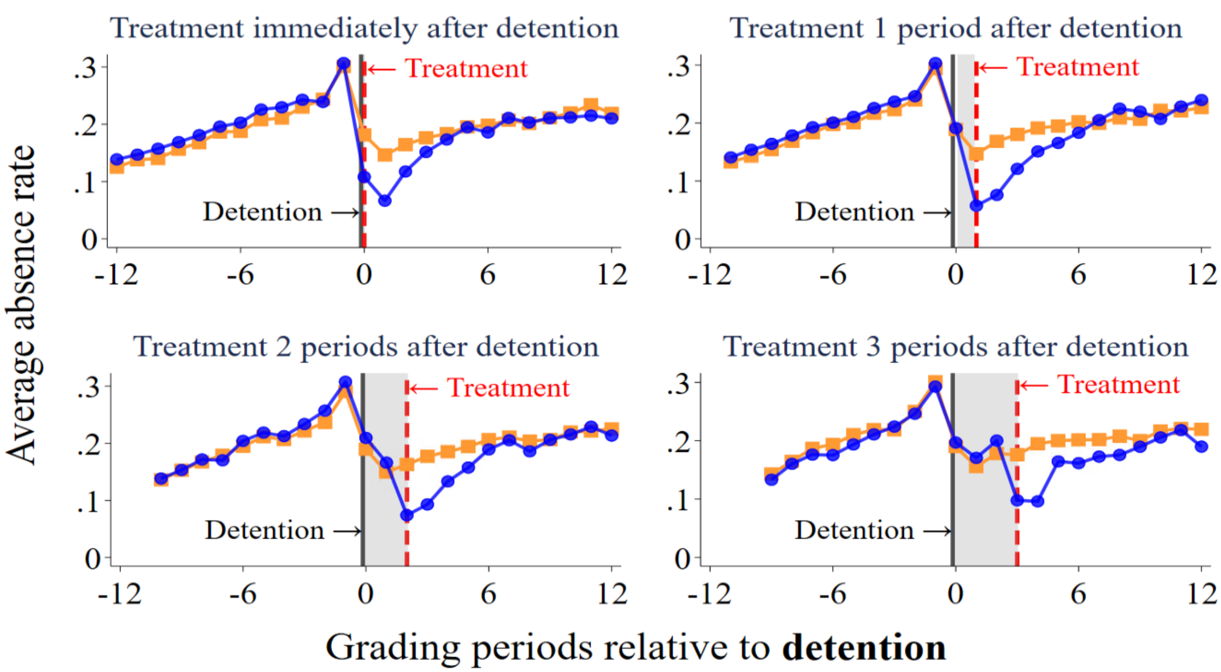
Figure 3: Treatment Center School Enrollment: Raw Trends and Event Studies

(a) Raw Trends (b) Event Study



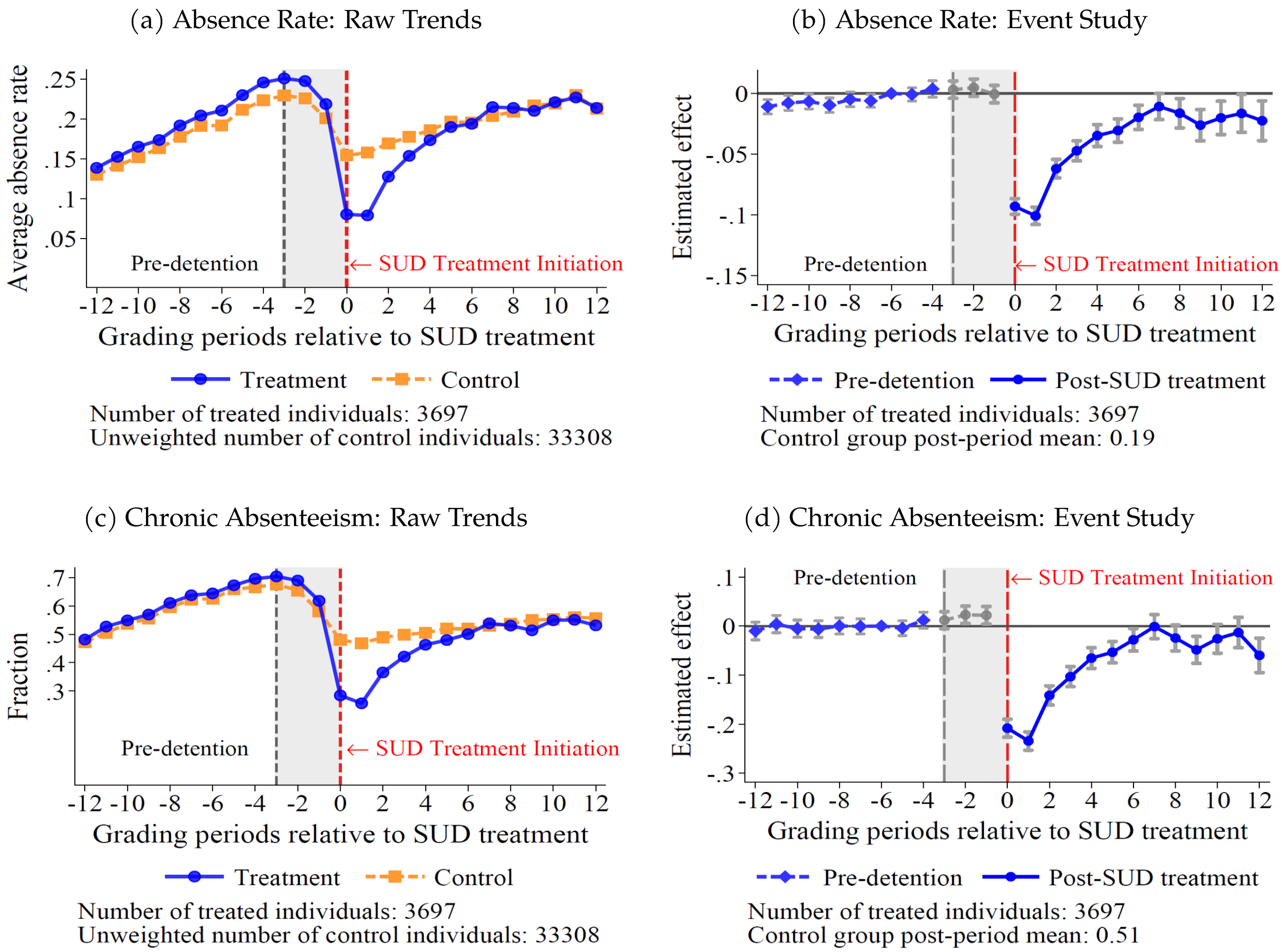
Notes: The figure plots raw data trends and event study results. In Panel (a), I present raw trends in the likelihood of being enrolled in a substance use disorder (SUD) treatment center school from 12 six-week-level grading periods before (i.e., about two academic years) to 13 grading periods after the time of SUD treatment initiation, separately for treated and matched control individuals. Panel (b) plots the regression analogue to raw trends presented in Panel (a). I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2).

Figure 4: Absence Rate: Raw Trends by the Length of the Intermediate Pre-Period Between Detention and SUD Treatment Initiation



Notes: These figures plot raw data trends in enrollment in a SUD treatment center school separately for four sub-groups that are defined based on the length of the intermediate pre-period.

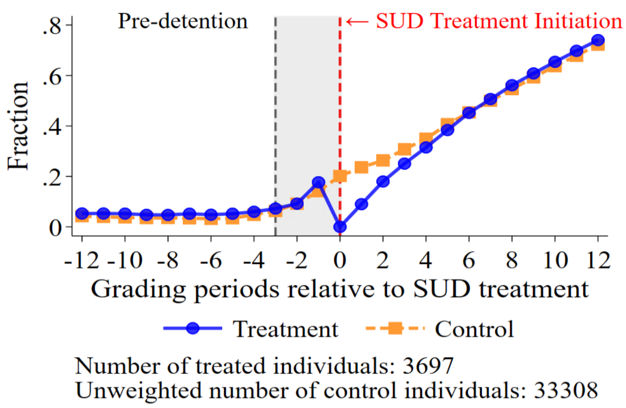
Figure 5: Absenteeism: Raw Trends and Event Studies



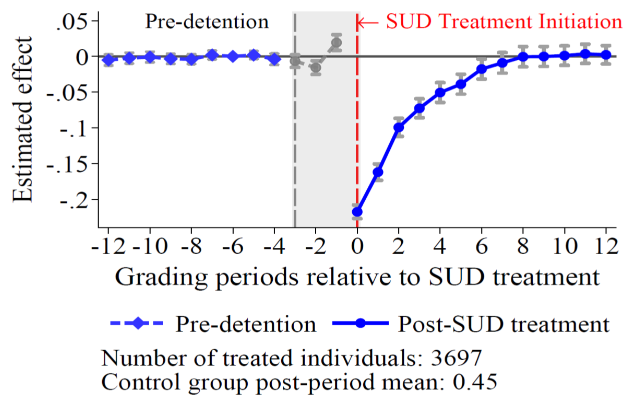
Notes: The figure plots raw data trends and event study results. In Panels (a) and (c), I present raw trends in my short-run outcomes from 12 six-week-level grading periods before (i.e., about two academic years) to 13 grading periods after the time of SUD treatment initiation, separately for treated and matched control individuals. Panels (b) and (d) plot the regression analogue to raw trends presented in Panels (a) and (c), respectively. I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2). Standard errors are clustered at the individual level.

Figure 6: Not Being in Public School: Raw Trends and Event Studies

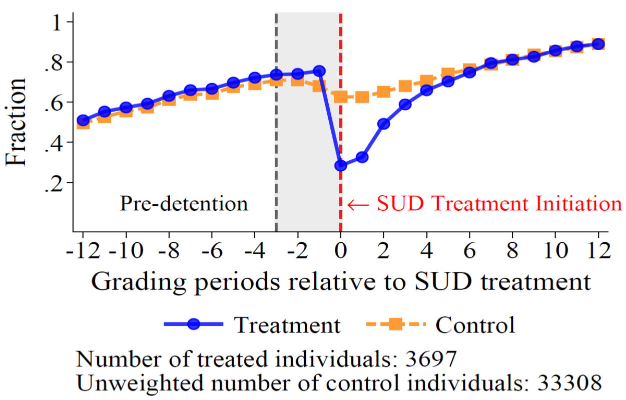
(a) Not in Public School System: Raw Trends



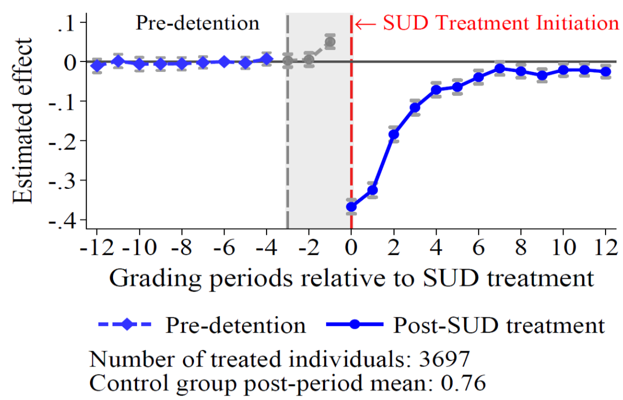
(b) Not in Public School System: Event Study



(c) Not in Public School Or
Chronic Absenteeism: Raw Trends

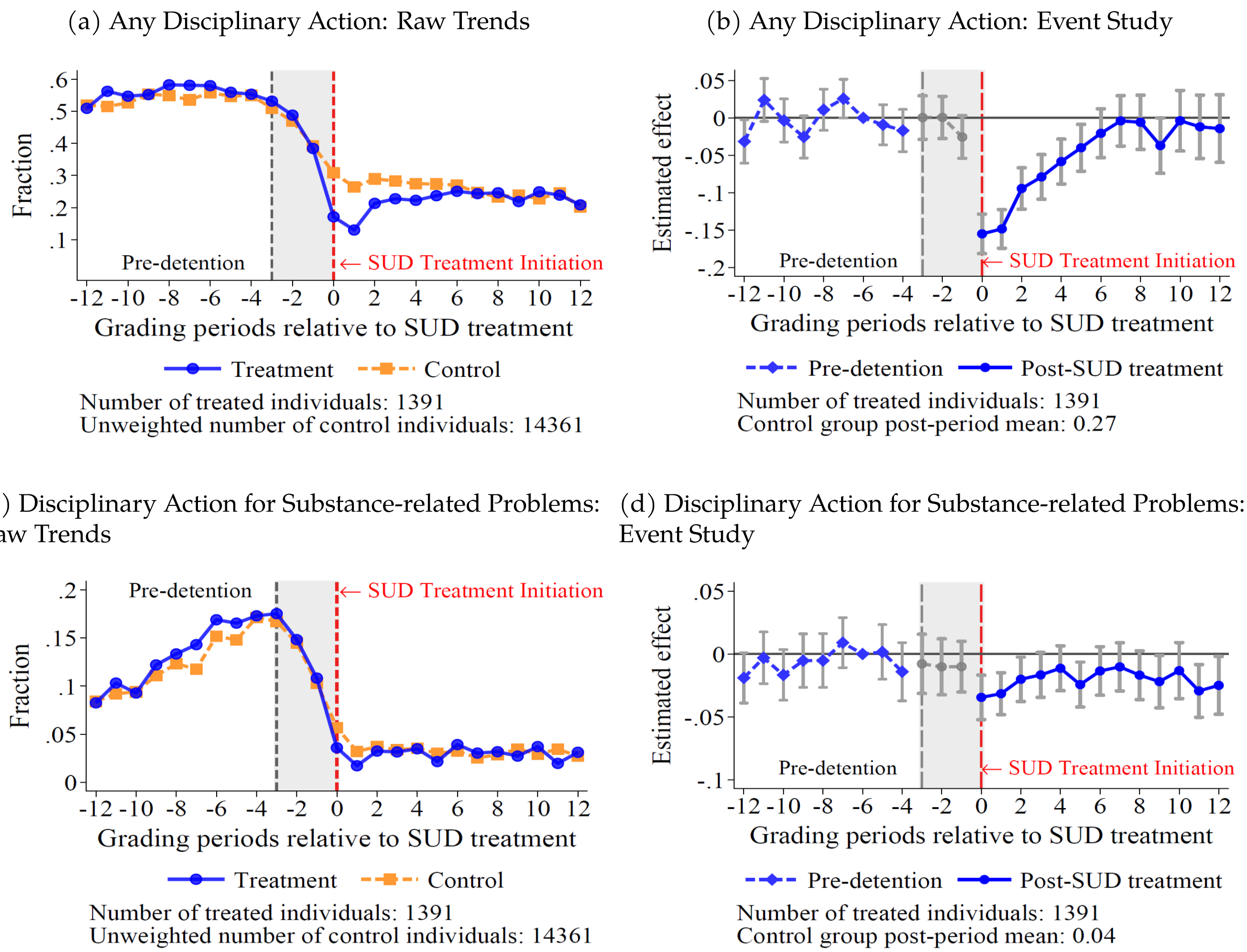


(d) Not in Public School Or
Chronic Absenteeism: Event Study



Notes: The figure plots raw data trends and event study results. In Panels (a) and (c), I present raw trends in my short-run outcomes from 12 six-week-level grading periods before (i.e., about two academic years) to 13 grading periods after the time of SUD treatment initiation, separately for treated and matched control individuals. Panels (b) and (d) plot the regression analogue to raw trends presented in Panels (a) and (c), respectively. I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2). Standard errors are clustered at the individual level.

Figure 7: Disciplinary Action in School: Raw Trends and Event Studies

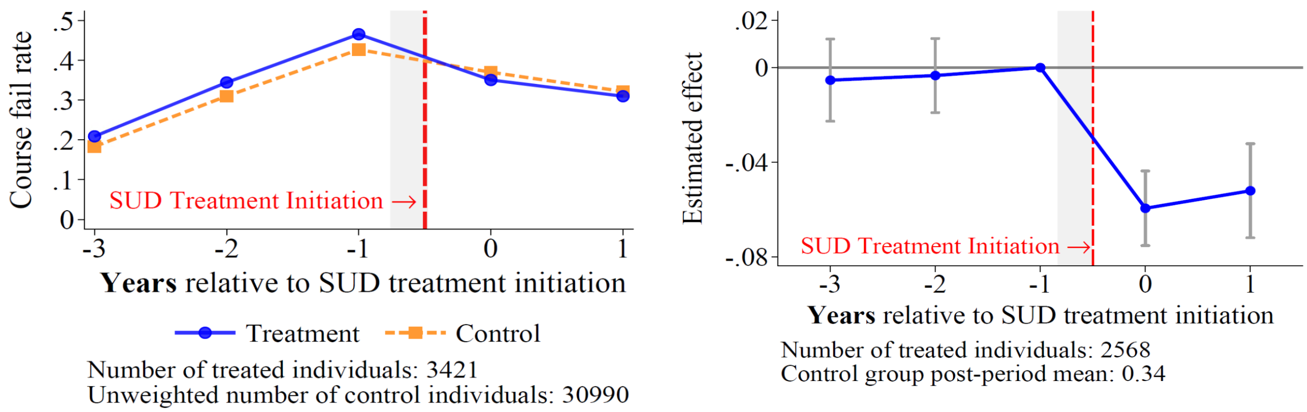


Notes: The figure plots raw data trends and event study results. In Panels (a) and (c), I present raw trends in my short-run outcomes from 12 six-week-level grading periods before (i.e., about two academic years) to 13 grading periods after the time of SUD treatment initiation, separately for treated and matched control individuals. Panels (b) and (d) plot the regression analogue to raw trends presented in Panels (a) and (c), respectively. I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2). Standard errors are clustered at the individual level.

Figure 8: Course Fail Rate: Raw Trends and Event Studies

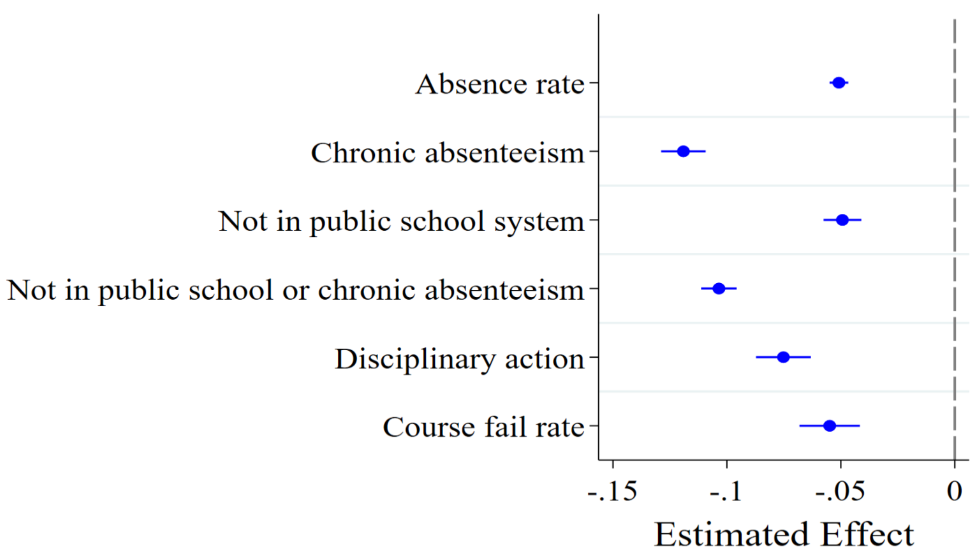
(a) Course Fail Rate: Raw Trends

(b) Course Fail Rate: Event Study



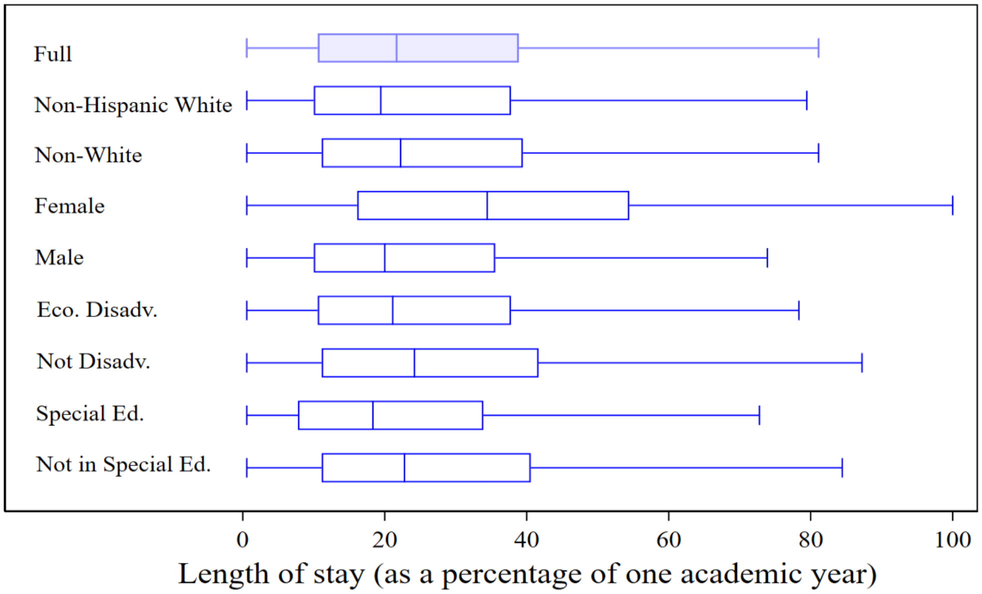
Notes: The figure plots raw data trends and event study results. In Panel (a), I present raw trends in course fail rate from three academic years before and two academic years after SUD treatment, separately for treated and matched control individuals. Panel (b) plots the regression analogue to raw trends presented in Panel (a). I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2). Standard errors are clustered at the individual level.

Figure 9: Impacts of SUD Treatment Center School Attendance on Short-Run Educational Outcomes



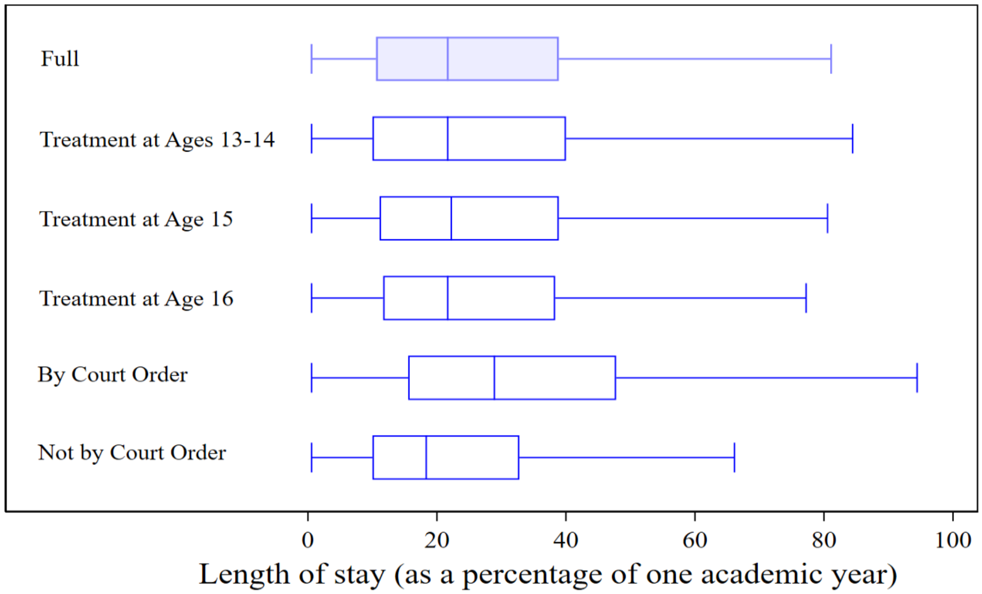
Notes: The figure plots the coefficients and 95% confidence intervals from equation (1), in which I pool the post-treatment periods to capture the average effects of attending a SUD treatment center on each of my short-run outcomes. Standard errors are clustered at the individual level.

Figure 10: Distribution of the Length of Stay by Individual Characteristics



Notes: These box plots show the distribution of the length of stay in a treatment center school within the first year of SUD treatment initiation separately for each sub-group. The whiskers show the lower and upper extreme values (excluding outliers); and vertical lines show the 25th percentile, median, 75th percentile of the length of stay. The length of stay is winsorized at 180 school days (i.e., about an academic year) and then expressed as a percentage of one academic year.

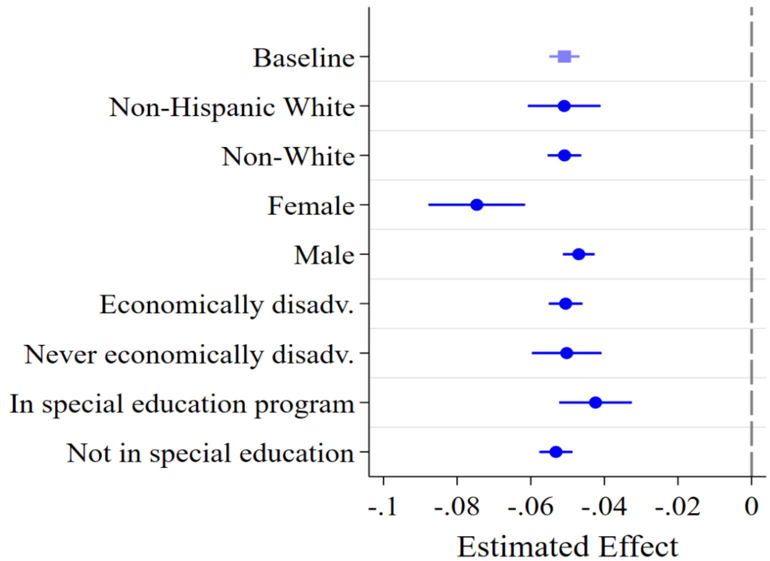
Figure 11: Distribution of the Length of Stay by Treatment Characteristics



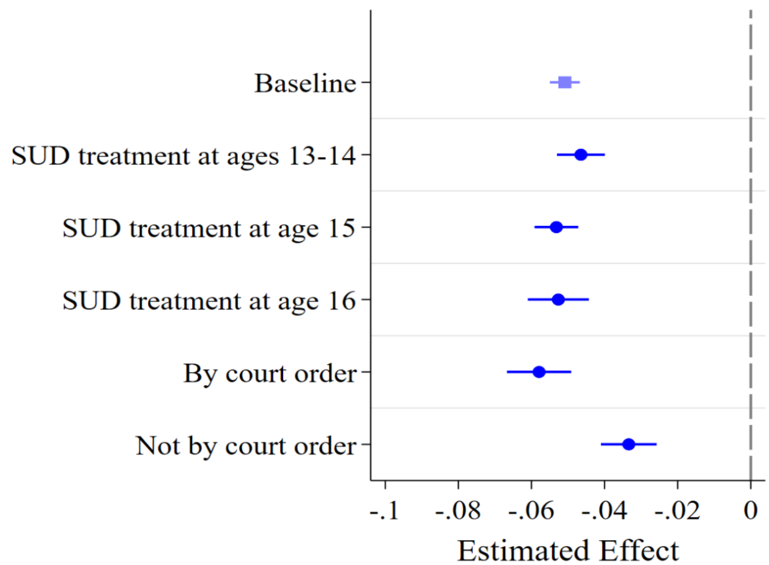
Notes: These box plots show the distribution of the length of stay in a treatment center school within the first year of SUD treatment initiation separately for each sub-group. The whiskers show the lower and upper extreme values (excluding outliers); and vertical lines show the 25th percentile, median, 75th percentile of the length of stay. The length of stay is winsorized at 180 school days (i.e., about an academic year) and then expressed as a percentage of one academic year.

Figure 12: Heterogeneity in the Short-Run Effects: **Absence Rate**

(a) By Demographic Characteristics



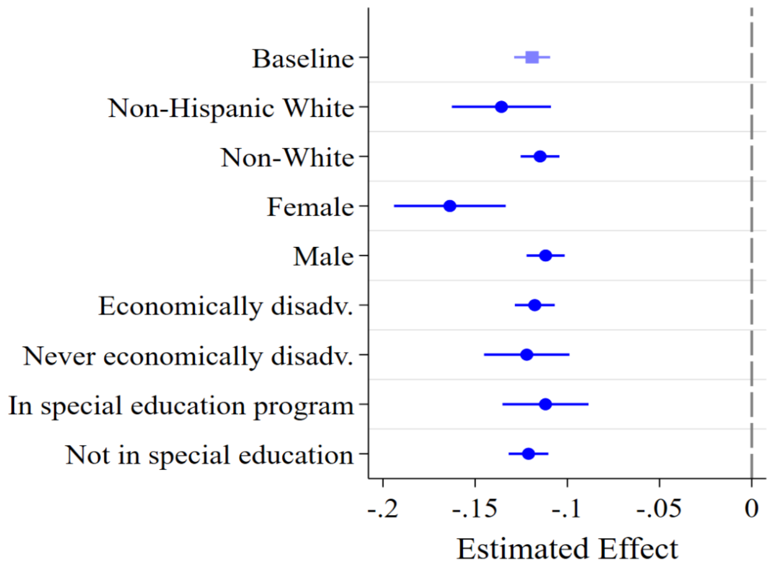
(b) By Treatment Characteristics



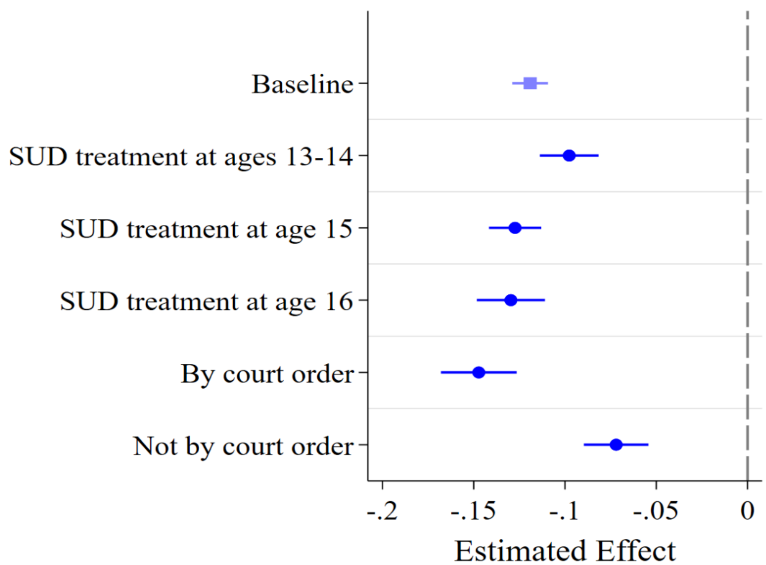
Notes: These figures plot present the effect of treatment school attendance on short-run outcomes for individuals belonging to the sub-group presented on the y-axis. Panel (a) includes the following sub-groups based on individual characteristics: (1) Non-Hispanic White, (2) Non-White (Hispanic or Non-Hispanic Black), (3) female, (4) male, (5) economically disadvantaged (measured using free/reduced-price lunch receipt in the two years prior to detention), (6) not economically disadvantaged, (7) in a special education program (measured in the last two years prior to detention), and (8) not in a special education program. In Panel (b), I investigate heterogeneity by treatment characteristics using the following sub-groups: (1) treatment at ages 13–14, (2) treatment at age 15, (3) treatment at age 16, (4) by court order, and (5) not by court order.

Figure 13: Heterogeneity in the Short-Run Effects: **Chronic Absenteeism**

(a) By Demographic Characteristics



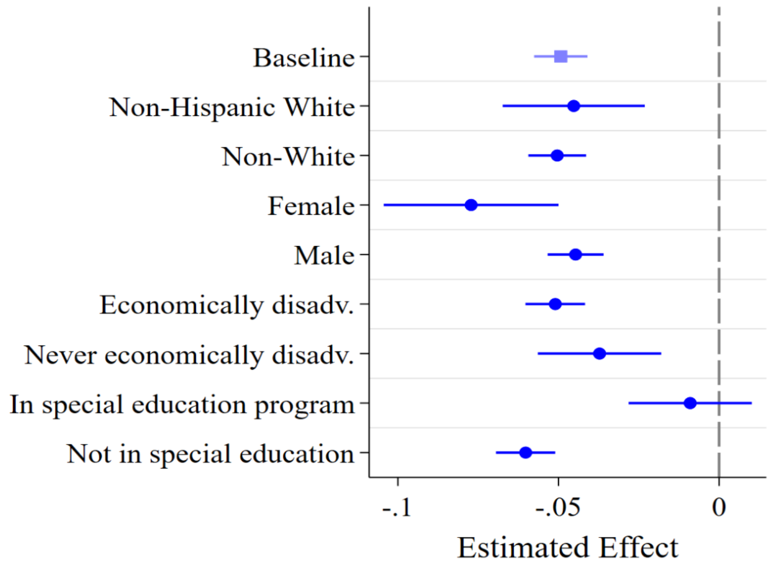
(b) By Treatment Characteristics



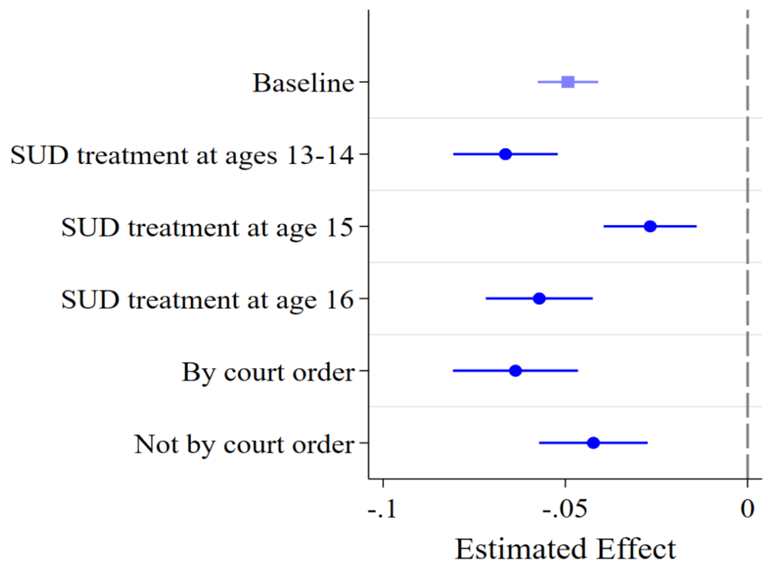
Notes: These figures plot present the effect of treatment school attendance on short-run outcomes for individuals belonging to the sub-group presented on the y-axis. Panel (a) includes the following sub-groups based on individual characteristics: (1) Non-Hispanic White, (2) Non-White (Hispanic or Non-Hispanic Black), (3) female, (4) male, (5) economically disadvantaged (measured using free/reduced-price lunch receipt in the two years prior to detention), (6) not economically disadvantaged, (7) in a special education program (measured in the last two years prior to detention), and (8) not in a special education program. In Panel (b), I investigate heterogeneity by treatment characteristics using the following sub-groups: (1) treatment at ages 13–14, (2) treatment at age 15, (3) treatment at age 16, (4) by court order, and (5) not by court order.

Figure 14: Heterogeneity in the Short-Run Effects: **Not in Public School**

(a) By Demographic Characteristics



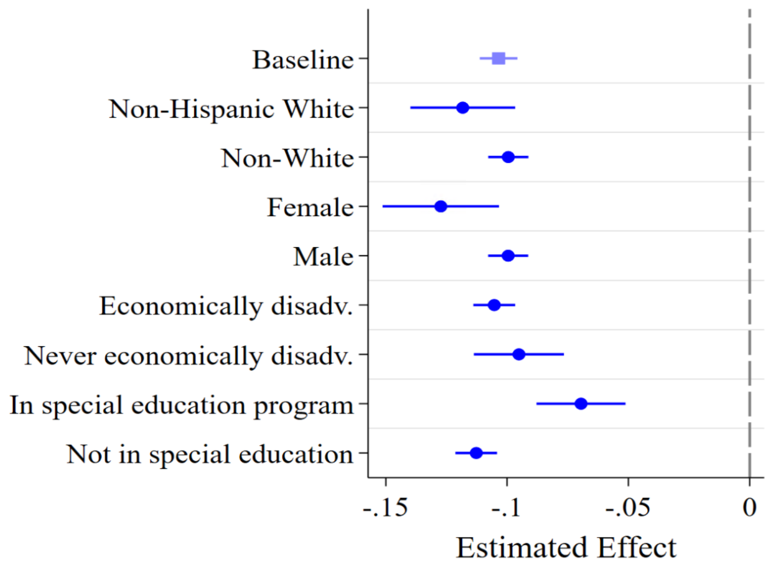
(b) By Treatment Characteristics



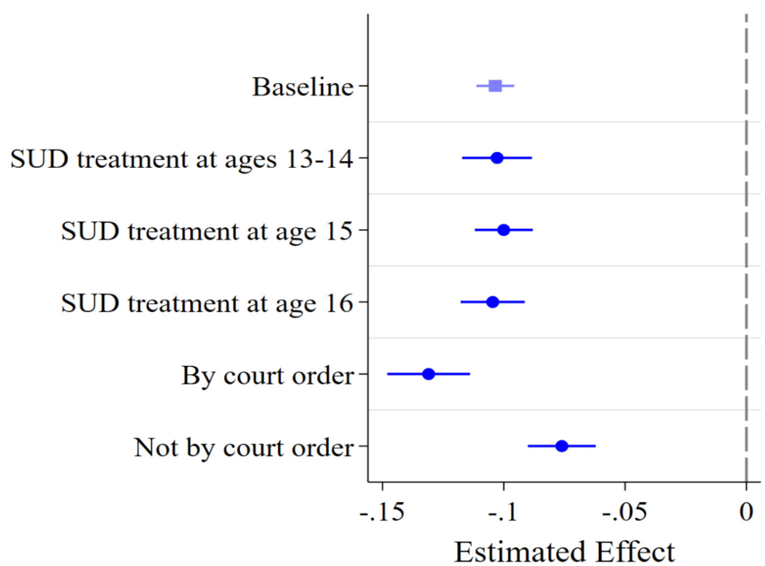
Notes: These figures plot present the effect of treatment school attendance on short-run outcomes for individuals belonging to the sub-group presented on the y-axis. Panel (a) includes the following sub-groups based on individual characteristics: (1) Non-Hispanic White, (2) Non-White (Hispanic or Non-Hispanic Black), (3) female, (4) male, (5) economically disadvantaged (measured using free/reduced-price lunch receipt in the two years prior to detention), (6) not economically disadvantaged, (7) in a special education program (measured in the last two years prior to detention), and (8) not in a special education program. In Panel (b), I investigate heterogeneity by treatment characteristics using the following sub-groups: (1) treatment at ages 13–14, (2) treatment at age 15, (3) treatment at age 16, (4) by court order, and (5) not by court order.

Figure 15: Heterogeneity in the Short-Run Effects: **Not in Public School or Chronic Absenteeism**

(a) By Demographic Characteristics



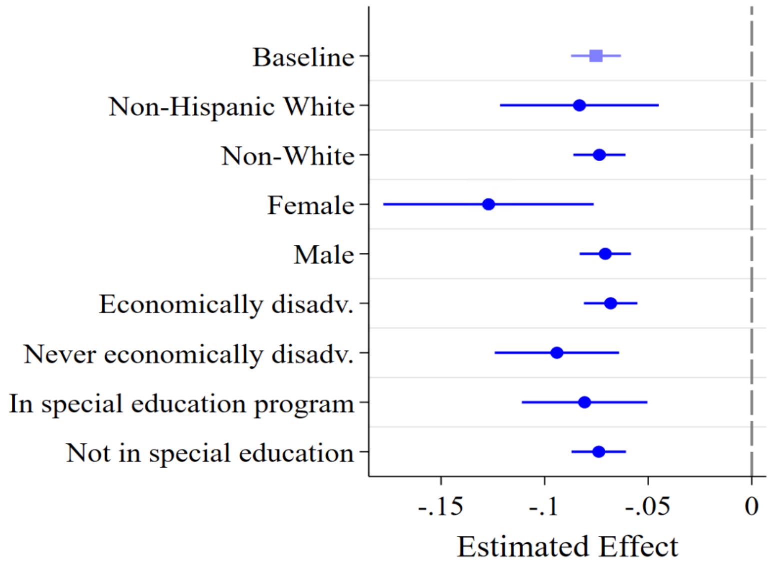
(b) By Treatment Characteristics



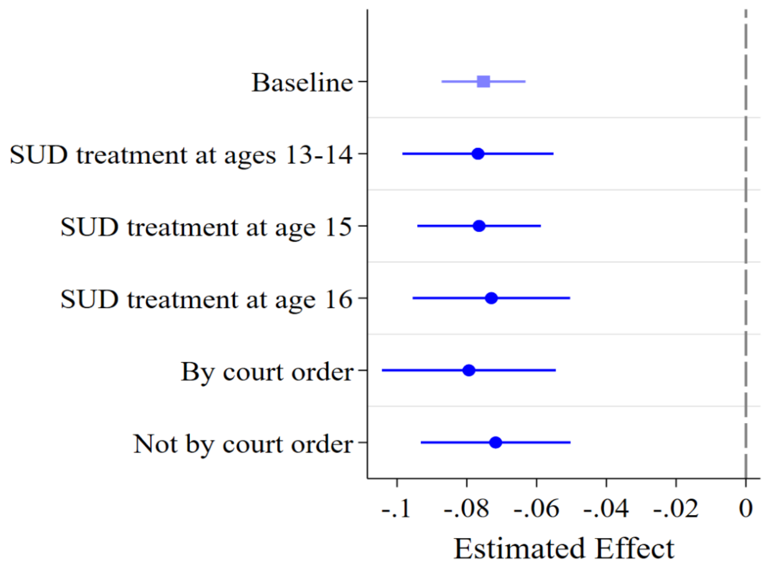
Notes: These figures plot present the effect of treatment school attendance on short-run outcomes for individuals belonging to the sub-group presented on the y-axis. Panel (a) includes the following sub-groups based on individual characteristics: (1) Non-Hispanic White, (2) Non-White (Hispanic or Non-Hispanic Black), (3) female, (4) male, (5) economically disadvantaged (measured using free/reduced-price lunch receipt in the two years prior to detention), (6) not economically disadvantaged, (7) in a special education program (measured in the last two years prior to detention), and (8) not in a special education program. In Panel (b), I investigate heterogeneity by treatment characteristics using the following sub-groups: (1) treatment at ages 13–14, (2) treatment at age 15, (3) treatment at age 16, (4) by court order, and (5) not by court order.

Figure 16: Heterogeneity in the Short-Run Effects: **Disciplinary Action**

(a) By Demographic Characteristics



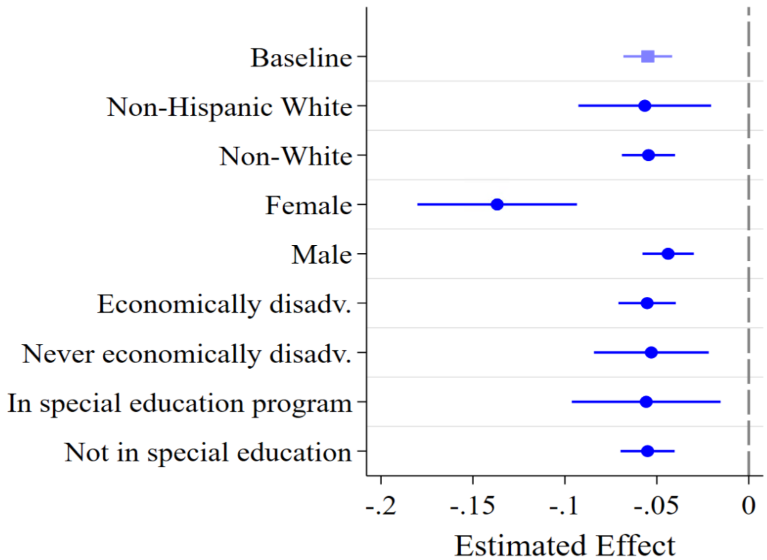
(b) By Treatment Characteristics



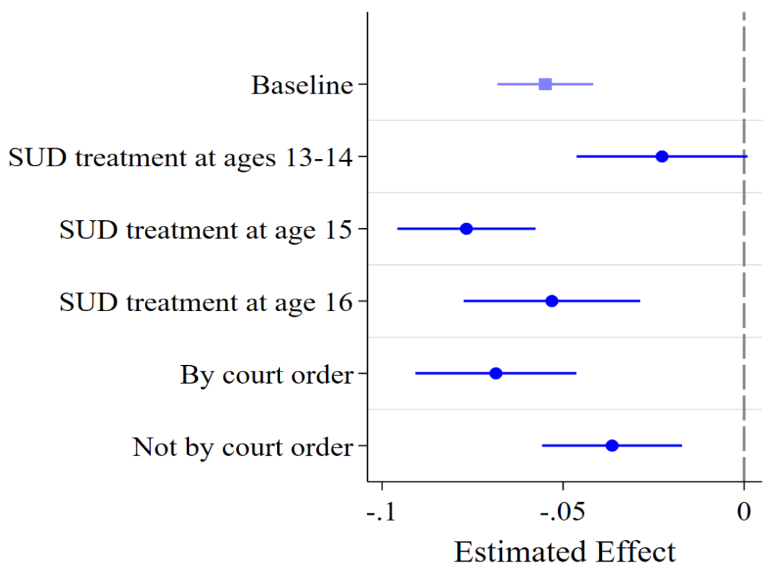
Notes: These figures plot present the effect of treatment school attendance on short-run outcomes for individuals belonging to the sub-group presented on the y-axis. Panel (a) includes the following sub-groups based on individual characteristics: (1) Non-Hispanic White, (2) Non-White (Hispanic or Non-Hispanic Black), (3) female, (4) male, (5) economically disadvantaged (measured using free/reduced-price lunch receipt in the two years prior to detention), (6) not economically disadvantaged, (7) in a special education program (measured in the last two years prior to detention), and (8) not in a special education program. In Panel (b), I investigate heterogeneity by treatment characteristics using the following sub-groups: (1) treatment at ages 13–14, (2) treatment at age 15, (3) treatment at age 16, (4) by court order, and (5) not by court order.

Figure 17: Heterogeneity in the Short-Run Effects: **Course Fail Rate**

(a) By Demographic Characteristics

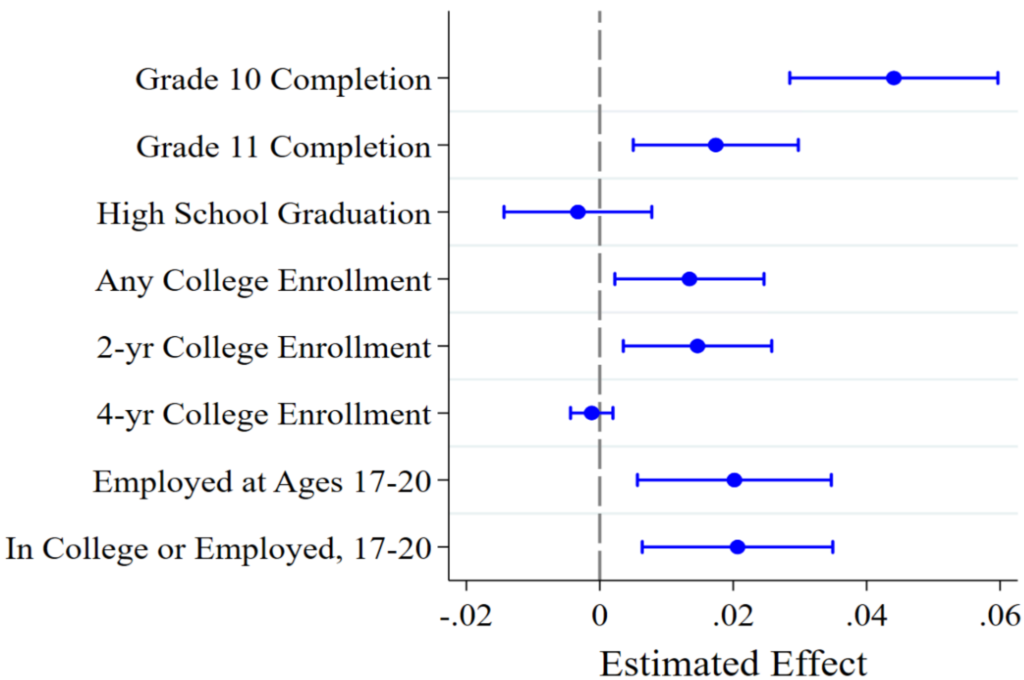


(b) By Treatment Characteristics



Notes: These figures plot present the effect of treatment school attendance on short-run outcomes for individuals belonging to the sub-group presented on the y-axis. Panel (a) includes the following sub-groups based on individual characteristics: (1) Non-Hispanic White, (2) Non-White (Hispanic or Non-Hispanic Black), (3) female, (4) male, (5) economically disadvantaged (measured using free/reduced-price lunch receipt in the two years prior to detention), (6) not economically disadvantaged, (7) in a special education program (measured in the last two years prior to detention), and (8) not in a special education program. In Panel (b), I investigate heterogeneity by treatment characteristics using the following sub-groups: (1) treatment at ages 13–14, (2) treatment at age 15, (3) treatment at age 16, (4) by court order, and (5) not by court order.

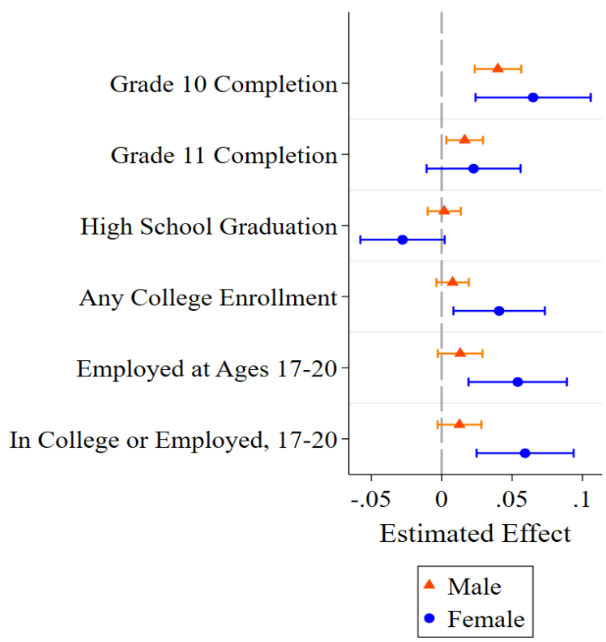
Figure 18: Long-Run Impacts of SUD Treatment Center School Attendance on Educational Outcomes and Employment



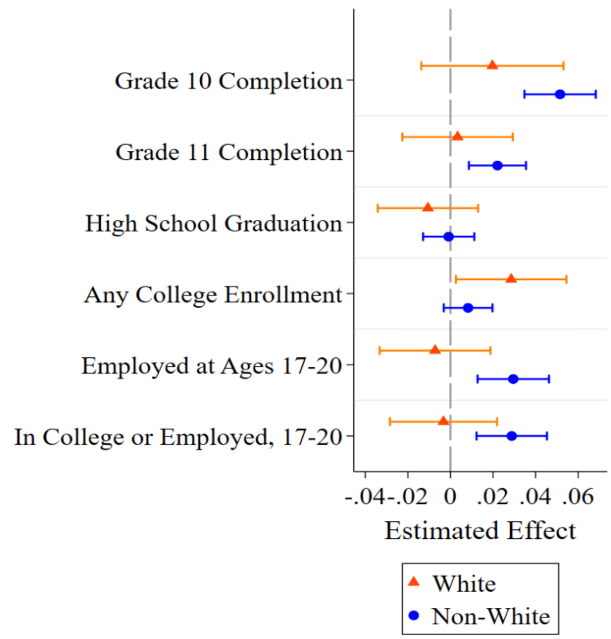
Notes: The figure shows the long-run impacts of SUD treatment center school attendance on educational outcomes and employment at age 17–20. Specifically, the figure plot the coefficients and 95% confidence intervals on the indicator for treatment individuals from estimation of equation 3.

Figure 19: Heterogeneity in the Long-Run Effects by Individual Characteristics

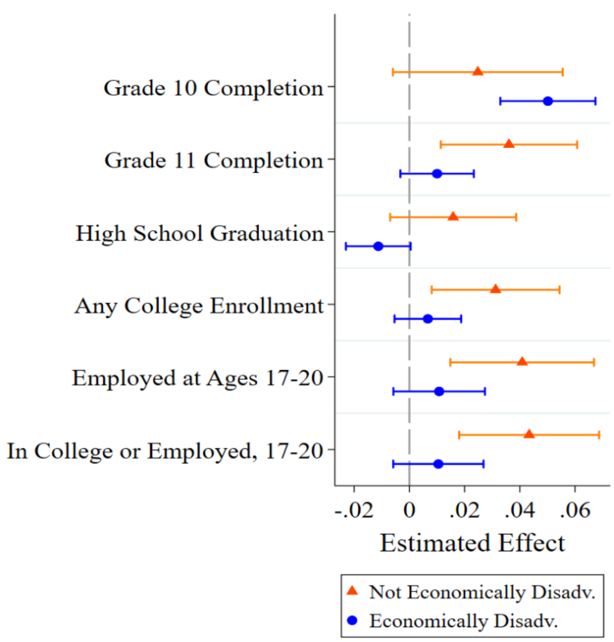
(a) Heterogeneity by **Gender**



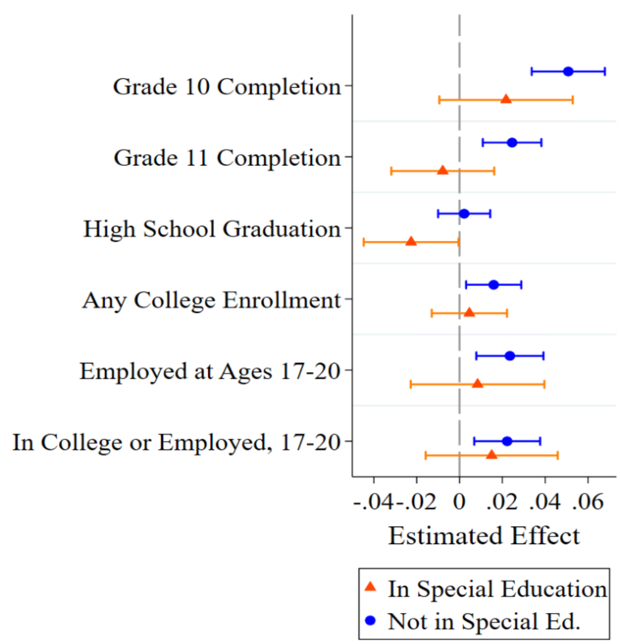
(b) Heterogeneity by **Race/Ethnicity**



(c) Heterogeneity by **Socioeconomic Status**

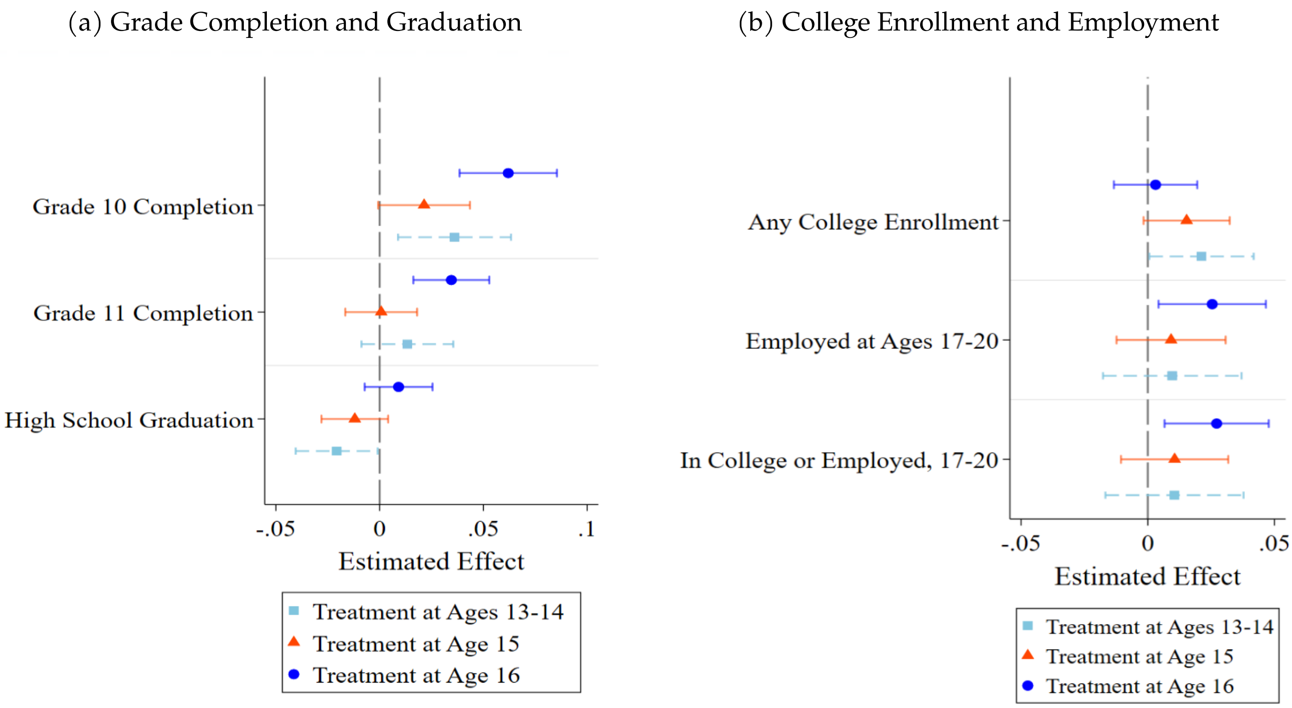


(d) Heterogeneity by **Special Education**



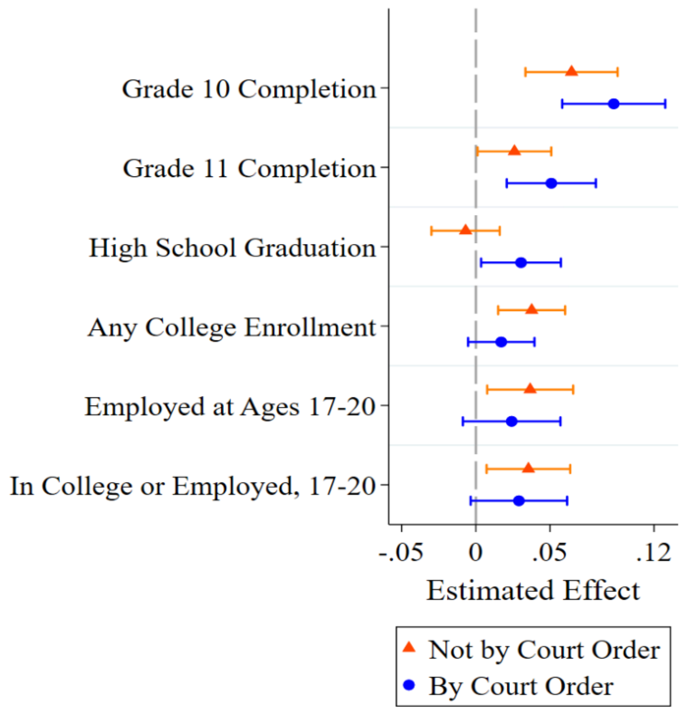
Notes: These figures present the long-run impacts of SUD treatment center school attendance on educational and employment outcomes for individuals belonging to each sub-group.

Figure 20: Heterogeneity in the Long-Run Effects by **Age at the Time of SUD Treatment**



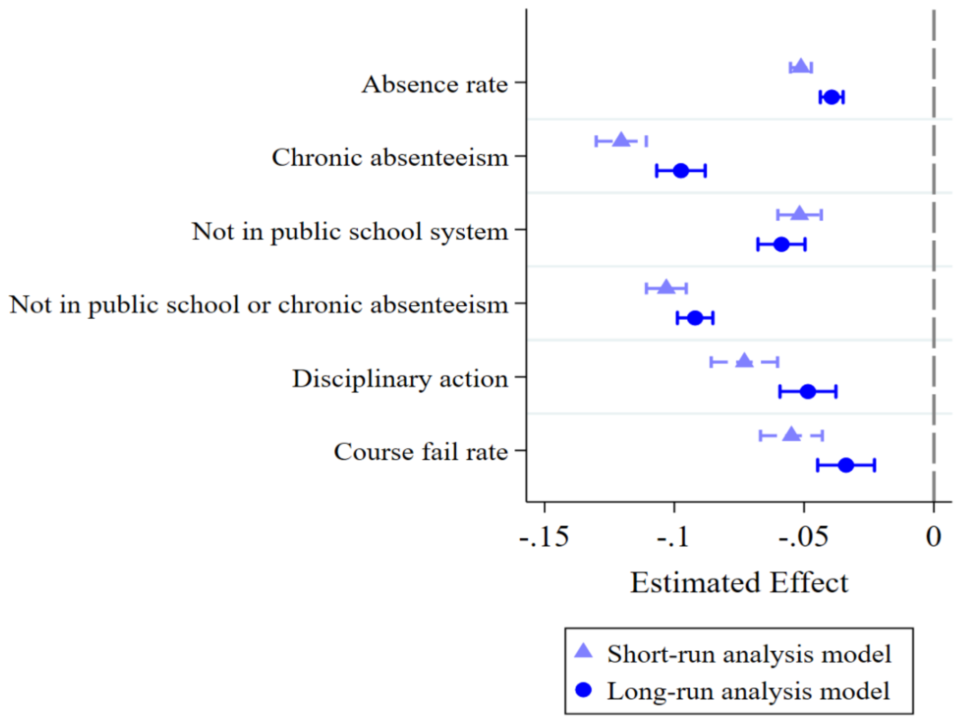
Notes: These figures present the long-run impacts of SUD treatment center school attendance on educational and employment outcomes for individuals belonging to each sub-group.

Figure 21: Heterogeneity in the Long-Run Effects by **Court Order Status**



Notes: These figures present the long-run impacts of SUD treatment center school attendance on educational and employment outcomes for individuals belonging to each sub-group.

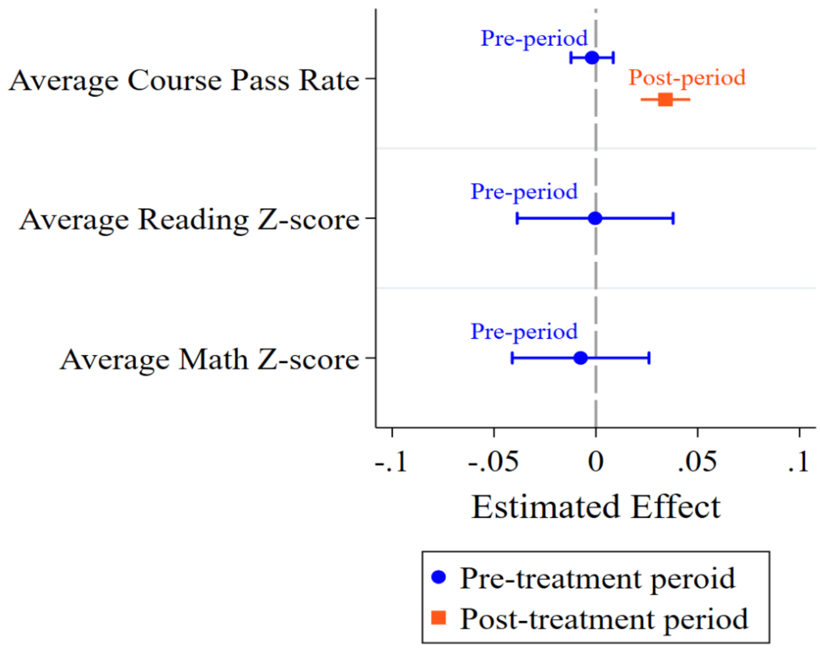
Figure 22: Long-Run Analysis Model with Short-Run Analysis Outcomes



Notes: The figure plots output from estimation of the long-run analysis model (equation 3) with my short-run analysis outcomes as the dependent variable. My baseline estimates are presented in light blue.

Figure 23: Alternative Explanation: Difference in Underlying Ability

(a) Academic Performance in the Pre-Treatment Period



Notes: The figure plots output from estimation of equation (3) with academic performance measured in the pre-treatment period as the dependent variable.

Table 1: Average Individual Characteristics Across Treatment and Matched Control Individuals

	Treatment	Matched Controls	Diff	*p-val*
	(1)	(2)	(1) - (2)	
A. Individual Characteristics (Exact Matching Variables)				
Female	0.146	0.146	0.000	[1.000]
Non-Hispanic white	0.230	0.230	0.000	[1.000]
Hispanic	0.610	0.610	0.000	[1.000]
Non-Hispanic Black	0.157	0.157	0.000	[1.000]
Age at detention	14.841	14.859	-0.018	[<0.001]
Economically disadvantaged	0.752	0.752	0.000	[1.000]
Special education	0.207	0.207	0.000	[1.000]
Urbanicity of county				
Large central metro	0.619	0.619	0.000	[1.000]
Large fringe metro	0.163	0.163	0.000	[1.000]
Medium metro	0.183	0.183	0.000	[1.000]
Small metro	0.029	0.029	0.000	[1.000]
Micropolitan	0.004	0.004	0.000	[1.000]
Noncore	0.003	0.003	0.000	[1.000]
B. Mean Absence Rate and Detention History at Baseline (Fuzzy Matching Variables)				
Mean absence rate, 1 yr before	0.235	0.217	0.018	[<0.001]
Share of periods detained, 1 yr before	0.098	0.083	0.015	[<0.001]
Share of periods detained, 2 yr before	0.033	0.031	0.002	[0.001]
C. Academic Performance at Baseline (Non-Matching Variables)				
Grade at detention	9.023	9.055	-0.033	[<0.001]
Mean past course pass rate, above median	0.609	0.615	-0.006	[0.103]
Mean past reading z-score, above median	0.553	0.556	-0.003	[0.399]
Mean past math z-score, above median	0.597	0.597	0.000	[0.972]
Number of individuals (weighted)	4,034	4,034		
Number of total individuals (unweighted)	4,034	35,714		
Number of unique individuals	4,034	13,212		

Notes: This table shows average characteristics for treatment and matched control individuals.

Table 2: Short-Run Effects of SUD Treatment Center School Attendance on Educational Outcomes

	Absence Rate	Chronic Absence	Not in Public School	(2) or (3)	Disc. Action	Course Fail Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Individual x Post	-0.0509	-0.1191	-0.0493	-0.1035	-0.0752	-0.0549
	(0.0021)	(0.0050)	(0.0042)	(0.0040)	(0.0061)	(0.0068)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Control group (post-treatment period) mean	0.1852	0.5076	0.4535	0.7566	0.2681	0.3420
Effect size relative to the control group mean	-27.48%	-23.46%	-10.87%	-13.68%	-28.05%	-16.05%
Treated individuals	3,697	3,697	3,697	3,697	1,391	2,568
Control individuals (weighted)	3,697.0	3,697.0	3,697.0	3,697.0	1,391.0	2,499.3
Control individuals (total)	33,308	33,308	33,308	33,308	14,361	23,987
Control individuals (unique)	12,145	12,145	12,145	12,145	7,441	9,070
Individual-six-week period observations	717,734	717,734	925,125	925,125	306,433	-
Individual-year observations	-	-	-	-	-	88,529
R-squared	0.549	0.504	0.663	0.498	0.496	0.700

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (1). Standard errors are clustered at the individual level.

Table 3: Long-Run Effects of SUD Treatment Center School Attendance on Educational Outcomes by Age 20

	Grade 10 Completion	Grade 11 Completion	High School Graduation	Maximum Grade Level Completed
	(1)	(2)	(3)	(4)
Treated Individual	0.0441	0.0174	-0.0033	0.1141
	(0.008)	(0.0063)	(0.0057)	(0.0163)
	[<0.001]	[0.006]	[0.561]	[<0.001]
Control group outcome mean	0.2857	0.1707	0.1320	9.1309
Effect size relative to the control group mean	15.44%	10.19%	-2.50%	1.25%
Treated individuals	2,967	3,240	3,252	3,252
Control individuals (weighted)	2,963.2	3,239.6	3,252.0	3,252.0
Control individuals (total)	27,461	28,682	28,723	28,723
Control individuals (unique)	10,152	10,818	10,841	10,841
Observations	30,428	31,922	31,975	31,975
R-squared	0.469	0.464	0.433	0.576

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (3). Standard errors are clustered at the individual level.

Table 4: Long-Run Effects of SUD Treatment Center School Attendance on College Enrollment and Employment by Age 20

	Enroll Any College (1)	Enroll 2-yr Col. (2)	Enroll 4-yr Col. (3)	Employed, Ages 17–20 (4)	In College or Employed (5)
Treatment Individual	0.0134	0.0146	-0.0012	0.0202	0.0206
	(0.0057)	(0.0057)	(0.0016)	(0.0074)	(0.0073)
	[0.018]	[0.010]	[0.456]	[0.007]	[0.005]
Control group outcome mean	0.1150	0.1089	0.0061	0.7571	0.7662
Effect size relative to the control group mean	11.65%	13.41%	-19.67%	2.67%	2.69%
Treatment individuals	3,160	3,160	3,160	3,160	3,160
Control individuals (weighted)	3,186.4	3,186.4	3,186.4	3,186.4	3,186.4
Control individuals (total)	28,161	28,161	28,161	28,161	28,161
Control individuals (unique)	10,610	10,610	10,610	10,610	10,610
Observations	31,321	31,321	31,321	31,321	31,321
R-squared	0.387	0.377	0.357	0.382	0.383

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (3). Standard errors are clustered at the individual level.

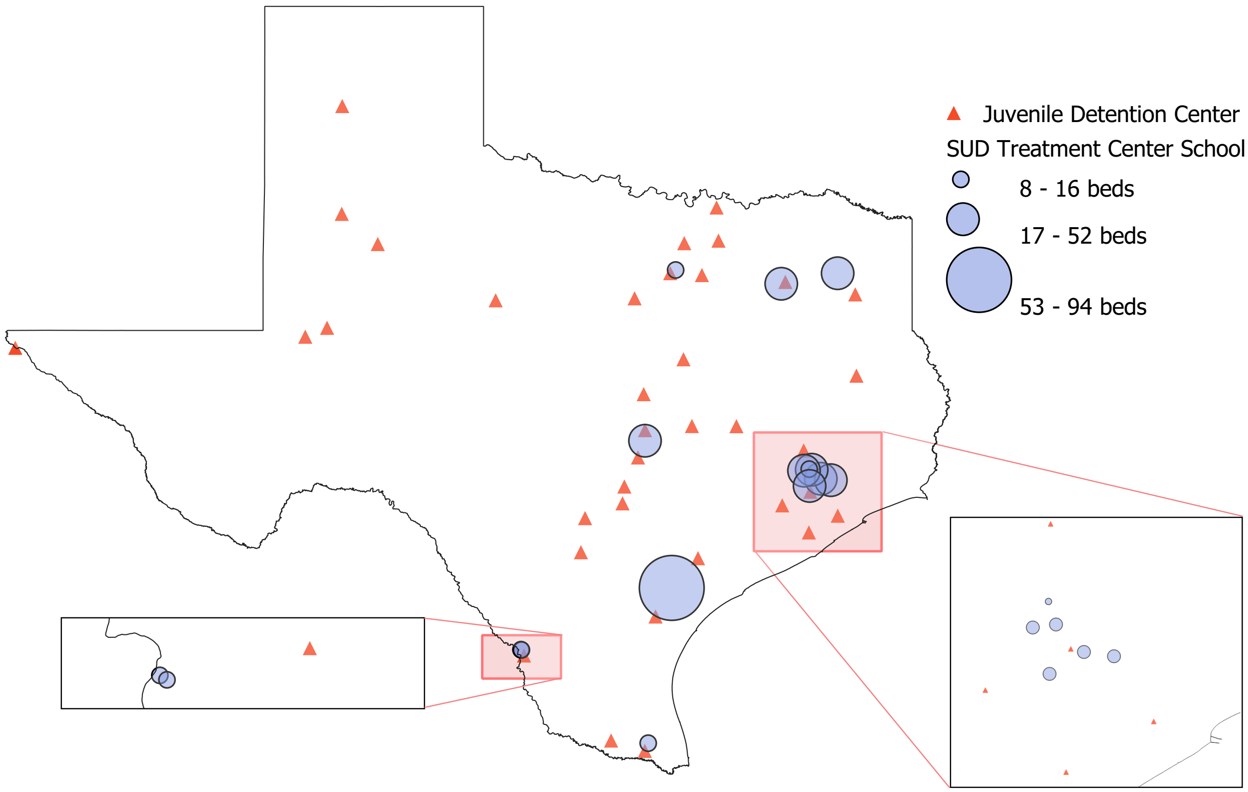
For Online Publication

"Substance Use Disorder Treatment and Human Capital:
Evidence from At-Risk Youth"

Kim (2022)

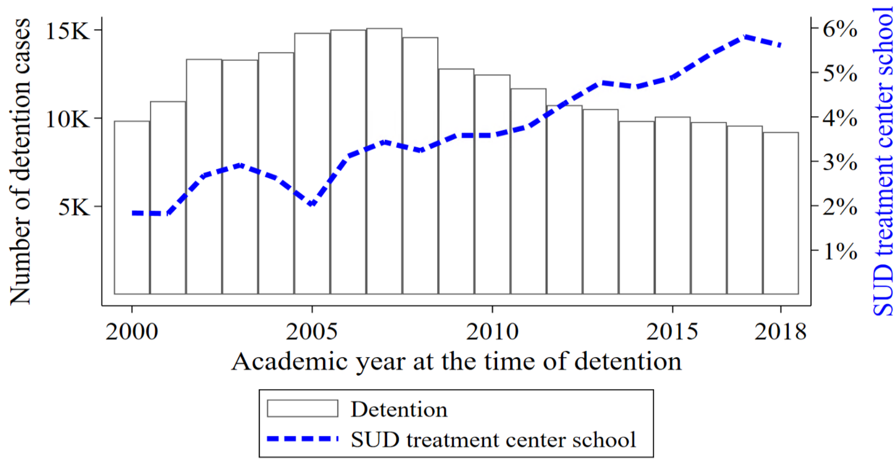
A Appendix Figures

Figure A1: Location of Treatment Center Schools and the Number of Beds



Notes: The figure presents the location of 14 treatment center schools and juvenile detention centers that are included in my analysis. Note that the map only presents juvenile detention centers that are active between 2019 and 2020.

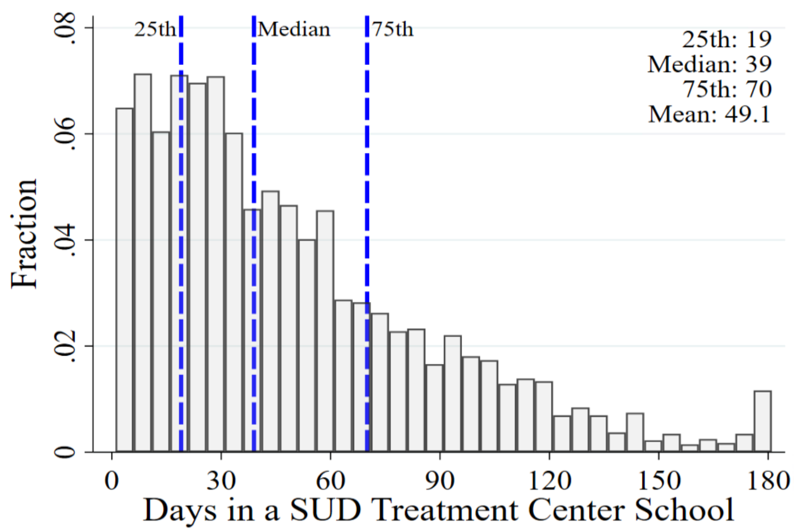
Figure A2: Number of Juvenile Detention Cases and Share of Cases Entering a SUD Treatment Center School



Unique individuals entering a SUD treatment center school: 6,302

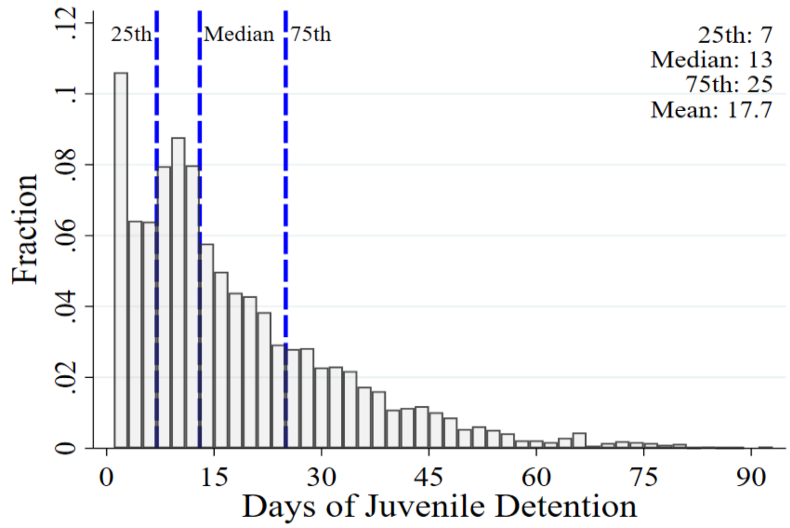
Notes: The figure plot trends in the number juvenile detention cases and the percentage of detainees who enter a SUD treatment center school within a year.

Figure A3: Distribution of Days in a SUD Treatment Center School



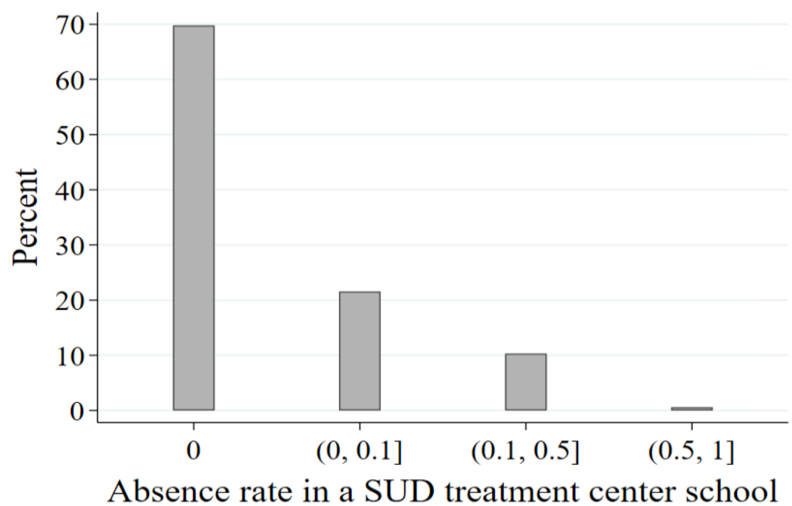
Notes: The figure presents the distribution of the length of stay within a SUD treatment center school among my analysis sample.

Figure A4: Distribution of Days in a Juvenile Detention Center



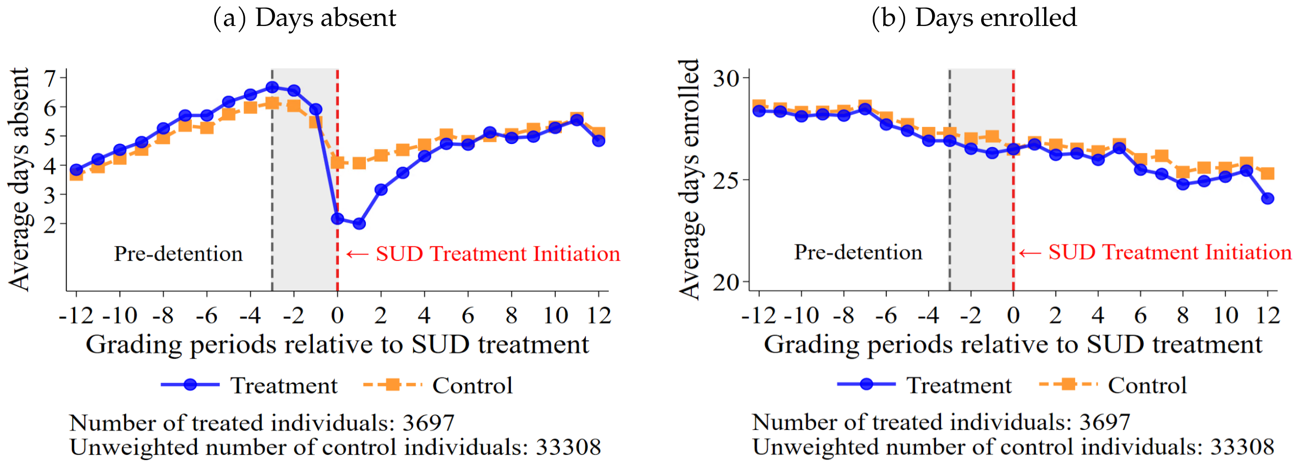
Notes: The figure presents the distribution of the length of juvenile detention among my analysis sample.

Figure A5: Distribution of Absence Rate in a SUD Treatment Center School



Notes: The figure present the distribution of absence rate within a treatment center school measured as the total days absent from a SUD treatment center school relative to the days enrolled in the same center school. For almost 70 percent of the treated individuals in my sample, the absence rate within a SUD treatment center school is zero, but the other 30 percent are absent from a SUD center school for at least one school day.

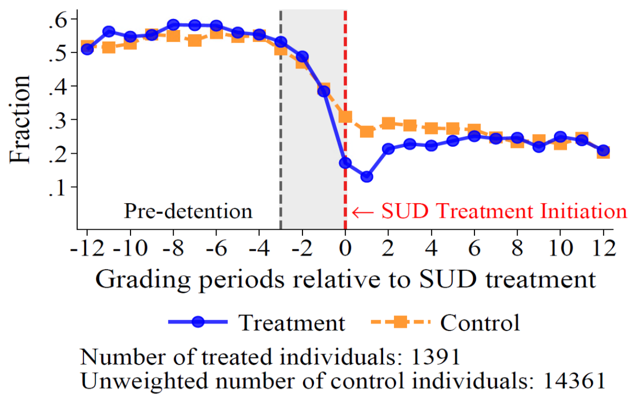
Figure A6: Raw Trends in Days Absent and Days Enrolled in Public Schools or Juvenile Detention Centers



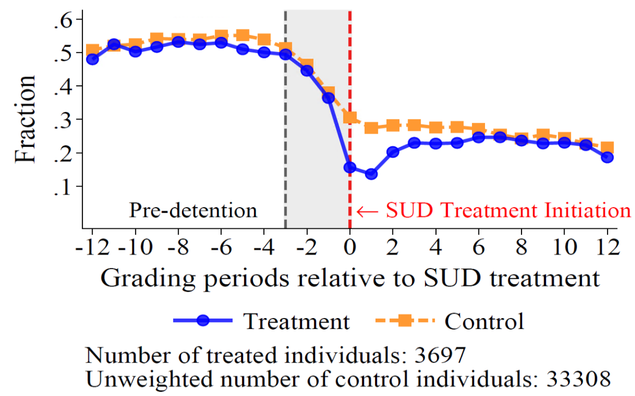
Notes: The figure plots raw data trends and event study results. I present raw data trends in the outcomes from 12 six-week-level grading periods before (i.e., about two academic years) to 13 grading periods after the time of SUD treatment initiation, separately for treated and matched control individuals.

Figure A7: Any Disciplinary Action: Sample with Substance-related Discipline History vs. Full Sample

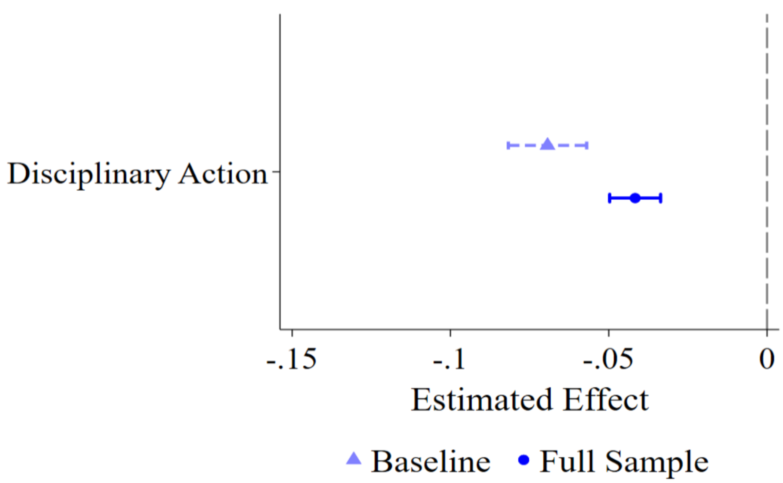
(a) Raw Plot: Individuals with Substance-related Discipline History



(b) Raw Plot: Full sample

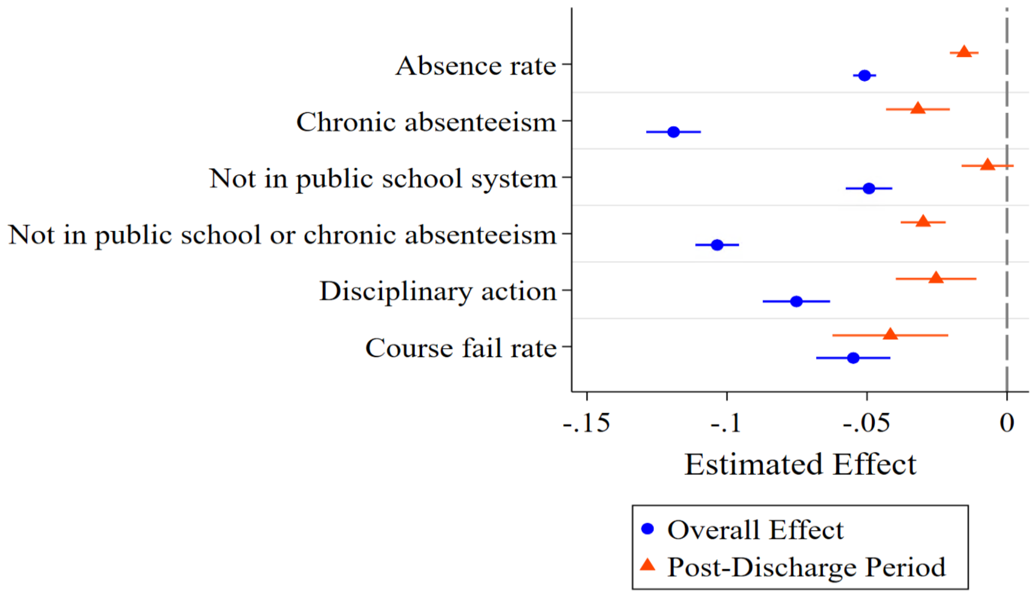


(c) Effect on Disciplinary Action: Individuals with Substance-related Discipline History vs. Full Sample



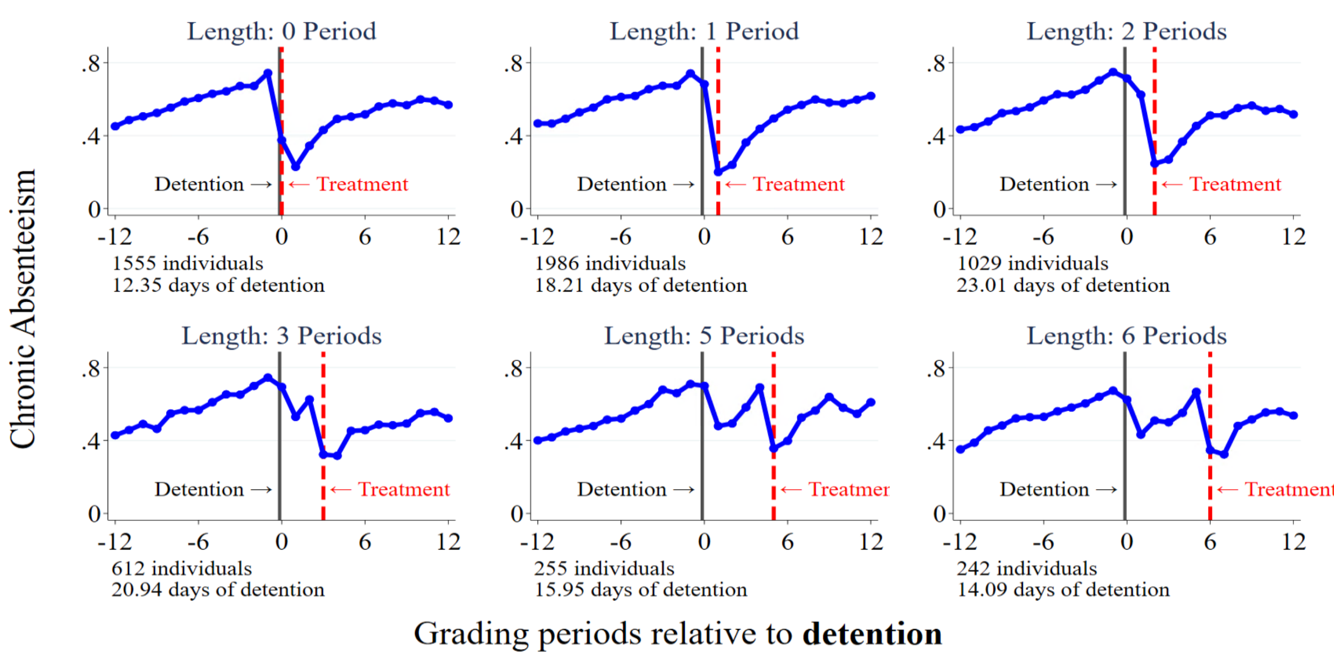
Notes: The figure compares raw data trends and regression results across individuals with substance-related discipline history and the full sample—individuals with and without such history.

Figure A8: Impacts of SUD Treatment Center School Attendance on Short-Run Outcomes: Post-Discharge Period



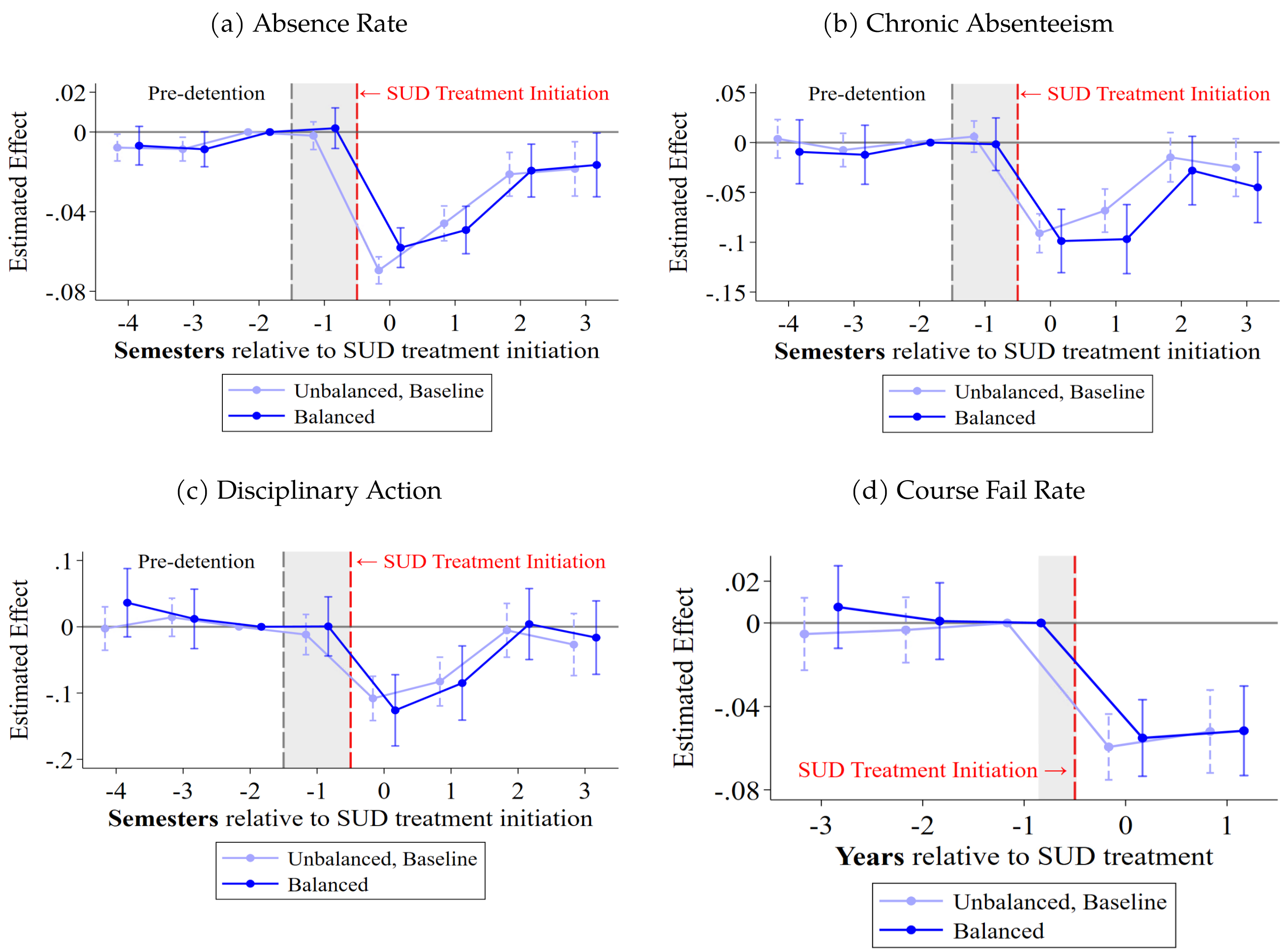
Notes: The figure presents the impact of SUD treatment center school attendance on short-run outcomes during the post-discharge period.

Figure A9: Raw Trends in Chronic Absenteeism by the Length of Intermediate Pre-Period



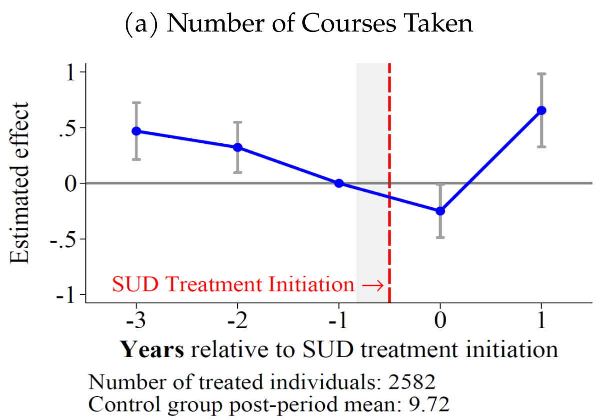
Notes: The figure plots raw trends in the likelihood of chronic absenteeism separately for six sub-groups that are defined based on the length of the intermediate pre-period.

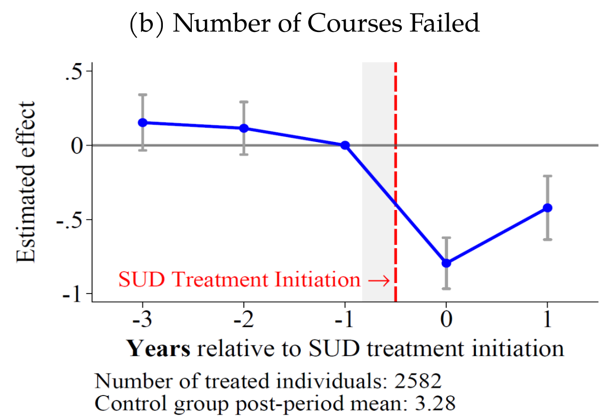
Figure A10: Short-Run Effects: Unbalanced and Balanced Sample



Notes: The figure shows the event study results using a balanced sample. I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2). Standard errors are clustered at the individual level.

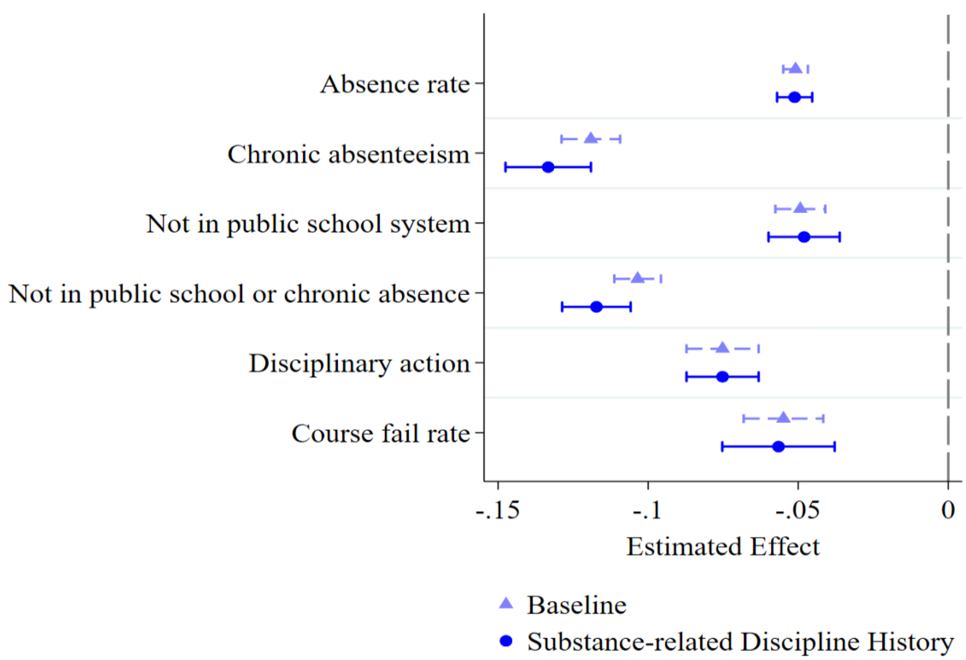
Figure A11: Impacts on Courses Taken and Failed





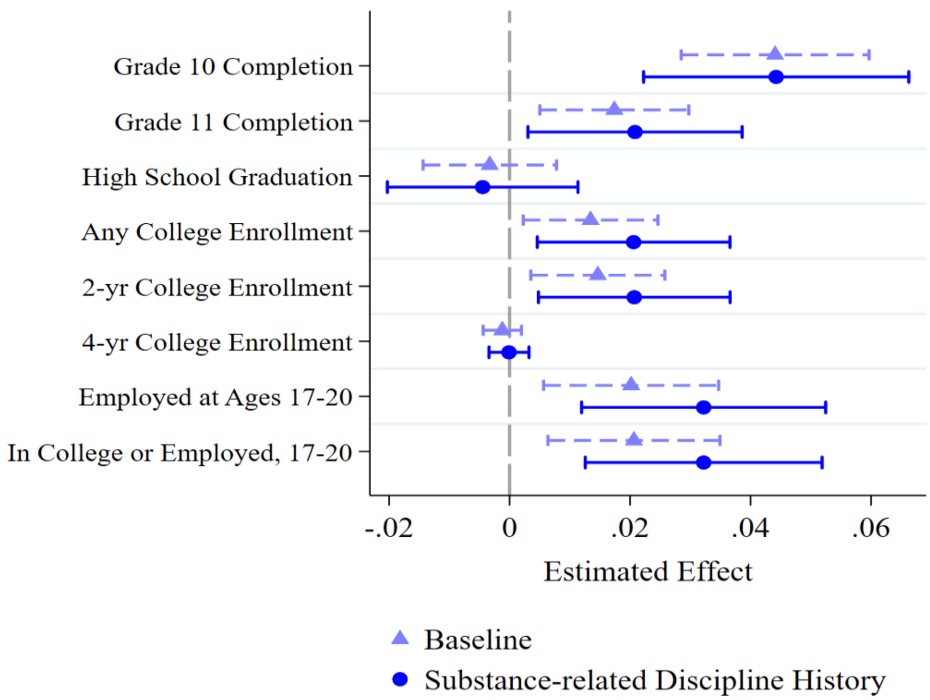
Notes: The figure shows the event study results using a balanced sample. I plot the coefficients and 95% confidence intervals on the interactions between the indicator for a treated individual and the indicators for the periods around the time of SUD treatment initiation from equation (2). Standard errors are clustered at the individual level.

Figure A12: Robustness of Short-Run Analysis Results: Restricting Sample to Adolescents Disciplined for Substance Use in Pre-Period



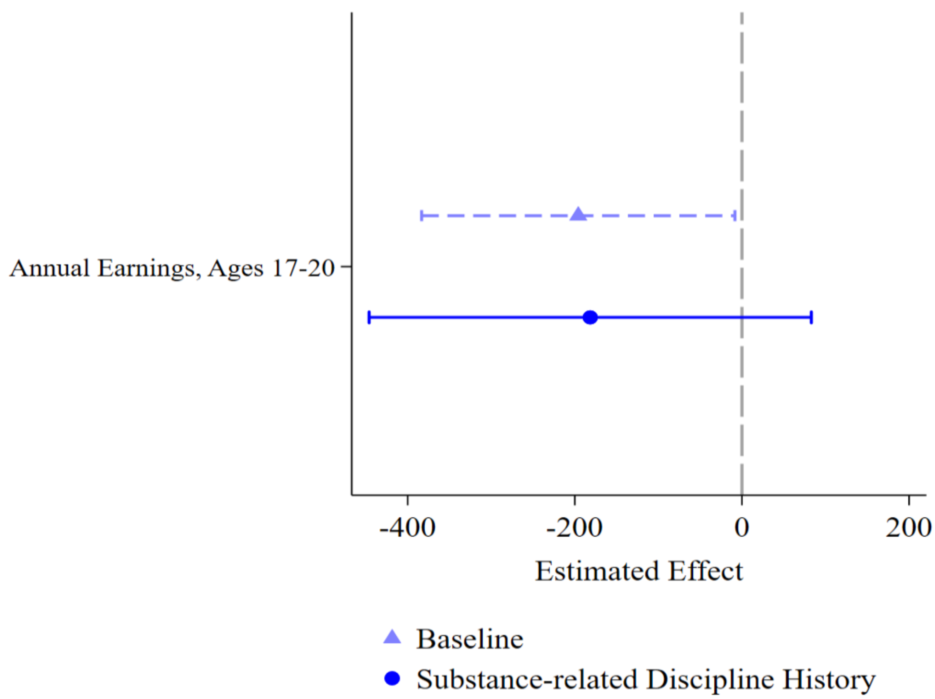
Notes: The figure shows the short-run analysis results obtained when I only include match groups where both the treated and matched control individuals were ever disciplined for substance-related reasons prior to detention.

Figure A13: Robustness of Long-Run Analysis Results: Restricting Sample to Adolescents Disciplined for Substance Use in Pre-Period



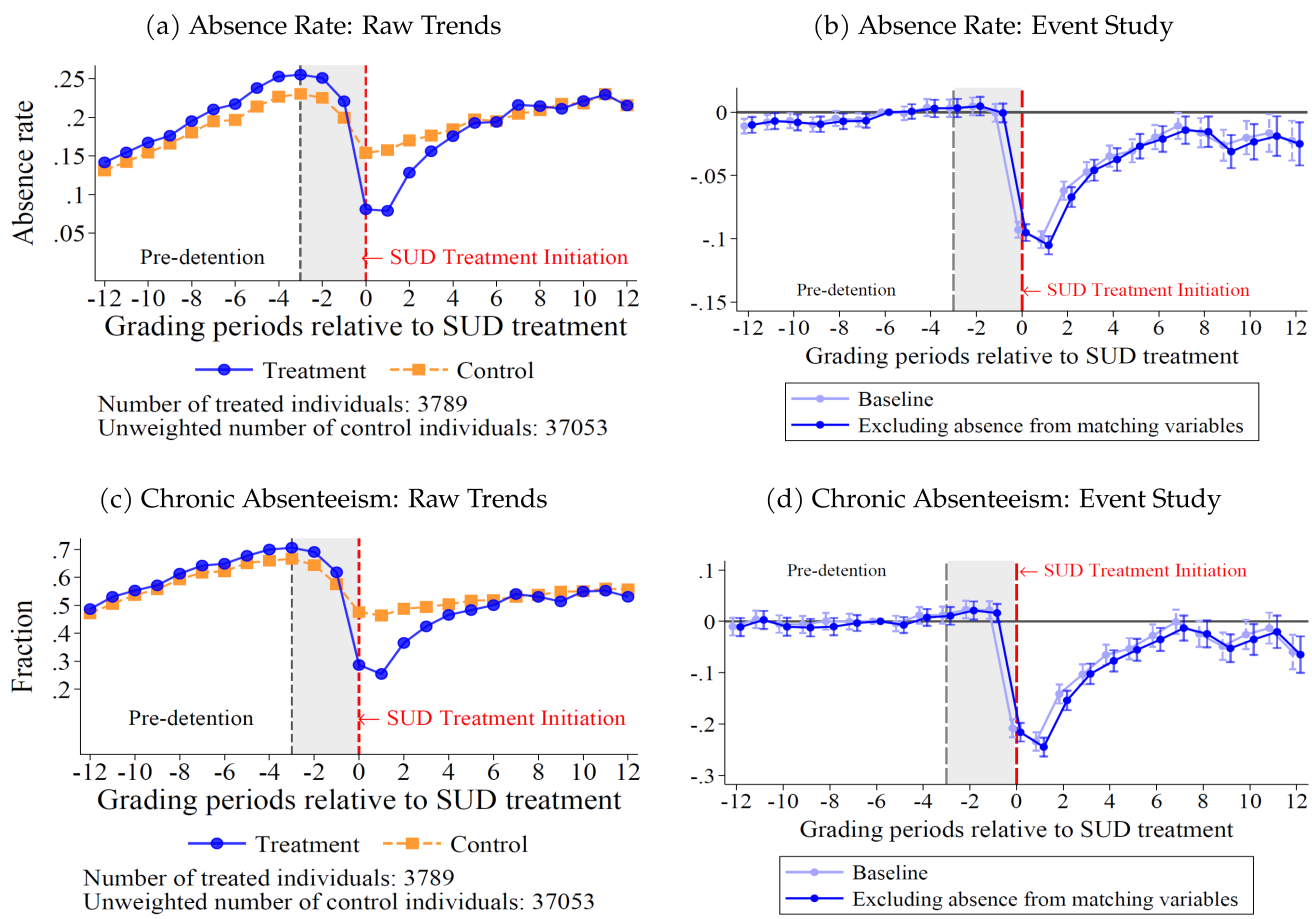
Notes: The figure shows the long-run analysis results obtained when I only include match groups where both the treated and matched control individuals were ever disciplined for substance-related reasons prior to detention.

Figure A14: Robustness of Long-Run Analysis Earning Results: Restricting Sample to Adolescents Disciplined for Substance Use in Pre-Period



Notes: The figure shows the long-run analysis results obtained when I only include match groups where both the treated and matched control individuals were ever disciplined for substance-related reasons prior to detention.

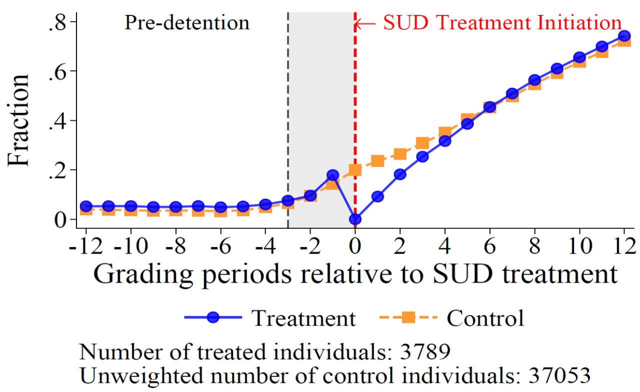
Figure A15: Robustness of Absence Results: Excluding Absence from the Matching



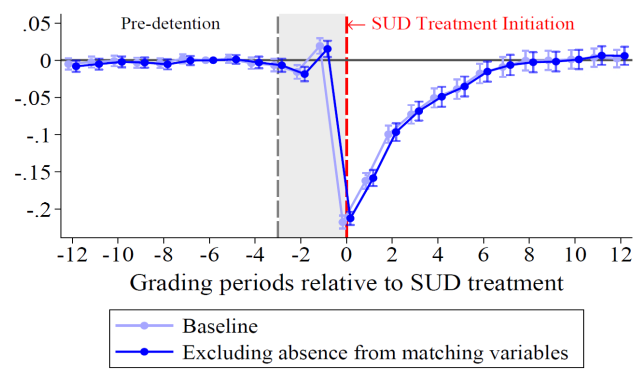
Notes: The figure shows raw data trends and the short-run analysis results obtained when I exclude absence from the fuzzy matching.

Figure A16: Robustness of Attrition Results: Excluding Absence from the Matching

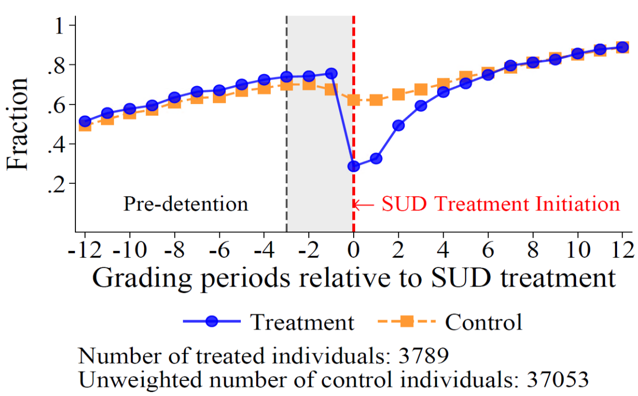
(a) Not in Public School System: Raw Trends



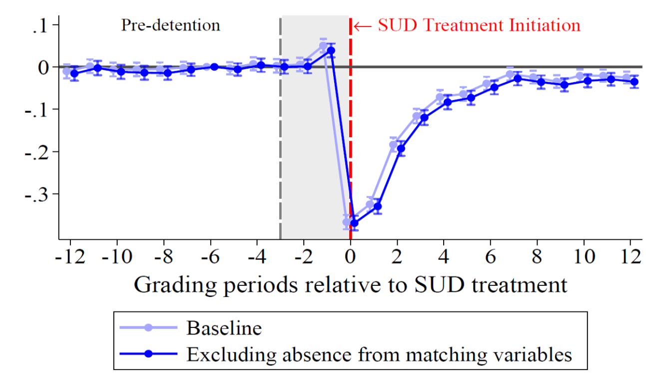
(b) Not in Public School System: Event Study



(c) Not in Public School Or Chronic Absenteeism: Raw Trends



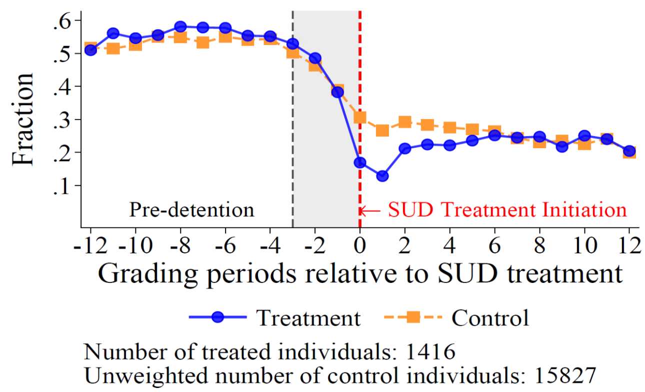
(d) Not in Public School Or Chronic Absenteeism: Event Study



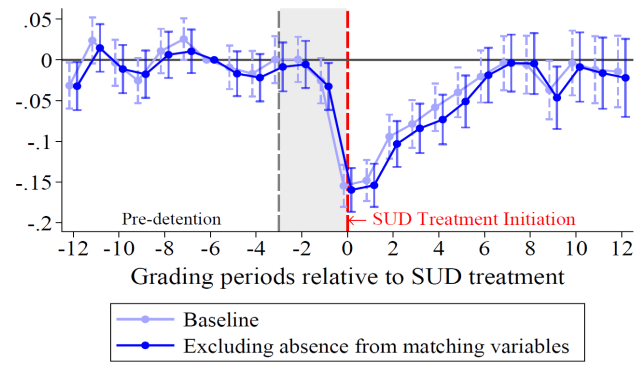
Notes: The figure shows raw data trends and the short-run analysis results obtained when I exclude absence from the fuzzy matching.

Figure A17: Robustness of Disciplinary Action Results: Excluding Absence from the Matching

(a) Any Disciplinary Action: Raw Trends



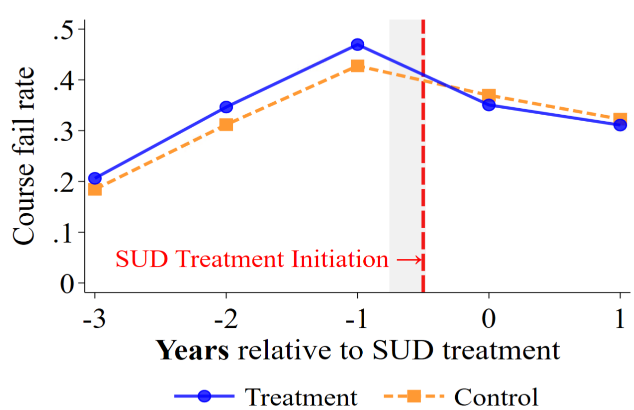
(b) Any Disciplinary Action: Event Study



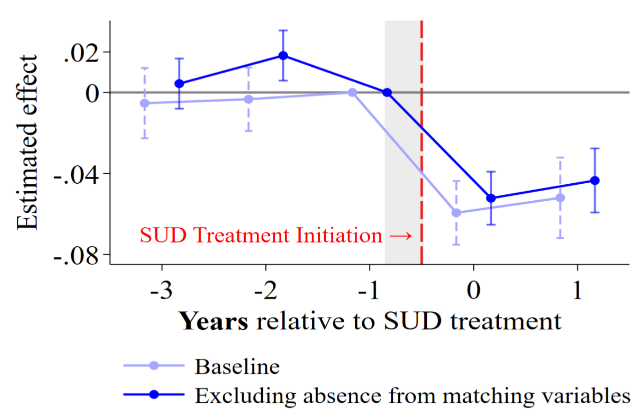
Notes: The figure shows raw data trends and the short-run analysis results obtained when I exclude absence from the fuzzy matching.

Figure A18: Robustness of Course Fail Rate Results: Excluding Absence from the Matching

(a) Course Fail Rate: Raw Trends

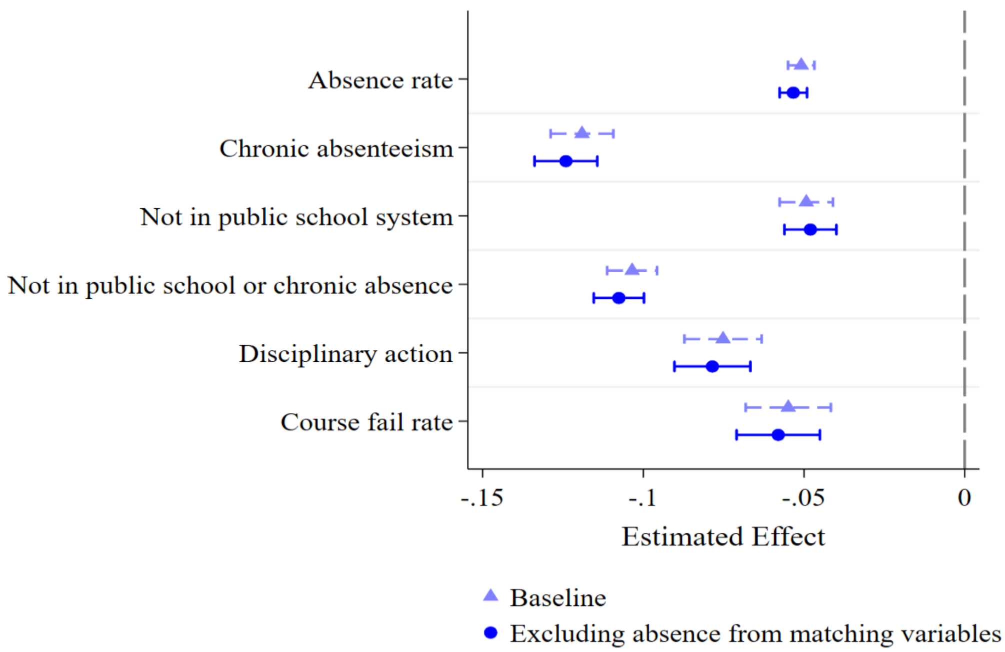


(b) Course Fail Rate: Event Study



Notes: The figure shows raw data trends and the short-run analysis results obtained when I exclude absence from the fuzzy matching.

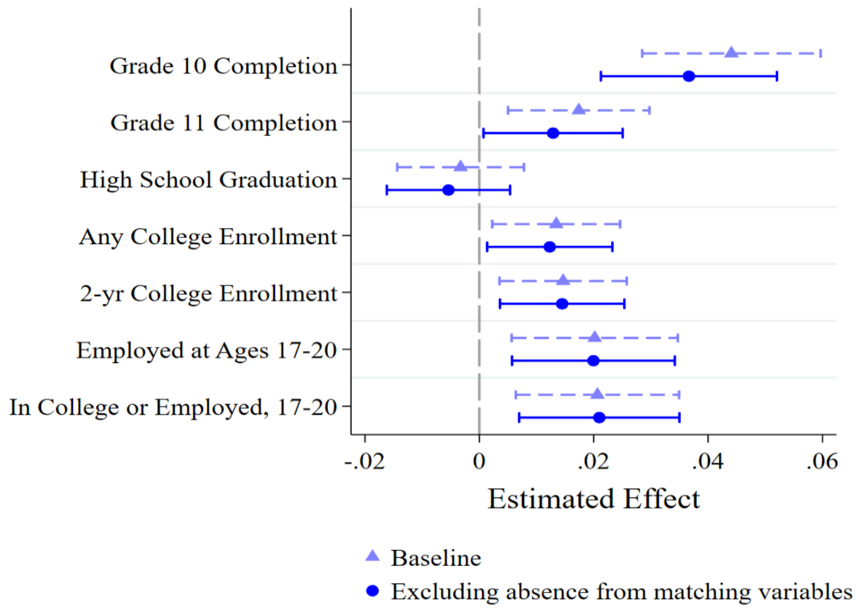
Figure A19: Robustness of of Short-Run Analysis Results: Excluding Absence from the Matching



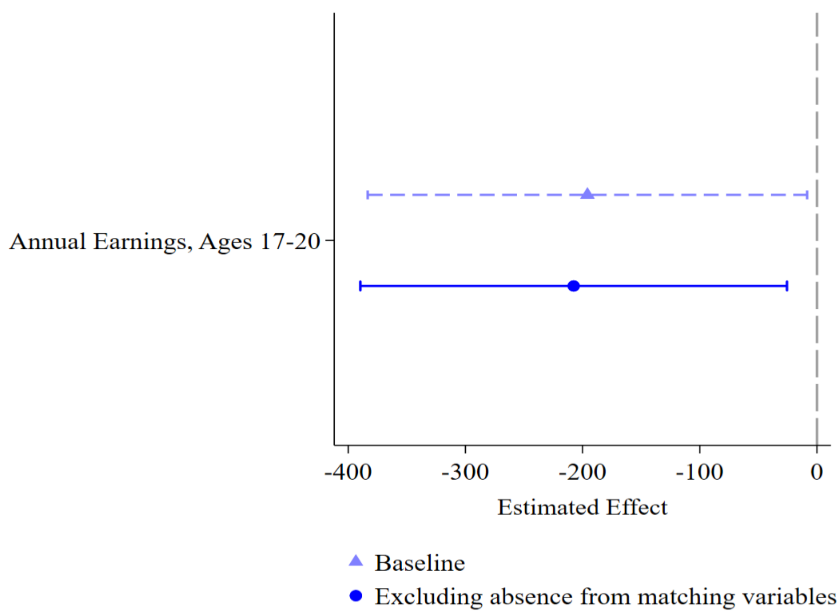
Notes: The figure shows the short-run analysis results obtained when I exclude absence from the fuzzy matching.

Figure A20: Robustness of Long-Run Analysis Results: Excluding Absence from the Matching

(a) Educational Outcomes and Employment

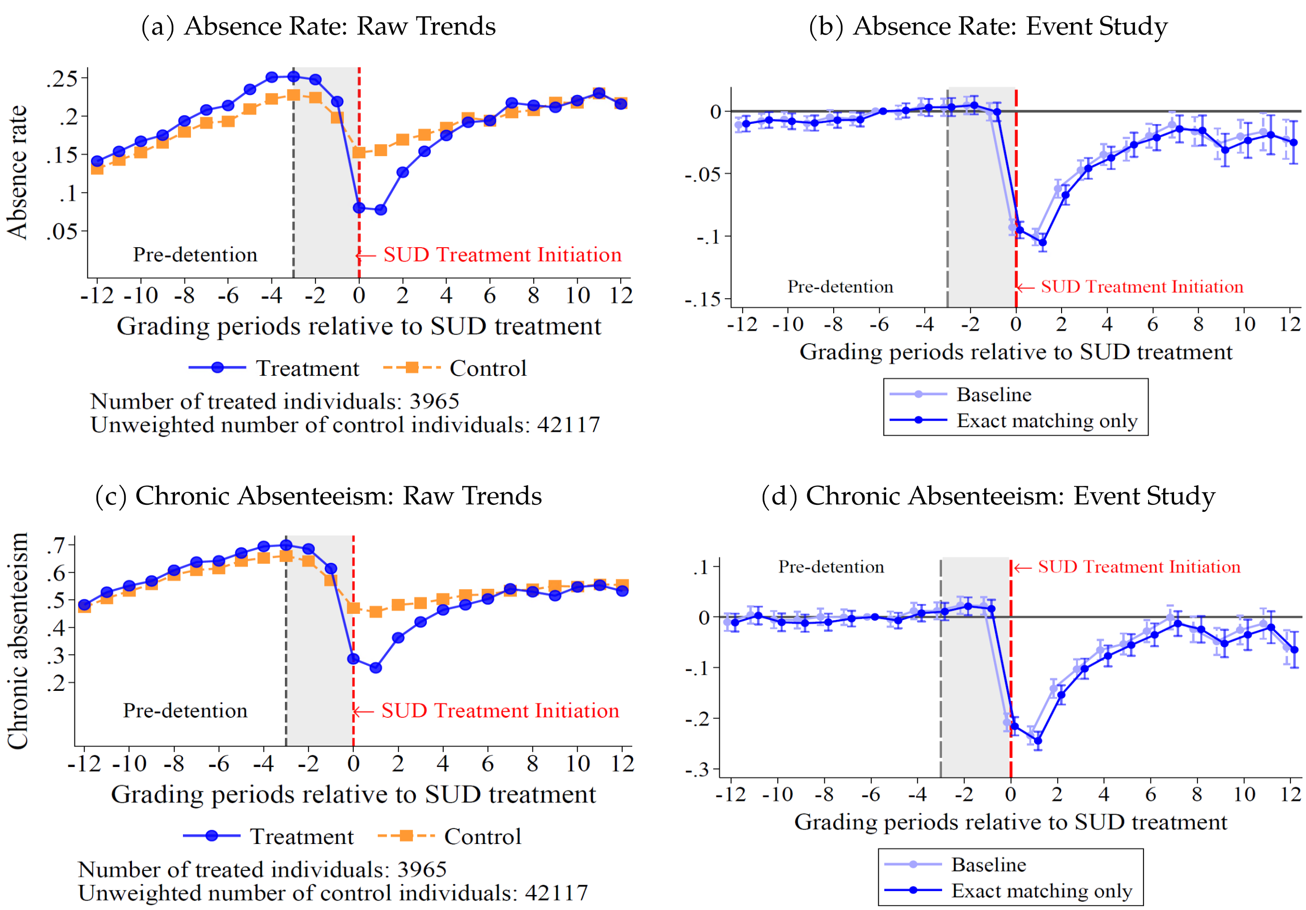


(b) Earnings



Notes: The figure shows the long-run analysis results obtained when I exclude absence from the fuzzy matching.

Figure A21: Robustness of Absence Results: Exact Matching Only



(a) Absence Rate: Raw Trends

(b) Absence Rate: Event Study

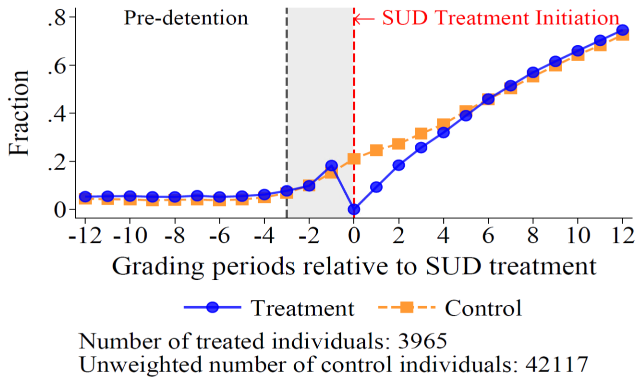
(c) Chronic Absenteeism: Raw Trends

(d) Chronic Absenteeism: Event Study

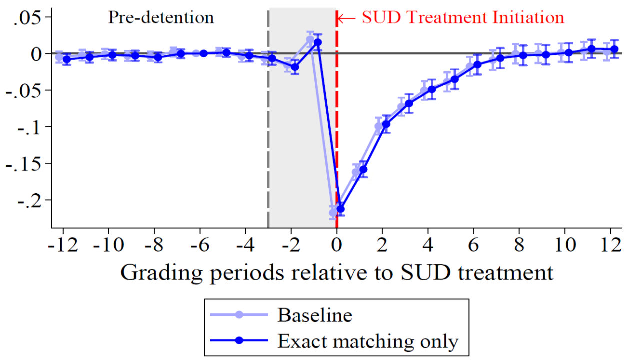
Notes: The figure shows raw data trends and the short-run analysis results obtained when I do the exact matching omitting the fuzzy matching.

Figure A22: Robustness of Attrition Results: Exact Matching Only

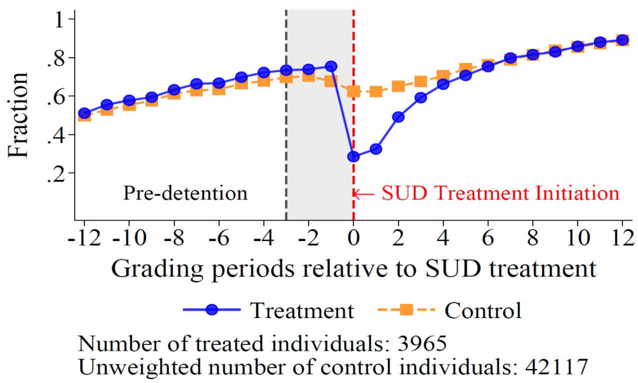
(a) Not in Public School System: Raw Trends



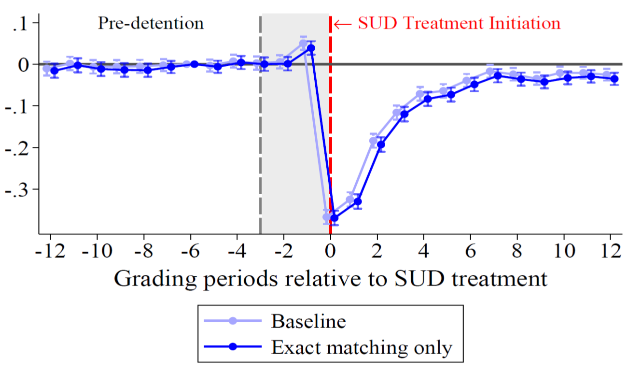
(b) Not in Public School System: Event Study



(c) Not in Public School Or Chronic Absenteeism: Raw Trends



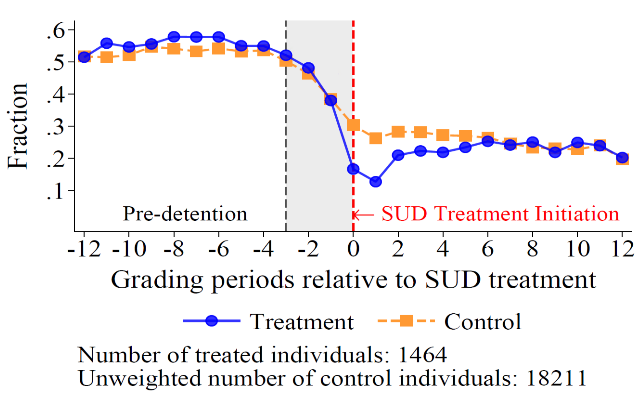
(d) Not in Public School Or Chronic Absenteeism: Event Study



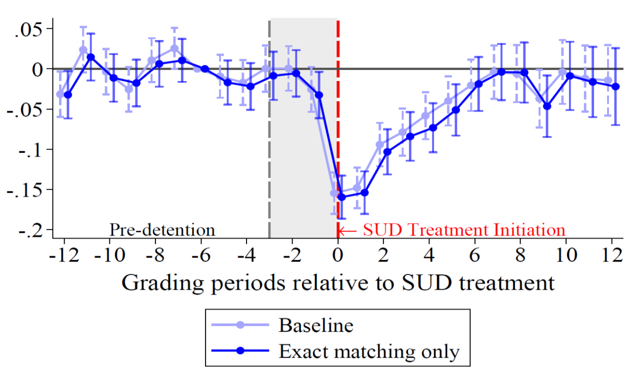
Notes: The figure shows raw data trends and the short-run analysis results obtained when I do the exact matching omitting the fuzzy matching.

Figure A23: Robustness of Disciplinary Action Results: Exact Matching Only

(a) Any Disciplinary Action: Raw Trends

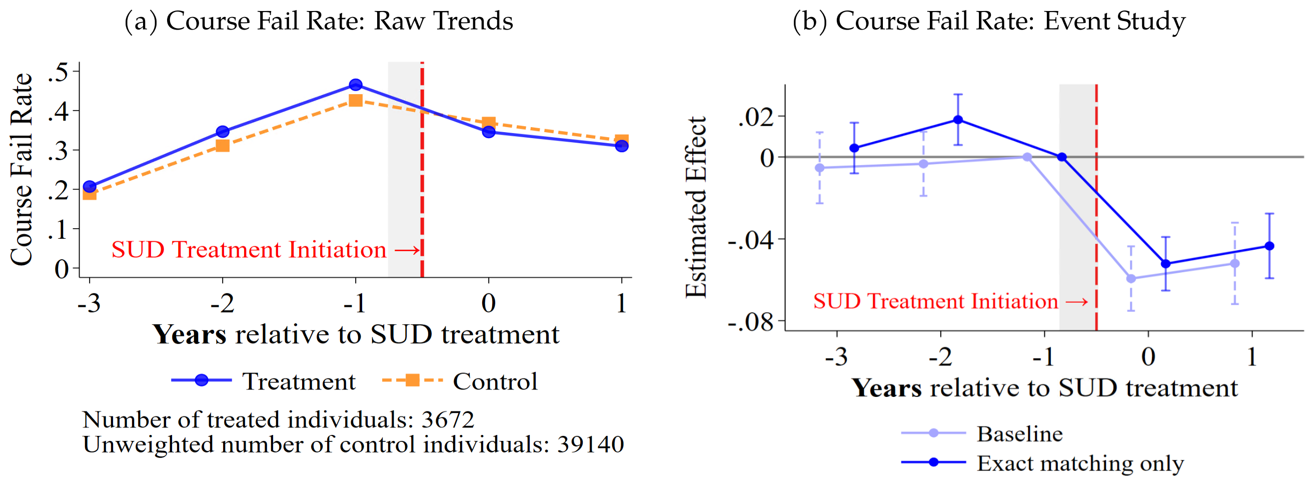


(b) Any Disciplinary Action: Event Study



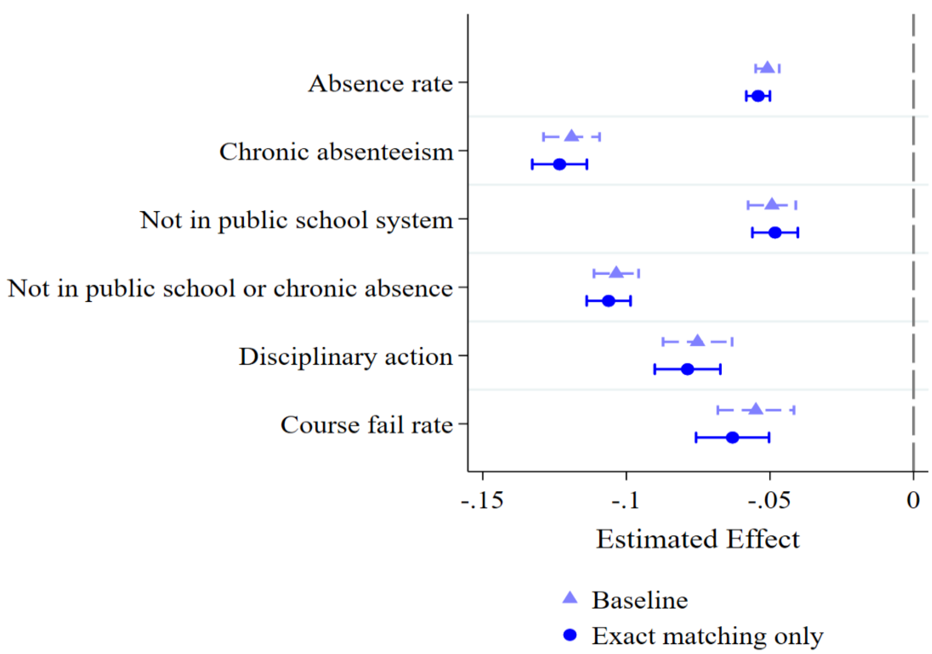
Notes: The figure shows raw data trends and the short-run analysis results obtained when I do the exact matching omitting the fuzzy matching.

Figure A24: Robustness of Course Fail Rate Results: Exact Matching Only



Notes: The figure shows raw data trends and the short-run analysis results obtained when I do the exact matching omitting the fuzzy matching.

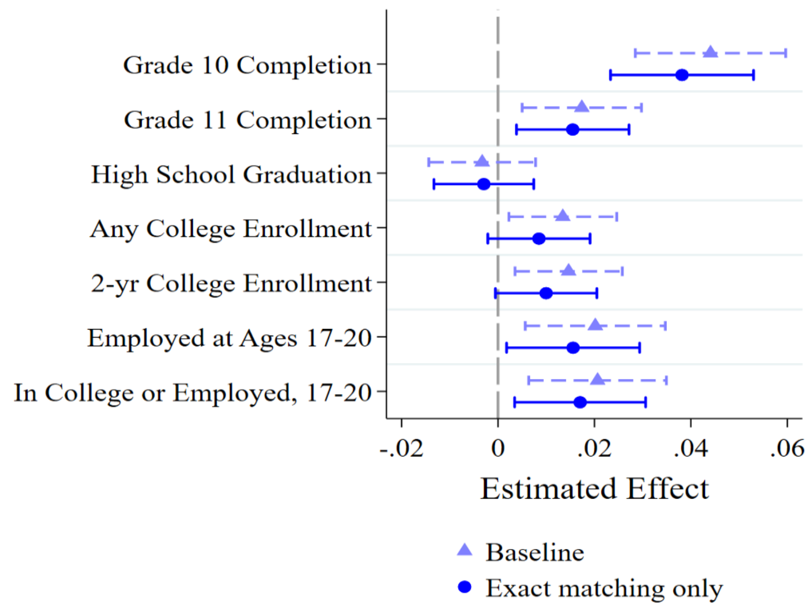
Figure A25: Robustness of of Short-Run Analysis Results: Exact Matching Only



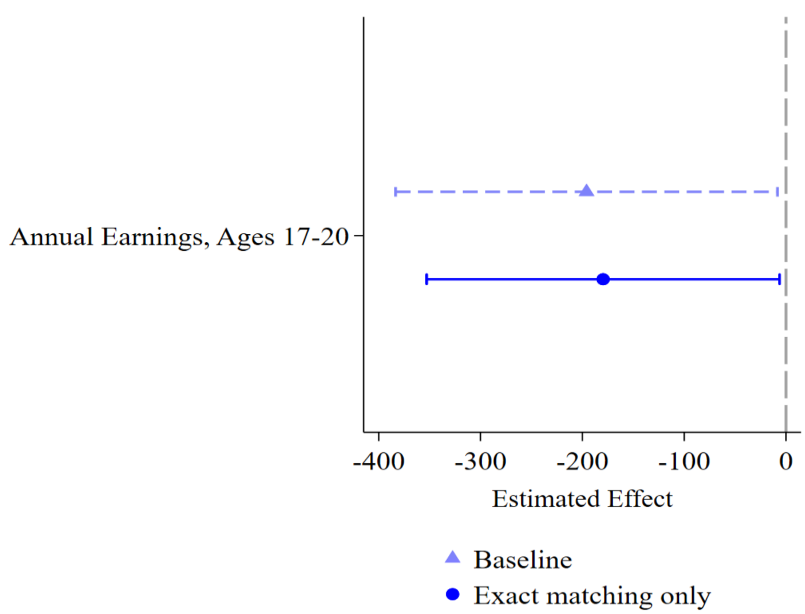
Notes: The figure shows the short-run analysis results obtained when I do the exact matching omitting the fuzzy matching.

Figure A26: Robustness of Long-Run Analysis Results: Exact Matching Only

(a) Educational Outcomes and Employment

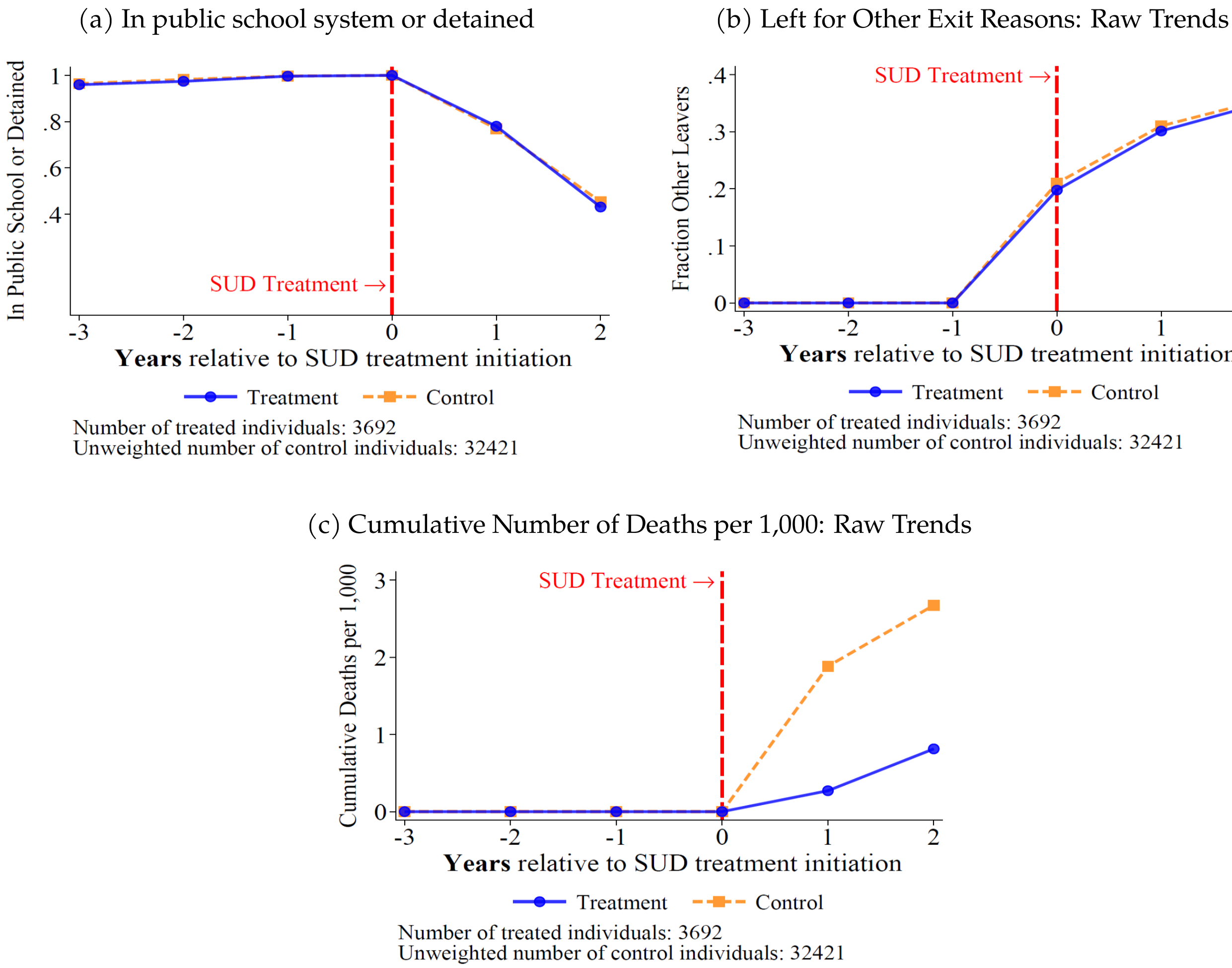


(b) Earnings



Notes: The figure shows the long-run analysis results obtained when I do the exact matching omitting the fuzzy matching.

Figure A27: Analysis for Other Exit Reasons: Raw Trends



Notes: The figure plots raw data trends in the outcomes from three years before to two years after SUD treatment, separately for treated and matched control individuals.

B Appendix Tables

Table A1: Average Individual Characteristics Across Treatment, Detainees with Substance-Related Discipline History, and All Detainees

	SUD Treatment School	Substance Disc. History	All Detainees	Diff	*p-val*	Diff	*p-val*
	(1)	(2)	(3)	(1) - (2)		(1) - (3)	
A. Individual Characteristics (Exact Matching Variables)							
Female	0.204	0.167	0.231	0.037	[<0.001]	-0.028	[<0.001]
Non-Hispanic white	0.285	0.192	0.226	0.093	[<0.001]	0.059	[<0.001]
Hispanic	0.544	0.612	0.486	-0.068	[<0.001]	0.057	[<0.001]
Non-Hispanic Black	0.161	0.187	0.278	-0.026	[<0.001]	-0.117	[<0.001]
Age at detention	14.869	14.860	14.611	0.010	[0.532]	0.258	[<0.001]
Economically disadvantaged	0.729	0.773	0.787	-0.043	[<0.001]	-0.057	[<0.001]
Special education	0.245	0.269	0.310	-0.023	[<0.001]	-0.064	[<0.001]
Urbanicity of county							
Large central metro	0.559	0.551	0.518	0.008	[0.248]	0.041	[<0.001]
Large fringe metro	0.172	0.177	0.194	-0.005	[0.396]	-0.021	[<0.001]
Medium metro	0.189	0.194	0.174	-0.006	[0.328]	0.014	[0.007]
Small metro	0.047	0.052	0.072	-0.005	[0.099]	-0.025	[<0.001]
Micropolitan	0.018	0.012	0.018	0.007	[<0.001]	0.000	[0.995]
Noncore	0.014	0.014	0.024	0.001	[0.691]	-0.009	[<0.001]
B. Mean Absence Rate and Detention History at Baseline (Fuzzy Matching Variables)							
Mean absence rate, 1 yr before	0.232	0.217	0.189	0.014	[<0.001]	0.043	[<0.001]
Share of periods detained, 1 yr before	0.108	0.097	0.079	0.010	[<0.001]	0.028	[<0.001]
Share of periods detained, 2 yr before	0.041	0.043	0.041	-0.002	[0.248]	0.000	[0.893]
C. Academic Performance at Baseline (Non-Matching Variables)							
Grade at the time of detention	9.067	9.038	8.846	0.029	[0.050]	0.221	[0.001]
Mean past course pass rate, above median	0.610	0.611	0.711	-0.002	[0.811]	-0.102	[<0.001]
Mean past reading z-scores, above median	0.562	0.555	0.609	0.007	[0.363]	-0.047	[<0.001]
Mean past math z-scores, above median	0.604	0.596	0.643	0.009	[0.235]	-0.039	[<0.001]
Individuals (total)	5,182	41,775	227,505				
Individuals (unique)	5,182	30,412	155,861				

Notes: The table presents average individual characteristics across (i) juvenile detainees who enter a SUD treatment center school after detention, (ii) juvenile detainees who never attended a SUD treatment center during my sample period (1998–2020) but who were disciplined for substance-related problems, and (iii) all juvenile detainees. All individuals were detained in a juvenile detention center at some point between ages 12–16 over the academic years 1999–2000 to 2017–2018.

Table A2: Average Individual Characteristics Across Baseline Treatment Sample, Treatment Individuals with No Qualified Matches, Treatment Individuals with No Exact Matches

	Baseline Treatment Sample	No Qualified Matches	No Exact Matches
	(1)	(2)	(3)
A. Individual Characteristics (Exact Matching Variables)			
Female	0.146	0.281	0.448
Non-Hispanic White	0.230	0.347	0.523
Hispanic	0.610	0.512	0.242
Non-Hispanic Black	0.157	0.130	0.190
Age at detention (gap ≤ 1)	14.841	15.042	14.947
Economically disadvantaged	0.752	0.648	0.562
Special education	0.207	0.305	0.381
Urbanicity of county			
Large central metro	0.619	0.477	0.309
Large fringe metro	0.163	0.196	0.210
Medium metro	0.183	0.239	0.197
Small metro	0.029	0.063	0.127
Micropolitan	0.004	0.007	0.090
Noncore	0.003	0.018	0.066
B. Mean Absence Rate and Detention History at Baseline (Fuzzy Matching Variables)			
Mean absence rate, 1 yr before	0.235	0.298	0.066
Share of periods detained, 1 yr before	0.098	0.255	0.103
Share of periods detained, 2 yr before	0.033	0.149	0.043
C. Academic Performance at Baseline (Non-Matching Variables)			
Grade at detention	9.023	9.049	9.280
Mean past course pass rate, above median	0.609	0.509	0.648
Mean past reading z-score, above median	0.553	0.544	0.608
Mean past math z-score, above median	0.597	0.579	0.644
Number of individuals	4,034	285	863

The table presents average individual characteristics across (i) the final treatment sample that is used in my baseline analyses, (ii) individuals who attended a SUD treatment center school but do not have qualified matches (i.e., exact matches with non-outlier distance values), and (iii) individuals who attended a SUD treatment center school but do not have exact matches (i.e., those who are dropped during the exact matching procedure).

Table A3: Summary Table, Other Exit Reasons

Percentage (%)	All (1)	Treat (2)	Control (3)	Diff (2) - (3)	*p-val*	Diff relative to control mean
Not Observed in "other exit reasons" data	64.591	64.870	64.306	0.583	[0.081]	0.91%
Observed in "other exit reasons" data	35.409	35.13	35.694			
1. Left Texas or died	**5.245**	**5.255**	**5.236**	**0.040**	**[0.842]**	**0.76%**
1-A. Enroll in school outside Texas	2.841	2.736	2.949	-0.178	[0.230]	-6.04%
1-B. Returned to home country	2.233	2.438	2.022	0.399	[0.003]	19.73%
1-C. Death	0.172	0.081	0.264	-0.181	[<0.001]	-68.56%
2. Alternative programs	**2.041**	**2.411**	**1.662**	**0.741**	**[<0.001]**	**44.58%**
2-A. Alternative programs toward GED/diploma	2.017	2.384	1.640	0.736	[<0.001]	44.88%
2-B. High School Equivalency certificate outside Texas	0.025	0.027	0.022	0.006	[0.693]	27.27%
3. Moved to other educational setting	**11.380**	**11.241**	**11.523**	**-0.317**	**[0.260]**	**-2.75%**
3-A. Home schooling	8.904	8.613	9.202	-0.607	[0.016]	-6.60%
3-B. Enroll in Texas private school	2.476	2.627	2.321	0.290	[0.038]	12.49%
4. Other reasons	**16.742**	**16.224**	**17.273**	**-1.047**	**[<0.001]**	**-6.06%**
4-A. Expelled for offense	0.251	0.217	0.287	-0.095	[0.033]	-33.10%
4-B. Removed—Child Protective Services	0.491	0.488	0.494	-0.004	[0.943]	-0.81%
Old reason codes (used between 1999–2007)						
4-C. Enroll in other Texas public school (not verified)	9.257	8.722	9.805	-1.044	[0.005]	-10.65%
4-D. Incarcerated in a facility outside the district	3.500	3.277	3.728	-0.443	[<0.001]	-11.88%
4-E. Other	3.243	3.521	2.958	0.540	[<0.001]	18.26%
Individuals (weighted)	7,291.3	3,692.0	3,599.3			
Individuals (total)	36,113	3,692	32,421			
Individuals (unique)	15,628	3,692	11,936			

Notes: The table presents the distribution of "other exit reasons"—reasons for leaving the Texas public school system other than dropout and graduation—across (i) the full sample, (ii) treatment individuals, and (iii) matched control individuals.

Table A4: Long-Run Effects of SUD Treatment on Earnings at Ages 17–20

	Earnings, Ages 17–20 (1)
Treatment Individual	-195.88
	(95.66)
	[0.041]
Control group outcome mean	3529.50
Effect size relative to control group mean	-5.55%
Treatment individuals	3,160
Control individuals (weighted)	3,186.4
Control individuals (total)	28,161
Control individuals (unique)	10,610
Observations	31,321
R-squared	0.361

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (3). Standard errors are clustered at the individual level.